
EX LIBRIS
G Brown Goode
AIX 1634
BARBADOES
NEW ENGLAND
NEW JERSEY
VIRGINIA
OHIO
Ffor hym was lefere to have at hese bedis hed
Twenty bokis I-clad in blak or red
Of Aristotle & his philoso-phie
Than robis ryche or fedele or gay sautrie
S. I.
U.S. National Museum
S. I.

NATURE

A WEEKLY

ILLUSTRATED JOURNAL OF SCIENCE

VOLUME V.

NOVEMBER 1871 to APRIL 1872

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

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NATURE

A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE

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

THURSDAY, NOVEMBER 2, 1871

*RIPPLES AND WAVES**

YOU have always considered cohesion of water (capillary attraction) as a force which would seriously disturb such experiments as you were making, if on too small a scale. Part of its effect would be to modify the waves generated by towing your models through the water. I have often had in my mind the question of waves as affected by gravity and cohesion jointly, but have only been led to bring it to an issue by a curious phenomenon which we noticed at the surface of the water round a fishing-line one day slipping out of Oban (becalmed) at about half a mile an hour through the water. The speed was so small that the lead kept the line almost vertically downwards; so that the experimental arrangement was merely a thin straight rod held nearly vertical, and moved through smooth water at speeds from about a quarter to three-quarters of a mile per hour I tried boat-hooks, oars, and other forms of moving solids, but they seemed to give, none of them, so good a result as the fishing-line. The small diameter of the fishing-line seemed to favour the result, and I do not think its roughness interfered much with it. I shall, however, take another opportunity of trying a smooth round rod like a pencil, kept vertical by a lead weight hanging down under water from one end, while it is held up by the other end. The fishing-line, however, without any other appliance proved amply sufficient to give very good results.

What we first noticed was an extremely fine and numerous set of short waves preceding the solid much longer waves following it right in the rear, and oblique waves streaming off in the usual manner at a definite angle on each side, into which the waves in front and the waves in the rear merged so as to form a beautiful and symmetrical pattern, the tactics of which I have not been able thoroughly to follow hitherto. The diameter of the "solid" (that is to say the fishing-line) being only two or three millimetres and the longest of the observed waves five or six centimetres, it is clear that the waves at distances in any directions from the solid exceeding fifteen or twenty centimetres, were sensibly unforced (that is to say moving each as if it were part of an endless series of uniform parallel waves undisturbed by any solid). Hence the waves seen right in front and right in rear showed (what became immediately an obvious result of theory) two different wave-lengths with the same velocity of propagation. The speed of the vessel falling off, the waves in rear of the fishing-line became shorter and those in advance longer, showing another obvious result of theory. The speed further diminishing, one set of waves shorten and the other lengthen, until they become, as nearly as I can distinguish, of the same lengths, and the oblique lines of waves in the intervening pattern open out to an obtuse angle of nearly two right angles. For a very short time a set of parallel waves some before and some behind the fishing-line, and all advancing direct with the same velocity, were seen. The speed further diminishing the pattern of waves disappeared altogether. Then slight tremors of the fishing-line (produced for example by striking it above water) caused circular rings of waves to diverge in all directions, those in front advancing at a greater speed relatively to the water than that of the fishing-line. All these phenomena illustrated very remarkably a geometry of ripples communicated a good many years ago to the *Philosophical Magazine* by Hirst, in which, however, so far as I can recollect, the dynamics of the subject were not discussed. The speed of the solid which gives the uniform system of parallel waves before and behind it, was clearly an absolute minimum wave-velocity, being the limiting velocity to which the common velocity of the larger waves in rear and shorter waves in front was reduced by shortening the former and lengthening the latter to an equality of wave-length.

Taking ·074 of a gramme weight per centimetre of breadth for the cohesive tension of a water surface (calculated from experiments by Gay Lussac, contained in Poisson's theory of capillary attraction, for pure water at a temperature, so far as I recollect, of about 9° Cent.), and one gramme as the mass of a cubic centimetre, I find, for the minimum velocity of propagation of surface waves, 23 centimetres per second.* The mini-

* Extract from a letter to Mr. W. Froude, by Sir W. Thomson.

* One nautical mile per hour, the only other measurement of velocity, except the French metrical reckoning, which ought to be used in any practical measurement, is 51·6 centimetres per second.

mum wave velocity for sea-water may be expected to be not very different from this. (It would of course be the same if the cohesive tension of sea water were greater than that of pure water in precisely the same ratio as the density.)

About three weeks later, being becalmed in the Sound of Mull, I had an excellent opportunity, with the assistance of Prof. Helmholtz, and my brother from Belfast, of determining by observation the minimum wave velocity with some approach to accuracy. The fishing-line was hung at a distance of two or three feet from the vessel's side, so as to cut the water at a point not sensibly disturbed by the motion of the vessel. The speed was determined by throwing into the sea pieces of paper previously wetted, and observing their times of transit across parallel planes, at a distance of 912 centimetres asunder, fixed relatively to the vessel by marks on the deck and gunwale. By watching carefully the pattern of ripples and waves, which connected the ripples in front with the waves in rear, I had seen that it included a set of parallel waves slanting off obliquely on each side, and presenting appearances which proved them to be waves of the critical length and corresponding minimum speed of propagation. Hence the component velocity of the fishing-line perpendicular to the fronts of these waves was the true minimum velocity. To measure it, therefore, all that was necessary was to measure the angle between the two sets of parallel lines of ridges and hollows, sloping away on the two sides of the wake, and at the same time to measure the velocity with which the fishing-line was dragged through the water. The angle was measured by holding a jointed two-foot rule, with its two branches, as nearly as could be judged, by the eye, parallel to the sets of lines of wave-ridges. The angle to which the ruler had to be opened in this adjustment was the angle sought. By laying it down on paper, drawing two straight lines by its two edges, and completing a simple geometrical construction with a length properly introduced to represent the measured velocity of the moving solid, the required minimum wave-velocity was readily obtained. Six observations of this kind were made, of which two were rejected as not satisfactory. The following are the results of the other four :—

Velocity of Moving Solid.	Deduced Minimum Wave-Velocity.
51 centimetres per second.	23·0 centimetres per second.
38 ,, ,,	23·8 ,, ,,
26 ,, ,,	23·2 ,, ,,
24 ,, ,,	22·9 ,, ,,
	Mean 23·22

The extreme closeness of this result to the theoretical estimate (23 centimetres per second) was, of course, merely a coincidence, but it proved that the cohesive force of sea-water at the temperature (not noted) of the observation cannot be very different from that which I had estimated from Gay Lussac's observations for pure water.

I need not trouble you with the theoretical formulæ just now, as they are given in a paper which I have communicated to the Royal Society of Edinburgh, and which will probably appear soon in the *Philosophical Magazine*. If 23 centimetres per second be taken as the minimum speed they give 1·7 centimetres for the corresponding wave-length.

I propose, if you approve, to call ripples, waves of lengths less than this critical value, and generally to restrict the name waves to waves of lengths exceeding it. If this distinction is adopted, ripples will be undulations such that the shorter the length from crest to crest the greater the velocity of propagation; while for waves the greater the length the greater the velocity of propagation. The motive force of ripples is chiefly cohesion; that of waves chiefly gravity. In ripples of lengths less than half a centimetre the influence of gravity is scarcely sensible; cohesion is nearly paramount. Thus the motive of ripples is the same as that of the trembling of a dew drop and of the spherical tendency of a drop of rain or spherule of mist. In all waves of lengths exceeding five or six centimetres, the effect of cohesion is practically insensible, and the moving force may be regarded as wholly gravity. This seems amply to confirm the choice you have made of dimensions in your models, so far as concerns escaping disturbances due to cohesion.

The introduction of cohesion into the theory of waves explains a difficulty which has often been felt in considering the patterns of standing ripples seen on the surface of water in a finger-glass made to sound by rubbing a moist finger on its lip. If no other levelling force than gravity were concerned, the length from crest to crest corresponding to 256 vibrations per second would be a fortieth of a millimetre. The ripples would be quite undistinguishable without the aid of a microscope, and the disturbance of the surface could only be perceived as a dimming of the reflections seen from it. But taking cohesion into account, I find the length from crest to crest corresponding to the period of $\frac{1}{256}$ of a second to be 1·9 millimetres, a length which quite corresponds to ordinary experience on the subject.

When gravity is neglected the formula for the period (P) in terms of the wave-length (l), the cohesive tension of the surface (T), and the density of the fluid (ρ), is

$$P = \sqrt{\frac{l^3 \rho}{2 \pi T}},$$

where T must be measured in kinetic units. For water we have $\rho = 1$, and (according to the estimate I have taken from Poisson and Gay Lussac) $T = 982^* \times \cdot 074 = 73$. Hence for water

$$P = \frac{l^{\frac{3}{2}}}{\sqrt{2 \pi \times 73}} = \frac{l^{\frac{3}{2}}}{21 \cdot 4}$$

When l is anything less than half a centimetre the error from thus neglecting gravity is less than 5 per cent. of P. When l exceeds $5\frac{1}{2}$ centimetres the error from neglecting cohesion is less than five per cent. of the period. It is to be remarked that, while for waves of sufficient length to be insensible to cohesion, the period is proportional to the square-root of the length, for ripples short enough to be insensible to gravity, the period varies in the sesquiplicate ratio of the length.

WILLIAM THOMSON

Mr. Froude having called my attention to Mr. Scott Russell's Report on Waves (British Association, York, 1844) as containing observations on some of the phenomena which formed the subject of the preceding letter to him, I find in it, under the heading "Waves of the Third Order," or, "Capillary Waves," a most interesting account of the

* 982 being the weight of one gramme in kinetic units of force-centimetres per second.

"ripples" (as I have called them), seen in advance of a body moving uniformly through water; also a passage quoted by Russell from a paper of date, Nov. 16, 1829, by Poncelet and Lesbros,* where it seems this class of waves was first described.

Poncelet and Lesbros, after premising that the phenomenon is seen when the extremity of a fine rod or bar is lightly dipped in a flowing stream, give a description of the curved series of ripples (which first attracted my attention in the manner described in the preceding letter). Russell's quotation concludes with a statement from which I extract the following:— . . . . "on trouve que les rides sont imperceptibles quand la vîtesse est moyennement au dessous de 25c. per seconde."

Russell gives a diagram to illustrate this law. So far as I can see, the comparatively long waves following in rear of the moving body have not been described either by Poncelet and Lesbros or by Russell, nor are they shown in the plan contained in Russell's diagram. But the curve shown above the plan (obviously intended to represent the section of the water-surface by a vertical plane) gives these waves in the rear as well as the ripples in front, and proves that they had not escaped the attention of that very acute and careful observer. In respect to the curves of the ripple-ridges, Russell describes them as having the appearance of a group of confocal hyperbolas, which seems a more correct description than that of Poncelet and Lesbros, according to which they present the aspect of a series of parabolic curves. It is clear, however, from my dynamical theory that they cannot be accurate hyperbolas; and, as far as I am yet able to judge, Russell's diagram exhibiting them is a very good representation of their forms. Anticipating me in the geometrical determination of a limiting velocity, by observing the angle between the oblique terminal straight ridge-lines streaming out on the two sides, Russell estimates it at $8\frac{1}{2}$ inches ($21\frac{1}{2}$ centimetres) per second.

Poncelet and Lesbros's estimate of 25 centimetres per second for the smallest velocity of solid relatively to fluid which gives ripples in front, and Russell's terminal velocity of $21\frac{1}{2}$ centimetres per second, are in remarkable harmony with my theory and observation which give 23 centimetres per second as the minimum velocity of propagation of wave or ripple in water.

Russell calls the ripples in front "forced," and the oblique straight waves streaming off at the sides "free"—appellations which might seem at first sight to be in thorough accordance with the facts of observation, as, for instance, the following very important observation of his own:—

"It is perhaps of importance to state that when, while these forced waves were being generated, I have suddenly withdrawn the disturbing point, the first wave immediately sprang back from the others (showing that it had been in a state of compression), and the ridges became parallel; and, moving on at the rate of $8\frac{1}{2}$ inches per second, disappeared in about 12 seconds."

Nevertheless I maintain that the ripples of the various degrees of fineness seen in the different† parts of the fringe are all properly "free" waves, because it follows from dynamical theory that the motion of every portion of fluid in a wave, and, therefore, of course, the velocity of propagation, is approximately the same as if it were part of an infinite series of straight-ridged parallel waves, provided that in the actual wave the radius of curvature of the ridge is a large multiple of the wave-length, and that there are several approximately equal waves preceding it and following it.

* Memoirs of the French Institute, 1829.

† The dynamical theory shows that the length from crest to crest depends on the corresponding component of the solid's velocity. For very fine ripples it is approximately proportional to the reciprocal of the square of this component velocity, and therefore to the square of the secant of the angle between the line of the solid's motion and the horizontal line perpendicular to the ridge of the ripple.

No indication of the dynamical theory contained in my communication to the *Philosophical Magazine*, and described in the preceding letter to Mr. Froude, appears either in the quotation from Poncelet and Lesbros, or in any other part of Mr. Scott Russell's report; but I find with pleasure my observation of a minimum velocity below which a body moving through water gives no ripples, anticipated and confirmed by Poncelet and Lesbros, and my experimental determination of the velocity of the oblique straight-ridged undulations limiting the series of ripples, anticipated and confirmed by Russell. W. T.

ALLBUTT ON THE OPHTHALMOSCOPE

On the Use of the Ophthalmoscope in Diseases of the Nervous System and of the Kidneys; also in certain other General Disorders. By Thomas Clifford Allbutt, M.A., M.D., Cantab. &c. (London and New York: Macmillan and Co., 1871.)

THE advances that have been made in the knowledge of the diseases of the eye since the introduction of the ophthalmoscope are now very widely known, not alone in the medical profession but to the general public. This little instrument, essentially consisting of a mirror with a hole in the centre by which a ray of light can be thrown into the interior of the eye, lighting up its recesses, and enabling, with the aid of a common hand lens, almost every portion of it to be explored, may be said to have revolutionised the surgery of the eye. Many separate and distinct types of disease have been distinguished in conditions that were formerly grouped together under the general term of amaurosis, and the ophthalmic surgeon, no longer administering, as was too often formerly the case, his remedies in rash ignorance, is now able either to infuse well-grounded hope of recovery, or to spare his patient the annoyance of protracted treatment when treatment would be hopeless. For nearly twenty years the use of the ophthalmoscope has been, as was natural, almost entirely restricted to those who devoted themselves to the study of ophthalmic diseases. Like other mechanical aids to diagnosis, as the stethoscope and laryngoscope, its employment requires practice, the opportunities for acquiring a mastery over it were till recently rare, and its value in the practice of medicine was by no means generally recognised. Within the last few years, however, several excellent surgeons and physicians, amongst whom Mr. Hutchinson, Dr. Hughlings Jackson, Dr. John Ogle, and the author of the treatise before us may be especially mentioned, have gradually begun to recognise that the ophthalmoscope may be made available not only to determine the nature of any defect of vision of which the patient may complain, but as a means of reading within certain limits changes in the conditions of the system at large, and of the nervous system in particular.

The work of Dr. Allbutt is, however, the first treatise in English that is occupied exclusively with the ophthalmoscopic appearances presented in cases of cerebral disease, or in other words with the diagnosis of nervous affections by the ophthalmoscope. Abroad he has been preceded by M. Bouchut, whilst the volumes of the "Archiv für Ophthalmologie" are a mine of original memoirs written by the best ophthalmologists in Germany on the bearings of ophthalmoscopic observations on nervous affections. To these, of course, Dr. Allbutt makes frequent reference. In no instance, however, have we noticed a servile adherence to the opinions of others, the statements he quotes being always checked by his own observations, and every page bearing the stamp of very careful and sound investigation. It is impossible with the limited space here at disposal, and it would perhaps scarcely be interesting to many of our readers, to give what the work really deserves, a *résumé* and discussion of its successive chapters; but we may here perhaps point out one or two of the principal points of interest.

In speaking of the disc of the optic nerve, Dr. Allbutt expresses himself in favour of the view of Galezowski, who is fortified by the observations of Leber, to the effect that the vascularity of the disc is to a great extent independent of that of the retina, and rather forms a part of the vascular system of the brain. The importance of this principle in enabling deductions to be drawn respecting the occurrence of intercranial disease is obvious. Proceeding on this hypothesis, Dr. Allbutt points out the changes that are visible in a large number of different affections. He draws a strong line of distinction between ischæmia and optic neuritis, conditions that have hitherto been almost invariably confounded by ophthalmic surgeons, but of which the former is produced by some cause, often of a mechanical nature, interfering with the return of the blood from the retina, whilst the latter is a true inflammation of the nerve. The diagnosis of the two in their earlier stages is very clearly and correctly laid down. At a later period both conditions pass into white atrophy, and it is not always then easy to pronounce which of the two has previously been present. His views, in regard to changes in the optic disc from intercranial disease, are clearly laid down in the following passage (pp. 129, 130):—"We find optic changes in connection with two kinds of intracranial disease in particular; the one tumour, the other meningitis. When we analyse the matter one degree further, we ascertain that, although the choked disc (ischæmia) and the inflamed nerve may co-exist with either of these kinds of disease, that nevertheless the choked disc is far more commonly found in association with tumour and hydrocephalus than the inflamed nerve. The inflamed nerve, on the other hand, is very commonly found in association with meningitis, and of meningitis not of the surface, nor of parts near any supposed vasomotor centres, but with meningitis near the centre." And with this we are disposed substantially to agree. Dr. Allbutt expresses himself in very doubtful terms in regard to the existence of tobacco amaurosis, and it certainly is extraordinary that, if really constituting an effect of the use of that leaf, it is not of more frequent occurrence amongst the Germans and Americans, who are much larger consumers than either the French or ourselves.

Our readers will see that Dr. Allbutt has, if not exactly opened up, at all events vigorously worked at, a new field of medical investigation. This field promises when duly cultivated to yield very valuable fruit; and, we are sure, the conclusion at which every candid reader will arrive, after carefully perusing it, will be that no physician should consider he has fully examined any case of cerebral disease unless he has accurately investigated the appearances presented by the eye under the ophthalmoscope. It is not to be supposed that Dr. Allbutt has by any means exhausted the subject. Many difficulties lie in the path of the most diligent inquirer. In many instances conditions of disease are seen to be present, as to the nature of which only a guess can be formed, and respecting which from forgetfulness or lack of observation on the part of the patient no history can be obtained; whilst in a multitude of cases the disease is seen only at one stage of its progress, and the physician is unable to ascertain, owing to his losing sight of his patient, the ulterior changes that take place.

Lastly, in many cases the prejudice of friends (a point to be greatly regretted) prevents the examination of the eyes after death. The fragmentary character of many of the reports of cases collected by Dr. Allbutt in his appendix is painfully evident, and leaves many hiatuses to be filled up by future research. We may, however, in conclusion, thank Dr. Allbutt for having published a work which constitutes an important step in the advancement of medicine, and will certainly form a very valuable guide to the profession at large, nor may we omit to thank the publishers for the excellent manner in which the book has been issued from the press. H. POWER

OUR BOOK SHELF

Hardy Flowers: Descriptions of upwards of thirteen hundred of the most ornamental species, and directions for their arrangement, culture, &c. By W. Robinson, F.L.S. (London: F. Warne and Co., 1871.)

MR. ROBINSON is a prolific writer, but his prolificacy (as Webster has it, if Dr. Ingleby and Dr. Latham will allow us the word) does not degenerate into mere book-making. Like its predecessors, this volume is one of practical utility both to the professional gardener and to the cultivator of flowers for their beauty. Much the greater part of the volume is occupied with a descriptive list of the most ornamental hardy flowers, with directions for their culture, suitable positions, &c.; but this is introduced by some practical hints on the general subject of gardening. That Mr. Robinson has the courage to attack some time-honoured gardening customs, will be seen from the following paragraph:—"No practice is more general, or more in accordance with ancient custom, than that of digging shrubbery borders, and there is none in the whole course of gardening more profitless or worse. When winter is once come, almost every gardener, although animated with the best intentions, simply prepares to make war upon the roots of everything in his shrubbery border. The generally accepted practice is to trim, and often to mutilate, the shrubs, and to dig all over the surface that must be full of feeding roots. Delicate half-rooted shrubs are often disturbed; herbaceous plants, if at all delicate and not easily recognised, are destroyed; bulbs are often displaced and injured; and a sparse depopulated aspect is given to the margins, while the only 'improvement' that is effected by the process is the annual darkening of the surface of the upturned earth." After this we find some pertinent and useful hints on the best mode of grouping hardy perennials, and the art of managing the rock-garden, the wild-garden, water, and boggy ground; on the culture and propagation of early flowers, and other subjects dear to the dweller in the country. Compared with the art of gardening as practised twenty years ago, we are certainly now in an altogether new and improved epoch, and Mr. Robinson is one of the pioneers to whom we are mainly indebted for the introduction of a better and more rational style. A. W. B.

Hints on Shore-Shooting: with a chapter on skinning and preserving Birds. By James Edmund Harting, F.L.S., &c. (London: Van Voorst, 1871.)

A GOOD sportsman, whether he knows it or not, must be more or less of a good naturalist, and this Mr. Harting is. His unpretending little book, therefore, certainly deserves mention here, and the more so since he has worthily won his spurs by making the group of birds most sought by the "shore-shooter" an especial subject of study. What he tells us is the result of his own observation, and is pleasantly told. What he does not tell us is whether "shore-shooting" has, with most people,—for we except him—any other *raison d'être* than the "fine-day-let's-go-and-kill-something" impulse. If not, we really do not see that there is much difference in principle between Pagham and Hurlingham.

LETTERS TO THE EDITOR

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

An Universal Atmosphere

I HAVE much pleasure in replying to Mr. Browning's question in NATURE, vol. iv. p. 487, as it is one that legitimately strikes at the root of all my speculations, and which, if unanswerable, conveys an objection that must demolish the whole structure I have endeavoured to erect in my essay on the "Fuel of the Sun."

If I am right, the atmospheres of the sun, the moon, the planets, or of any other cosmical body of known mass and dimensions, may be calculated in units of the earth's total atmosphere by the simple formula reasoned out in Chap. iii. of the above-named work, *i.e.*, by multiplying the mass of the body (expressed in units of the earth's mass) by its own square root, thus

$$x = m \sqrt{m};$$

where x is the atmosphere of the body in question expressed in units of the earth's known total atmosphere, and m is the mass of the body expressed in units of the earth's mass.

The mass of the moon being $\frac{1}{80}$ that of the earth, we get $\frac{1}{80} \sqrt{\frac{1}{80}} = \frac{1}{715 \cdot 5416}$, or, discarding fractions, the moon's atmosphere as $\frac{1}{715}$ that of the earth. But the diameter of the moon being to that of the earth as 0·264 to unity, the lunar surface will be to that of the earth as $0 \cdot 264^2$ or 0·0697 to 1, and the lunar atmosphere will be concentrated accordingly, bringing the mean atmospheric pressure on the lunar surface to

$$\frac{\frac{1}{715}}{0 \cdot 0697} = \frac{1}{49 \cdot 8355}, \text{ or } \frac{1}{50},$$

nearly of that of the earth's mean atmospheric pressure. Such an atmosphere would support a column of mercury six-tenths of an inch in height. Mr. Browning will recognise this as about equal to the best vacuum obtainable in an old-fashioned air-pump of average defectiveness.

Such is the theoretical pressure upon every part of the moon's surface, supposing the form of the moon to be a perfect spheroid of rotation with a perfectly smooth surface. But the moon is no such regular body. It presents far more irregularities in proportion to its size than would our earth if the ocean were evaporated, and its depths laid bare so that our mountain heights should be measured from the ocean bottom. Under such conditions the bulk of even our atmosphere would occupy the ocean valleys, and very rare indeed would be the remainder that reached the mountain tops and elevated ridges of the earth. On the moon with its filmy atmosphere of only six-tenths of an inch mean pressure, the rarefaction on the high lands and mountains would be carried beyond the limits of observable refractive power under the conditions assumed—viz., of a special atmosphere merging gradually into the universal interstellar medium.

The visible edge of the moon which effects the occultation of a star must in almost every possible case be formed by the ridges and summits of the lunar mountains, in no case by the bottom of the lower valleys, for in looking horizontally across the moon's rotundity these valleys and even the maria must be foreshortened, and their lower depths walled out of the reach of our vision by the great lunar elevations. Thus the occultation of a star would occur without its previous plunging behind any outlying lunar atmospheric matter of appreciable density. We must not forget that Sir J. Herschel's calculation, which assigns one second of refraction to an atmosphere equal to $\frac{1}{1980}$ of the density of the earth, is based on the theory of a limited atmosphere with a sharp and definite boundary suddenly terminating in a vacuum.

But this rarefaction on the elevated portions of the moon demands a compensating condensation or concentration of atmospheric matter in the valleys, crater-pits, and maria. Here the pressure on the moon's surface should considerably exceed the calculated mean. This consideration suggests a very interesting question. Would such an atmosphere, say capable of supporting one inch of mercury, produce any observable effects? If I am right in regarding water as one of the constituents of the universal atmosphere, there are good reasons for supposing that it would.

The small share of water due to the moon would all be raised far above its low boiling point, early in the lunar day, by the heated lunar surface. There would be no sea, no clouds, no rain, no snow, but on the plains and in the valleys a formation of hoar-frost should occur at the lunar eventide, beginning just where the sun's rays become too oblique to maintain the temperature of the rapidly radiating lunar surface above the freezing-point.

In a note appended to Mr. Lockyer's translation of M. Guillemin's work on "The Heavens," the Rev. T. W. Webb thus corrects the author's rather positive statements concerning the total absence of a lunar atmosphere: "After all fair deductions on the score of imperfection of observation or precipitancy of inference, there are still residuary phenomena, such as, for instance, the extraordinary profusion of brilliant points which on rare occasions diversify the Mare Crisium, so difficult of interpretation, that we may judge it wisest to avoid too positive an opinion." Now the Mare Crisium is a great depression of the lunar surface close upon that edge of the moon which, to our vision, first receives and loses the solar illumination. If I am right, aqueous vapour should be suddenly forming there during the early crescent period after the new moon, and the hoar-frost should be as suddenly precipitated as this wide depression rolls towards the darkness after the full moon. In that chapter of the "Fuel of the Sun" which is devoted to the meteorology of the moon and Mercury, I have discussed some of the theoretical results of these conditions and the appearances they should present. I may here merely add that, as the temperature of any part of the moon's unmantled surface must directly and very rapidly vary with the incidence of solar radiation, all the undulating regions of the moon must at morning and evening have a very patchy temperature, the slopes towards the sun being hotter than our tropics, while the opposite side of the same hill receiving the solar rays with great obliquity, and radiating into space almost without impediment, must retain a freezing temperature, and thus the cryophorous phenomena, which Sir John Herschel describes as a possible result of the contrasted temperatures of the opposite sides of the moon, should be effected even by the shady lunar craters and contrasted hill-slopes.

On the highlands of the moon no appreciable amount of hoar-frost precipitation should take place on account of the absence of sufficient atmosphere; but on the deeper maria, wherever the conditions are the most favourable, the patchy temperature should produce patches of such precipitation. If anywhere visible, these should be seen on the Mare Crisium, on account of its proximity to the edge of the moon, for there the morning rays that strike most obliquely upon the cold slopes would be the most effectively reflected towards the earth. Not having seen any original or detailed account of the phenomena to which Mr. Webb alludes, I am unable to say whether they fulfil these theoretical conditions, but I believe that something more may be learned by means of careful observations specially directed to the elucidation of the questions I have suggested.

W. MATTIEU WILLIAMS

Woodside, Croydon, Oct. 23

Pendulum Autographs

IT may interest some of your readers to know that they can for themselves observe in the most accurate manner the motion of the compound pendulum described by Mr H Airy* by merely attaching the ends of a fine thread to two points in the ceiling of a room, and suspending a leaden bullet by means of a second thread tied to the middle point of the former, so that the bullet may just escape the floor. Lay underneath a large sheet of white paper ruled with two dark lines at right angles to each other to correspond to the two axes of vibration. It is Mr. Airy's experiment with the hoop on an extended scale. The motion of the bullet, unimpeded by contact of pencil with paper, is graceful and accurate in the extreme.

Perhaps the most remarkable case is that in which the two points of suspension are taken about an inch apart, and the third about half an inch below them; the pendulum will now keep reversing its motion as uniformly as before, and apparently without any adequate cause, a matter of astonishment to the uninitiated spectator.

I believe the general equation to the path, including all the curves described, will be found to be $\sqrt{n}\, cos^{-1} \frac{x}{a} = \sqrt{m}\, cos^{-1} \frac{y}{b}$, where the particle starts from the point (a, b) and is attracted to the axes of X and Y by forces $= -ny$ and $-mx$ respectively.

Woolwich, Oct. 24 GEO. S. CARR

* See NATURE, vol. iv pp. 310, 317.

Exogenous Structures in Coal Plants

I CORDIALLY agree with your recommendation that discussion on the Exogenous Stems of the Coal Measures should cease for the present. It is evident that I shall not convince my two opponents, and they are as far as ever they were from convincing me. But I must request that in justice to me, you will allow me to enter a protest against the last paragraph of Prof. Dyer's article, in which he objects to my applying the term Protoplasmic to the cambium layer, and endeavours to show that I am two hundred years behind the age in my physiology. I cannot but think that Prof. Dyer, when he penned that paragraph, knew perfectly well in what sense I used that expression. I meant by it nothing more than is implied in the following sentence, taken from Prof. Balfour's "Manual of Botany," p. 43, which certainly does not belong to the age of Grew:—

"External to the woody layers, and between them and the bark, there is a layer of mucilaginous semifluid matter, which is particularly copious in spring, and to which the name Cambium has been given. In this are afterwards found cells, called Cambium Cells, of a delicate texture, in which the protoplasm and primary utricle are conspicuous."

W. C. WILLIAMSON

Fallowfield, Oct. 25

*** This correspondence must now close.—ED.

Classification of Fruits

IT seems from the numerous attempts that have been made that a philosophical classification of fruits is either unattainable or practically of very little value when attained. At any rate working botanists have, as a rule, discarded the majority of the carpological terms that are to be found in text-books as too cumbrous or too uncertain in their application. Among the latest attempts at simplification in the matter of the classification of fruits are those of my friends Prof. Dickson and Dr. M'Nab (see NATURE, vol. iv. p. 475). Both of these are open to some criticism on matters of detail, but I can hardly expect you to accord me space to point out what I believe to be the merits or shortcomings of their respective schemes. I should also trespass too much on your courtesy and on the patience of your readers did I enter into any engthened explanation of the following scheme, in which I have adopted to some extent the nomenclature of Prof. Dickson and Dr. M'Nab, and which I offer for consideration solely on the grounds of expediency and simplicity:—

Classification of Monothalamic Fruits

A. Ripe pericarp uniform
- Fruits indehiscent — I. Nuts or Achænocarps.
- Fruits dehiscent — II. Pods or Regmacarps.

B. Ripe pericarp of two or more layers of different substance *
- Seeds within a hardened endocarp — III. Stone-fruits or Pyrenocarps.
- Seeds embedded in pulp* — IV. Berries or Sarcocarps.

I. *Nuts or Achænocarps*

WINGLESS—
- Fruit of one carpel, or, if of more, apocarpous . . . . Achene
- Fruit of more than one carpel
  - Carpels ultimately separate but indehiscent . . . . Carcerule (Cremocarp).
  - Carpels inseparate
    - Pericarp adherent to the seed . . Caryopsis.
    - Pericarp free from the seed, within a cupule . . . . Glans.

WINGED . . . . Samara

II. *Pods or Regmacarps*

Fruit of one carpel
- Opening by one suture . . . . . Follicle
- two sutures . . . . . Legume
- transversely . . . . . Lomentum (Dichisma, *M'Nab*)

Fruit of more than one carpel
- Opening by pores or sutures . . . Capsule (Siliqua) (Regma) (Conceptaculum) (Tryma, &c.).
- Opening transversely . . . . . . Pyxis.

III. *Stone-fruits or Pyrenocarps*

- Carpels one or more, superior . . . . Drupe (Fibro-drupe as in *Cocos*, *Grewia*, sp., &c.).
- Carpels one or more, adherent to, or enclosed within a fleshy receptacle . . Pome (Sphalerocarpium, as in *Hippophae*)

IV. *Berries or Sarcocarps*

- Seeds embedded in pulp . . . . . Bacca (Hesperidium) (Uva) (Pepo).

I believe that the foregoing arrangement will include most of the varieties of fruits and seed-vessels, though, as in all similar cases, exceptional forms are not readily sorted into their proper place; the fruit of such Cassias as *C. Fistula*, generally called a lomentum, is a case in point. For general purposes the varieties enclosed in brackets may well be omitted, save in the case of so well known and constantly used a term as *siliqua*, which, despite Prof Dickson's veto, I think is too useful prac ically to be lightly abandoned.

MAXWELL T. MASTERS

* The pericarp is here understood as including not only the ripened carpellary wall, but also any adjunct to it which in process of development may be combined with it. In the same manner the pulp may be a production from the carpel or from the seed itself.

The Berthon Dynamometer

ABSENCE from home, and many engagements, have prevented an earlier reply to "W. R.'s" letter in NATURE, October 5. In my previous communication I believe I gave the address of the inventor, to whom I thought reference might naturally be made; in order, however, to meet "W. R.'s" wish, I will explain the construction of the very simple but efficient instrument in question. It is merely a V gauge, formed of two pieces of thin brass converging at a very acute angle, and graduated along one of the edges; the divisions being viewed through a lens held in the hand simultaneously with the image of the object-glass or speculum formed by the eye-piece; the diameter of that image is given at once by the divisions to $\frac{1}{500}$ of an inch, and can be readily estimated to half that value. The arrangement mentioned by "W. R." is no doubt very convenient, and quite adequate for his purpose; but for high powers I should suppose that the comparative coarseness of the engraved lines would make itself much more felt than it is in Mr. Berthon's invention, and the balance of economy is so greatly in favour of the latter in comparison with every contrivance with which I am acquainted, that I have no hesitation in saying that it ought to be in the hands of every amateur who cares to know the magnifying power of his telescope. It may be procured for five shillings, of Mr. Tuck, watchmaker, Romsey. T. W. WEBB

New Form of Cloud

THE kind of cloud described by M. André Poëy (NATURE, Oct. 19, 1871, p. 489) is by no means new or rare if I can judge correctly from the figure and explanation. It may often be seen on the lower part of the flank of a great rain or thunder cloud, and appears to arise from the dropping or subsidence of portions of the air heavily loaded with watery particles. My own impression is that it appears when the cloud is about to break up. M. Poëy will find the cloud described in the *Philosophical Magazine* for July 1857, where the name of *droplets* is given to the form, and its position in a thunder cloud indicated by a figure. J.

Spectrum of Blood

IN my letter, published in the last number of NATURE, I am strangely enough made to say that "we must not rely on *the* spectrum." This is an error of the printer. The sentence should have been :—"I have always argued that in such inquiries we must not rely on *one* spectrum, but compare the action of various reagents." H. C. SORBY

Broomfield, Sheffield, Oct. 28

Earthquake in Burmah

I HAVE not read in your "Notes" any record of the earthquake which was felt at daylight of the 16th February last in this city, in two successive and gentle but decided shocks, doing no damage, but which, from the files of the Calcutta *Englishman* of February 18, seems to have been severe to the N.W. of this, extending through Cachar, Silchar, Gowahatty, to Calcutta and Barrackpore.

This earthquake, you will observe, is synchronous with those of the western hemisphere already recorded by you.

CHARLES HALSTED

Mandalay, Burmah, Sept. 1

A Plane's Aspect

I AGREE with Mr. Proctor that the disuse of the term "position" in geometry would be a serious misfortune; happily, however, it is not its disuse, but the prevention of its misuse which is contemplated. I cannot agree with him that "position" is a word "which no one can misunderstand," for his own letter is a striking example of its being misunderstood, either by Mr. Proctor, or by others. "Aspect and slope," he tells us, "indicate two elements, which, together, *fix* the position" of a plane. Geometers, however, certainly understand, when a plane is said to be given *in position*, that something more than its aspect and slope may be regarded as known. Parallel planes have necessarily the same slope and aspect, but surely not the same position. To be told that, because its slope and aspect are invariable, the plane of Saturn's rings has a fixed position in space, notwithstanding that the planet moves bodily in its orbit, would scarcely satisfy a student of astronomy accustomed to geometrical precision.

There can be no doubt that "position" is the true English equivalent of the German word "Lage," and that no ambiguity of the kind above indicated could attach itself to the term, had we a suitable English rendering for the word "Stellung." I do not consider the term "aspect" to be perfect as an equivalent for "Stellung," but I have no hesitation in admitting that Mr. Laughton's suggestion is happier than any previous one I can remember. Mr. Proctor declares his intention of opposing the "use of the word 'aspect' in a sense not at present assigned nor properly assignable;" but when he wrote thus, he had not seen the letter of Mr. Wilson wherein the term "aspect" is very rigidly defined to be the direction of the normal. To me this very facility with which the word "aspect" lends itself to rigid definition, is a ground of objection against it. I have never seen Stellung defined in the manner in which Mr. Wilson has defined "aspect." Von Staudt, in whose admirable writings I first met the word, introduced it thus: "Parallel planes possess something in common, which may be regarded as appertaining to each one of them, and shall be called their 'Stellung;' the 'Stellung' of a plane, therefore, is determined by any plane which is parallel thereto, and two planes have the same 'Stellung' or different 'Stellungen' according as they are parallel to, or intersect one another."

That the term "aspect" is not sufficiently elastic to permit of its taking the place of "Stellung" in the above passage cannot, I think, be well maintained by Mr. Proctor, seeing that he has not himself hesitated to use it in two widely different senses in the following passage of his letter: "I can see no reason why 'aspect' should be regarded in a new and unfamiliar aspect." The expression "aspect of a plane," whether it be retained or not as the equivalent of the "Stellung einer Ebene," appears to me, I confess, to be much too good to be claimed by Mr. Proctor as indicative solely of *the direction of the projection of the normal upon a certain plane of reference.* I would suggest, in the interest of his twelve excellent books, that he might qualify "aspect," as thus defined, by an appropriate adjective, for the term is there used in a very technical sense indeed, and is not even applicable to all planes. Although Mr. Proctor can assign, for example, a southerly aspect to the face of a roof which has a slope of 30°, he would find some difficulty in describing the aspect of a roof which has no slope at all, whereas Mr. Wilson would without hesitation pronounce its aspect to be vertical.

Athenæum Club, Oct. 31 T. ARCHER HIRST

IT is due to my friend and your correspondent, Mr. Cecil J. Monro, of Hadley, to state that, to my knowledge, he was in the habit of employing the word "aspect" in this technical sense long before the publication of Mr. Laughton's letter, and I should not be surprised to learn that other geometers have used it before Mr. Monro.

I think Mr. Proctor will find few to agree with him in his condemnation of the word so used For myself I heartily agree with Mr. Wilson in the welcome he accords to this "old friend with a new face." C. M. INGLEBY

Highgate, N., Oct. 27

I AM glad to find, by Mr. Wilson's letter in NATURE for October 26, that the word "aspect," which I suggested, is accepted by him as satisfactory; as, in fact, the word wanted. But another correspondent in the same number, Mr. Proctor, pertinaciously insists on the superior merit of the word "position," to be used in the particular sense explained by Mr. Wilson in his former letter. In this I conceive Mr. Proctor is entirely wrong.

"In geometrical language"—I quote from Gregory's "Solid Geometry," 1845—"the position of a plane is determined by making it pass through three given points." Mr. Proctor says he "can see no reason why 'position' should be dismissed from the position it has so long occupied." No more can I. I would only call his attention to the fact that the meaning which he would assign to the word "position" is quite different from that which has been accepted, in a technical sense, by geometers, and in an everyday sense by everyday people.

Mr. Proctor's special objections to the word "aspect," rest, it seems to me, on a misconception of its meaning and familiar use. We speak of the aspect of a wall, but not of the aspect of a roof, nor of a hill. What the usage amongst builders in respect of roofs may be, I don't know, but geographers almost invariably speak of the "slope" of a hill, as, for instance, the southern slope of the Himalayas. Put into exact language, the aspect of a plane is the direction of its normal; and as parallel planes have parallel normals, any number of parallel planes have the same aspect, without reference to their position; but no two planes, parallel or not, can possibly have the same position.

The word "slope" is almost equally inadmissible; in the first place, it refers to some other plane, which is apt to cause confusion; and in the second, although all parallel planes have the same slope, any number of other planes not parallel can also have it; the word is therefore not sufficiently definite. "Tilt," a word spoken of by Mr. Proctor, as though it had been suggested, has no geometrical meaning whatever. As a substantive it is a "tent," or "awning;" it has also been sometimes used poetically as an equivalent to "tournament," and is, I believe, the familiar abbreviation of "tilt-hammer." These are its only meanings, and none of them apply to a plane.

I would only add that I do not quite see what the fact mentioned by Mr. Proctor, that he has written twelve books in the last six years—interesting as it may be from a bibliographical point of view—has to do with the matter.

Oct. 29 J. K. LAUGHTON

THREE elements are necessary to fix the position of a plane as I understand the word "position." If "aspect" and "slope" be the names of two of these, the third will be the perpendicular upon the plane from some fixed point. It is because the term "position" implies the fixedness of this third element that it is inappropriate to express my friend Mr. Wilson's meaning.

My friend Mr. Proctor will pardon me if I do not consider the question entirely settled by the fact that he has written perspicuously and explained clearly by the use of a term which fixes too much. With an improved scientific terminology, he will be able to make his next twelve books superior (if that be possible) to those he has written within the last six years.

"Aspect" and "slope" stand on the same footing, one connotes a reference to the points of the compass, the other to the horizon. Neither can be used in Mr. Wilson's sense without departing from their colloquial meaning, but it is perfectly competent for geometers to take a word from common conversation and give it a scientific meaning. Either of these words may be used in Mr. Wilson's test sentences. Parallel planes have the same slope, two slopes determine a direction, &c.

It is yet possible that some correspondent can suggest a better term, either one imported from ordinary life or one conceived for the purpose.

THE CORRESPONDENT WHO SUGGESTED "SLOPE"

Geometry at the Universities

PROF. THISELTON DYER has well pointed out a distinction which exists between the mathematical courses at Oxford and Cambridge. But his conclusion, that at Oxford "special attention to geometrical methods would pay very well," though acceptable in its way, falls far short of what I advocate. The great want at both Universities is a course of geometrical studies; and the proof that such a want exists is to be found in the fact that the geometrical treatises in use at either University, cover so very limited a range. There are not even any text-books on the geometry of the sphere, cone, cylinder, and like simple solids, or on such curves as the lemniscate, cycloid, and the simpler spirals. A few stray notes on these subjects may be found in some of the text-books, but not a thorough and systematic geometrical investigation of any of them. Geometrical treatises might with advantage range much further. A geometrical treatise on ellipsoids would be of immense use apart from its employment as a means of mental training. Geometrical treatises on paraboloids of both kinds, on hyperboloids of one sheet and of two sheets, on the various orders of ring-surfaces and screw-surfaces, and on many other tridimensional matters, would afford invaluable exercise to the student, besides having a real value to the scientific worker. I venture to express my conviction, that a course of such studies would tend to develop mathematical powers much more thoroughly even than the study of covariants and contravariants, Jacobians, Hessians, *et hoc genus omne*.

If there is one department of mathematical research in which our countrymen are fitted by their mental habitudes to distinguish themselves pre-eminently, it is precisely this neglected department of geometrical research. As it is, though we have geometricians of great power, no systematic geometrical work is done in England. Our treatises range only over the most elementary geometrical subjects, and even in discussing these subjects our writers are fain to accept the assistance of Continental geometricians. One would conceive that each of our Universities might yearly send out many who could treat of the elements of geometry without keeping a hand always on some French or German text-book.

Brighton, Oct. 27 RICHD. A. PROCTOR

DEEP-SEA DREDGING IN THE GULF OF ST. LAWRENCE

THE marine zoology of the deeper parts of the River and Gulf of the St. Lawrence has not been investigated until quite recently. This summer, under the auspices of the Natural History Society of Montreal, and in consequence of the kindness of the Hon. Peter Mitchell, Minister of Marine and Fisheries for the Dominion (who not only gave me facilities for dredging on board Government vessels, but also caused sufficient rope to be provided for the purpose), depths of from 50 to 250 fathoms were successfully examined. The greatest depth in the Gulf, to the west of the Island of Newfoundland, as given in the Admiralty charts, is 313 fathoms. It is thought that a general sketch of the results obtained may be of interest to the readers of NATURE.

The cruise lasted five weeks, the first three of which were spent on board the Government schooner *La Canadienne*, and the remaining two on the *Stella Maris*. The area examined includes an entire circuit round the Island of Anticosti, and extends from Point des Monts (on the north shore of the St. Lawrence) to a spot about half way between the east end of Anticosti and the Bird Rocks. As these investigations were almost necessarily subordinate to the special duties on which the schooners were engaged, in several cases the same ground was gone over twice.

The bottom at great depths generally consists of a tough clayey mud, the surface of which is occasionally dotted with large stones. So far as I could judge, using an ordinary thermometer, the average temperature of this mud was about 37° to 38° Fahrenheit, at least on the north shore. In the deepest parts of the river, on the south shore, between Anticosti and part of the Gaspe Peninsula, the thermometer registered a few degrees higher. Sand dredged on the north shore in 25 fathoms also made the mercury sink to 37° or 38°.

Many interesting Foraminifera and Sponges were obtained, but as yet only a few of these have been examined with any care. A number of Pennatulæ were dredged south of Anticosti; the genus has not been previously recorded, so far as I am aware, as inhabiting the Atlantic coast of America. They were found in mud, at depths of 160 and 200 fathoms, and it seems probable that this species, at least, is sedentary, and that it lives with a portion of the base of the stem rooted in the soft mud. *Actinia dianthus* and *Tealia crassicornis* were frequent in 200 to 250 fathoms. The Echinoderms characteristic of the greater depths are a *Spatangus* (specifically distinct from the common British species), *Ctenodiscus crispatus*, *Ophioglypha Sarsii* (very large), *Ophiacantha spinulosa*, and *Amphiura Holbollii*. Marine worms, of many genera and species, were both numerous and fine. Among the more interesting of the Crustacea were *Nymphon grossipes* (?) and a species of *Pycnogonum*. Several of the last named Crustaceans were taken at a depth of 250 fathoms, entangled on a swab, fastened in front of a deep-sea lead, which was attached to the rope, a few feet from the mouth of the dredge. This circumstance tends to show that the genus is not always parasitic in its habits. The Decapods, Amphipods, &c., at least those of greatest interest, have not yet been identified. Among the most noticeable of the marine Polyzoa are *Defrancia truncata*, and what appears to be a *Retepora*. Not many species in this group were obtained in very deep water, and those procured were, for the most part, of small size. About six species of Tunicates were collected. Being anxious to have Mr. J. Gwyn Jeffreys' opinion upon the various species of Mollusca during his visit to Montreal, I studied these carefully first, and submitted the whole of them to him for examination. Twenty-four species of Testaceous Mollusca were obtained at depths of from 90 to 250 fathoms. Nearly all of these are Arctic forms, and eleven of them are new to the continent of America.

The following are some of the most interesting of the deep-water Lamellibranchiata:—*Pecta grœnlandicus* of Chemnitz, but not of Sowerby; * *Arca pectunculoides* Scacchi; *Yoldia lucida* Loven; *Y. frigida** Torell; *Neæra arctica** Sars; *N. obesa** Loven. Among the novelties in the Gasteropoda of the same zone are the subjoined:—*Dentalium abyssorum* Sars; *Siphonodentalium vitreum* Sars; *Eulima stenostoma* Jeffreys; *Bela Trevelyana*;* *Chrysodomus (Sipho) Sarsii*.* Three Brachiopods occur in the Gulf, of which *Rhynchonella psithacea* and *Terebratella Spitzbergensis* are found in about 20—50 fathoms, and *Terebratula septentrionalis* in from 100—250. A few rare shells were obtained in comparatively shallow water; among them an undescribed *Tellina* (of the section *Macoma*), a new *Odostomia*, and *Chrysodomus (Siphho) Spitzbergensis** Reeve. Nor were even the Vertebrata unrepresented; from a depth of 96 fathoms off Trinity Bay, a young living example of the Norway "Haddock" (*Sebastes Norvegicus*) was brought up in the dredge. And off Charleton Point, Anticosti, in 112 fathoms, on a stony bottom, two small fishes were also taken; one, a juvenile wolf-fish (*Anarrhicas lupus*) the other a small gurnard, a species of *Agonus*, probably *A. hexagonus* Schneid.

The similarity of the deep-sea fauna of the St. Lawrence to that of the quaternary deposits of Norway, as described by the late Dr. Sars, is somewhat noticeable. *Pennatulæ*, *Ophiura Sarsii*, *Ctenodiscus crispatus*, several Mollusca, &c., are common to both; but on the other hand, the absence of so many characteristic European invertebrates on the American side of the Atlantic should be taken into consideration. The resemblance between the recent fauna of the deeper parts of the St. Lawrence, and that of the Post-pliocene deposits of Canada, does not seem very close, but our knowledge of each is so limited that any generalisations would be premature.

J. F. WHITEAVES

* I am indebted to Mr. Jeffreys for the identification of species to which an asterisk is attached. He corroborates also my determination of the remainder.

THE REDE LECTURE AT CAMBRIDGE

ONE of the indirect results of university reform has been the establishing at Cambridge of the Rede Lecture, one of the highest intellectual treats of the whole year, as will at once be acknowledged when the names of the distinguished persons who have delivered it since its establishment in 1858 are known—viz., Professors Owen, Phillips, Max Müller, Willis, Ansted, Airy, Tyndall, Miller, Ruskin, Huggins, General Sabine, Sir W. Thomson, and Mr. Norman Lockyer. For many years past there had been certain lecturers at various colleges, whose duty it was to deliver lectures on mathematics, philosophy, rhetoric, and logic; but in 1858 the endowments for these lectures (originally given in 1524 by Sir Robert Rede, Chief Justice of the Common Pleas in the reign of Henry VII.) were amalgamated, and the result has been the delivery once a year of the Rede Lecture by some distinguished man of science chosen by the Vice-Chancellor for the time being. Such is the history of the benefaction; but it must now be added that as the remains of this distinguished man lie in a village church in Kent, that of Chiddingstone, near Eden Bridge, in which parish he lived and died, without a memorial or inscription of any kind over his grave, it is proposed to do for him what Cicero did for the unhonoured grave of Archimedes, and an effort is, therefore, being made to mark his place of burial by erecting a window of stained glass in the chancel that he built. The cost of the memorial, with suitable inscription, cannot be less than 160*l*., but nearly 70*l*. has been raised by subscriptions from the distinguished persons who have delivered the lecture, and by other friends, members of the university and otherwise—viz., the Earls of Powis, Derby, and Strathmore, the Vice-Chancellor, the Masters of Jesus and Clare Colleges, the Provost of King's, Professors Selwyn and Sedgwick, Mr. Beresford-Hope, M.P., Sir John Lubbock, M.P., the Public Librarian, Rev. W. H. Latham, and J. Brocklebank, with many others; but the amount thus subscribed, together with the local effort, is inadequate for the full completion of the memorial, and it is hoped that there will be some others who will be willing to help on the work. Mr. Norman Lockyer, F.R.S., the present holder of the office of Rede Lecturer, has kindly consented to receive subscriptions at 6, Old Palace Yard, Westminster.

It is proposed to erect the following inscription, from the pen of Professor Selwyn, who will receive any subscription forwarded to him at Cambridge.

IN PIAM MEMORIAM
ROBERTI REDE MILITIS
CAPITALIS JVSTICIARII
DOMINI REGIS HENRICI VII.
DE COMMVNI BANCO
QVI HOC SACELLVM
ÆDIFICAVIT
GRATI AC MEMORES
BENEFICIORVM
CANTABRIGIENSES SVI
HANC FENESTRAM
PONI CURAVERVNT

*THE CONJOINT EXAMINATION SCHEME**

THE proposition carried at the last meeting of the Council of the College of Surgeons clears away, we suppose, the last difficulty in the way of an amalgamation between the Colleges of Physicians and Surgeons for the purposes of examination and of issuing diplomas. It is remarkable that the College of Surgeons should have come back to the original proposal, though it was at first demurred to and given to a committee for consideration. The College of Physicians, at its Comitia on Thursday, finally agreed to this proposal; and it now only remains for the General Medical Council to give its consent under the Medical Act of 1858, so as to allow of the fusion in question.

In order to get at the practical working of the proposed scheme of division of fees, we may take the present income of the College of Surgeons from the membership diploma, adding 10*l*. for each diploma issued to represent the additional fee to include the College of Physicians. The sum produced by the membership diploma during the last financial year was close upon 8,000*l*.; and if we add 10*l*. for each of the 291 diplomas issued, we have in round numbers the sum of 11,000*l*. The proposed scheme is, that one-half of this should be devoted to all the expenses of the examinations, and that the remaining moiety of 55,000*l*. should be divided into thirds. One-third is to go to the support of the Museum of the College of Surgeons and its unendowed professorships, one-third for the maintenance of the *personnel* of the College of Physicians, and one-third similarly to that of the College of Surgeons. This will give the Hunterian Museum and each of the Colleges some 1,800*l*. a year apiece, irrespective of other sources of income. With this income, it will, we imagine, be perfectly possible to carry on satisfactorily the establishments in Pall Mall and Lincoln's Inn Fields, if due economy be observed and proper supervision exercised over the subordinate officials. The Hunterian Museum will be upon a somewhat shorter allowance than heretofore; but if this prove insufficient, Parliament must be appealed to for a grant in favour of what the Council of the College of Surgeons properly characterises as an "institution of national as well as professional importance."

* Reprinted from *The Lancet*.

SIR RODERICK MURCHISON

THE life of a scientific man is for the most part uneventful, and perhaps to the world at large uninteresting. That he was born, lived a certain number of years, and died, are often the chief facts chronicled of the man himself. Of his work and of the influence of his work men are willing to read, but for the story of his life, with its quiet everyday monotony, they care little. Yet it is true, at least of the higher type of mind, that the story of the man's life and the history of the work he accomplished are inseparably connected, and are each necessary for the understanding of the other. There arise, too, ever and anon instances when the man was not merely a man of science, but one whose scientific career formed as it were a nucleus round which many other and often divergent interests gathered. Such a man's life is sometimes linked in so many ways with that of the society in which he lived, that its chronicle becomes in some degree the history of his time. And such a man was Roderick Impey Murchison. By no means standing on the highest platform of scientific intellect, a patient gatherer of facts rather than a brilliant generaliser from them, he yet gained by common consent in the commonwealth of science the position of a king, under whom men of all ranks, and even men of far higher ability and attainment than his own, were not only willing but delighted to serve. He held a place which no other man of science left among us now fills. It was not merely his achievements in geology, memorable as these were, which gave him that proud pre-eminence, nor did he owe anything to success in other branches of science, for he seldom travelled beyond what he knew to be his proper domain, nor to graces of literary style, on which men of slender acquirements often float

THE LATE SIR RODERICK I. MURCHISON

into popularity. He wrote only on geological and geographical subjects, and in a solid matter-of-fact way not likely to attract readers who did not previously care for his subjects. It was his personal character, his noble-heartedness, his indomitable energy, his tact and courtesy, the dignity and grace which he never failed to show even to opponents, and the social position which his family and fortune gave him, and which enabled him greatly to extend the respect shown in society to science and scientific men,—it was these causes which largely went to make Sir Roderick's influence what it was. A narrative, to do him justice, should tell how these causes came into play, and how, combined with the regard which he could always claim for his solid contributions to science, they placed him so high in the scientific circle in which he moved.

Murchison was born on February 19, 1792, at his father's little estate of Tarradale, in Eastern Ross shire. He used to speak with fondness of the fact that he first saw the light amid those old palæozoic sandstones, conglomerates, and schists, on which he was afterwards to rest part of his title to fame. Yet it was not among the wilds of Ross that he acquired a love for rocks. He was removed from his birthplace at an early age, and taken into Dorsetshire, and though when still a child he was brought back into Scotland, and remained with his mother at Edinburgh for a short while, it was in England that he spent most of his boyhood, and where he was educated. At the age of fifteen he obtained a commission in the 36th Regiment of Foot, and served in the Peninsula under Sir Arthur Wellesley. He carried the colours at the Battle of Vimiera, and went through much hardship in the retreat of Corunna. At the end of the war, after having become a captain of dragoons, he quitted the army, and marrying the daughter of General Hugonin, settled in England. So ended what he used to call the military episode of his life. Next came the fox-hunting period, when his activity of disposition found vent in the excitement of the chase, into which he threw himself heart and soul. He might have continued a merry, hearty, sporting, country gentleman, but for the influence of his wife, who was fond of natural history pursuits, and the advice of Sir Humphrey Davy, who, meeting him at the house of Mr. Morritt, of Rokeby, and seeing in him promise of something better than fox-hunting, advised him to attend the Lectures of the Royal Institution. Sir Roderick used to tell an interesting anecdote of that early beginning of his scientific career. He was attending the lectures of (if we remember) Dr. Brande, when one day the lecturer's place was taken in his absence by a pale thin lad, his assistant, who gave the lecture and experiments in so admirable a manner as to be received at the end with a hearty round of applause. It was Michael Faraday, and this was his first public appearance.

After gaining considerable knowledge from public lectures and private instruction, Sir Roderick's active mind sought as early as possible to study Nature in the field. Geology was the branch of science which suited best a nature so fond of out-of-door life as his. He had made the acquaintance of William Smith, the father of English Geology, from whose own lips he had learned the order of succession which the marvellous patience and ingenuity of that pioneer of the science had made out for the rocks of England and Wales, and indeed, as was afterwards found, for the rocks of all the world. In the year 1825, when he was thirty-three years of age, he wrote his first-published paper, "A Geological Sketch of the North-western Extremity of Sussex and the adjoining parts of Hants and Surrey." From that time onwards for nearly half a century he continued to furnish accounts of his observations in the field. Beginning, as was natural, with the district in which he lived, he soon extended his researches even as far as his own native Highlands, then step by step over the Continent of Europe, even as far as the confines of Asia. He has published more than 100 memoirs on British and Continental Geology, besides numerous addresses to scientific societies, and in addition to upwards of twenty memoirs in conjunction with other authors. To all this mass of work must be added what he published in separate volumes—his great "Silurian System," his splendid volumes on "Russia," and the successive editions of his "Siluria."

Of the incidents of his life during its scientific period it is not necessary here to say much, nor to try to count up the honours showered on him from all parts of the world. There was hardly a scientific Academy anywhere which had not enrolled him among its associates, and to the dignities conferred on him by his own Sovereign, were added others conferred by Emperors and Kings abroad. His time was largely passed in London, where he took an active share in scientific work. But every year he made a tour either in this country or on the Continent, and added to our knowledge of the geological structure of the districts which he visited. Sometimes these tours were prolonged, and in the case of his Russian campaign he was absent for two or three years from England.

At the time when Murchison broke ground as a geologist, the science of geology had entered a new phase of its history. The absurd system of Werner, though still upheld by high authority in this country, was daily losing ground, and the simple and obvious classification of William Smith on the one hand, and the doctrines of Hutton on the other, were guiding all the younger intellects of the day. Murchison's tact is nowhere more conspicuous than in his choice of a field for the exercise of his patient energy of research. He saw that the old Wernerian notion of "transition" rocks was doomed, and that it would be a task well worthy of his time and toil to unravel the succession of these rocks, and try to introduce into them the same order and consistency which Smith had shown to mark the Secondary series of England. He chose for the scene of his researches the border country of England and Wales, where these old rocks are well displayed, and after five years of unremitting labour he produced his "Silurian System"—a work, which, though dealing only with the rocks of a limited tract of Britain, yet first unfolded the earlier chapters of the history of life upon our globe. The classification he adopted, though of course necessarily subject to local variation and change, has been found to hold true on the great scale over the whole world.

This work laid the foundation of Sir Roderick's fame. In his subsequently published "Siluria," which has gone through several editions, he recast the original work, introducing much detail regarding the extension of Silurian and older palæozoic rocks into other countries; but while in the later publication, the results given were necessarily often the work of other observers—the "Silurian System" remains a monument of the unaided labour of a mind quick in observation, sagacious in inference, patient in the accumulation of data, and full of that instructive appreciation of the value of facts not yet understood, which is near of kin to genius.

It would be beyond the limits of this journal to offer an adequate outline of Sir Roderick's scientific work. He was distinctly and specially a geologist. His early attachment to palæozoic rocks never waned, and though now and then he was led to make and record observations on later formations, he always returned to the older deposits as his natural inheritance and domain. He was not a palæontologist, but no geologist could use more skilfully than he the data furnished by palæontology. This faculty he acquired at the beginning of his career, and it marked all his work in the field both at home and abroad. It enabled him to apply to distant countries the principles which he had so successfully used in his own. Perhaps the leading idea of his scientific life should be regarded as the establishment of the order of succession among rocks. This was what he did in the Silurian region originally, and what he always endeavoured to ascertain in every district to which choice or accident might lead him. He had a singularly quick eye for the geological structure of a country. No one who travelled with him through a hilly tract, and, after listening to his rapid inferences, has gone actually over the ground to see, could fail to be struck with the accuracy with which he seized on some of the leading features, and from these deduced the general arrangement of the rocks. It was in this way, and by the use of palæontological evidence, that he was enabled to arrive at one of the most brilliant generalisations he ever achieved, when he brought order and intelligibility into the chaos of the so-called primary rocks of his own Scottish Highlands—a deduction which is, perhaps, destined to bear fruit of which he never dreamed, in the still obscure subject of metamorphism.

Sir Roderick Murchison's early training in geology was acquired at a time when men believed in periodic cataclysms, by which the surface of the globe was destroyed and renewed. He never could, and he never seemed seriously to try, to shake himself free from the influence of that training. Though he modified his views as years went on, he remained a member, and indeed in this country the leader, of the Cataclysmic School. The upholders of a long line of successive creations and of the former greater intensity of all geological causes have lost in him one of their ablest, staunchest, and most influential associates.

To the world at large, however, it was not from his geological work chiefly that Murchison was known. His generalisation as to the probable gold-bearing nature of the Australian quartz-country, and as to the probable aspect of the interior of Africa, are probably familiar to most people. But in later years what has especially brought his name into prominence is the chivalrous devotion with which he has maintained one might almost say the national belief in the welfare of Dr. Livingstone. Yet this is only a sample, though one which has come more publicly before us, of the tenacious friendship and active benevolence which have always marked him. As President of the Geographical Society—a society which is in a sense his own creation—he had frequent opportunities of befriending not only the cause of geography but the personal well-being of travellers, and he never failed to use them. The geographers have good cause to lament the death of their chief.

Of the man himself, what he was as he lived and moved among us, his loss is too recent to permit us justly to speak. We can only think of him as the stately courteous old gentleman, carrying even to the last that military bearing which dated from the days of Wellesley and Moore, kindly and thoughtful in his kindliness—a man whose friendship, once given, even ingratitude and injustice could not wholly alienate. He was not without some of the littlenesses of humanity, but they were so transparent, and often even so child-like, that we forget them in the recollection of all the goodness of heart and strength of head and nobility of nature which have gladdened us for so long, but which are now only subjects of tender remembrance.

ARCH. GEIKIE

HOMOPLASY AND MIMICRY

ALL students of the remarkable phenomenon of superficial resemblances in the animal and vegetable kingdom will be glad that Prof. Dyer has published an extension of the paper which he read on this subject at the Edinburgh meeting of the British Association. It is especially satisfactory that he has abandoned the very objectionable term "pseudomorphic," and substituted that of "homoplastic," a very much better term, because it simply expresses a fact without committing one to any theory. There are, however, one or two points in his paper of last week, on which I should wish to be allowed to comment.

Prof. Dyer holds that the distinction between "mimicry" in animals and "homoplasy" in plants, is "sufficiently obvious," the difference assigned being, apparently, that in the one case it takes place between species found in the same locality, in the other between species unconnected geographically. I doubt, however, whether facts will warrant this distinction. The most remarkable instances of "mimicry" among animals hitherto published are, undoubtedly, in the case of species inhabiting the same area; but I am inclined to think that, when attention is called to the subject, others will be found between animals not so associated, though these instances would naturally not attract so much observation. And secondly, homoplasy in plants does frequently occur in species occupying the same area. The statement reported to have been made by Prof. Dyer at Edinburgh that "the resembling plants are hardly ever found with those they resemble," would scarcely be borne out by a careful investigation. The real objection to the terms "mimicry" and "imitation" is that they seem to imply a *conscious* effort at convergence, which will hardly be conceded in the case of Lepidoptera any more than of Ferns. The substantial difference betweeen the two is that, in the case of animals, the resemblance appears to be protective, while in the case of plants, there is seemingly no such benefit arising from it; but this is a difference in result and not in the nature of the phenomenon itself. I fail to see that the objections to the use of these terms in the case of plants do not equally apply to animals; we have no reason to suppose that the two sets of phenomena are not produced by similar causes.

Prof. Dyer states, and no doubt truly, that the external resemblances of plants may frequently be traced to the effect of similar external conditions, and quotes in support Mr. E. R. Lankester's view with regard to animals. But in assuming that this explanation will account for all such phenomena if fully investigated, I think too much is assumed. Cases of homoplasy are referable to two distinct classes—resemblances in general habit, and resemblances of particular organs. The former, as in the case of the homoplasy between a *Cactus* and a *Euphorbia* or a *Stapelia*, or between a *Kleinia* and a *Cotyledon*, are no doubt due to the operation of similar external conditions of climate and soil. But in the second class this explanation wholly fails.

As illustrations of the kind of resemblance I mean, I may refer to the two collections of "mimetic plants" exhibited by Mr. W. W. Saunders at the two last *soirées* of the Linnean Society, a list of which will be found in NATURE for May 26, 1870, and May 4, 1871. The extraordinary resemblance in the markings of the leaves in plants thus grouped together, might well deceive the most experienced botanist. To account for this homoplasy on the ground of similar external conditions, is to start a mere hypothesis, without any facts to warrant it. A still more curious series of resemblances occurs in the case of fruits than of leaves, so close that it has deceived botanists of the experience of the elder Hooker, Bentham, and Kunth into placing species in a genus with which they have no structural affinity whatever. I have in my mind in particular two samaroid fruits, both from the forests of Brazil, so absolutely identical in external facies, that distinction is quite impossible without dissection, and yet belonging to exceedingly remote orders. I will not, however, say more on this point, as it would be impossible to appreciate the closeness of the homoplasy without drawings, which I hope shortly to be able to publish. The singular part of this resemblance is, that, as far as we know, it is never protective. In our Bee-orchis we have what might well have been assumed *primâ facie* to be a case of protective resemblance, the flower being so fashioned in order to attract bees to assist in its fertilisation. It is remarkable, however, that the Bee-orchis is one of the few plants that appear to be perpetually self-fertilised, never being visited by insects. It is just possible that we have an instance of protective or rather beneficial resemblance of scent in the case of the carrion-like odour of the flowers of *Stapelia*, which attracts blue-bottle and other flies.

In a paper read at the recent meeting of the American Association for the Advancement of Science, by Prof. E. D. Cope, I find the following thoughtful remarks:—"Intelligence is a conservative principle, and will always direct effort and use into lines which will be beneficial to its possessor. Thus, we have the source of the fittest, *i.e.*, addition of parts by increase, and location of growth-force directed by the will, the will being under the influence of various kinds of compulsory choice in the lower, and intelligent option among higher animals. Thus intelligent choice may be regarded as the originator of the fittest, while natural selection is the tribunal to which all the results of accelerated growth are submitted. This preserves or destroys them, and determines the new points of departure on which accelerated growth shall build."

Biologists generally are, probably, hardly prepared to apply the terms "intelligence" and "will" to the vegetable kingdom; but the use of the term "vegetable life" seems to me to imply of necessity that there are powers at work in the economy of the plant, as of the animal, which it is vain to attempt to reduce to manifestations of the forces which govern the inorganic world.

ALFRED W. BENNETT

NOTES

In our present number we give a portion of Prof. T. Sterry Hunt's Address at the Indianopolis meeting of the American Association, and propose in following numbers to reprint some of the more important papers read at the meeting. The next meeting will be held at San Francisco, and the following officers were elected for the meeting of 1872: President, Prof. J. Lawrence Smith, of Louisville; Vice-President, Prof. Alex. Winchell, of Ann Arbor; Permanent Secretary, Prof. Joseph Lovering, of Cambridge; General Secretary, Prof. E. S. Morse, of Salem; Treasurer, William S. Vaux, of Philadelphia; Auditing Committee, Dr. H. Wheatland, of Salem, and Prof. H. L. Eustis, of Cambridge; Standing Committee, *Ex Officio*, Messrs. Smith, Winchell, Lovering, Morse, Vaux, Gray, Barker, Putnam. Committee from the Standing Committee to arrange for next meeting, Profs. J. L. Smith, Asa Gray, Joseph Lovering, in connection with a committee from the Association at large, consisting of Profs. J. L. Smith, J. D. Whitney, and O. C. Marsh.

The Senate of the University of London on Wednesday last week exercised for the first time its privilege, under the Public Schools Act, of appointing a member of the governing body of Rugby and Charterhouse Schools. To Charterhouse it appointed Mr. Busk, F.R.S., President of the Royal College of Surgeons, thus recognising the claims of science in the direction of education. To Rugby it nominated Dr. Temple, Bishop of Exeter (late head master of Rugby).

The Inaugural Meeting of the Newcastle College of Physical Science on Tuesday last week was a great success. The Council decided unanimously, on the application of upwards of seventy ladies, to make no distinction of sex in the admission of pupils, placing all on a footing of exact equality. The total number of students admitted up to the time of the inaugural ceremonial, was fifty-one. In contrast to this we may note that last week the governing body of the University of Edinburgh rejected by a small majority Dr. Alexander Wood's motion, "That, in the opinion of this Council, the University authorities have, by published resolutions, induced women to commence the study of medicine at the University; that these women, having prosecuted their studies to a certain length, are prevented from completing them for want of adequate provision being made for their instruction; that this Council, without again producing any opinion on the advisability of women studying medicine, do represent to the University Court, that, after what the Senatus and Court have already done, they are at least bound in honour and justice to render it possible for those women who have already commenced their studies to complete them."

According to M. Le Verrier, Prof. Alluard of Clermont-Ferrand has obtained a grant of the necessary funds for establishing his long-projected observatory on the summit of the Puy-de-Dome.

Fathers Secchi and Denza and M. Diamilla Müller are engaged in organising a series of researches in the Mont Cenis tunnel, for the purpose of ascertaining what variations gravity and magnetism may undergo there.

The Mayor and other inhabitants of the town of Belfast lately expressed their sense of Prof. Wyville Thomson's many efforts for the encouragement of Science, and for the improvement and gratification of the working classes, in a suitable address, accompanied by a valuable service of plate.

The *Bulletin Astronomique* gives the following observations of Tuttle's comet. From M. Borrelly of Marseilles:—October 12, Marseilles M.T., $16^h\ 29^m\ 19^s$, R.A. $9^h\ 9^m\ 44^s\ 68$, Decl. + $44°\ 16'\ 15''\cdot1$. The comet had the appearance of a diffuse nebulosity badly defined; it appeared elongated in the direction N.W. by S.E.; it was feeble but of moderate extent, about $2'\ 20''$. The approximate correction of Mr. Hind's ephemeris, given by this first observation, is $\Delta\alpha = +\ 0^m\cdot5$, $\Delta\delta = +\ 1°\ 3'$. From MM. Loevy and Tisserand of Paris:—October 14, Paris M. T. $12^h\ 36^m\ 12^s\cdot2$, R.A. $9^h\ 14^m\ 35^s\cdot29$, polar distance, $47°\ 12'\ 13''\cdot1$. The comet resembled a whitish nebulosity, diffuse, and of irregular form. Its diameter was about $3'$; the light scarcely that of a star of the 13th magnitude.

Dr Hooker, of Kew, has placed the Lichens which he collected during his Morocco expedition in the hands of the Rev. W. A. Leighton, of Shrewsbury, for examination and determination.

The first Servian Agricultural Exhibition was opened with great ceremony at Belgrade, on October 2.

In addition to the announcements last week, the following works are in preparation:—From Edward Stanford:—The Laws of the Winds Prevailing in Western Europe, by W. Clement Ley, with charts, diagrams, &c., Part I.; Notes on the Geography of North America, Physical and Political, intended to serve as a text-book for the use of elementary classes; Notes on the Geography of South America, intended to serve as a text-book for the use of elementary classes. The following additional volumes are also announced to Weale's Series, published by Lockwood and Co.:—Analytical Geometry and Conic Sections, by J. Hann, new edition, entirely re-written by J. R. Young, numerous diagrams; Treatise on the Construction of Iron Bridges, Girders, Roofs, and other Structures, by Francis Campin, C.E., numerous illustrations; Drawing and Measuring Instruments, by J. F. Heather, M.A., numerous woodcuts; Optical Instruments, by J. F. Heather, M.A., numerous woodcuts; Surveying and Astronomical Instruments, by J. F. Heather, M.A., numerous woodcuts; Physical Geology, partly based on Portlock's "Rudiments of Geology," by Ralph Tate, numerous woodcuts; Historical Geology, partly based on Portlock's "Rudiments of Geology," by Ralph Tate; Emigrants' Guide to Tasmania and New Zealand, by James Baird, B.A.; Workman's Manual of Engineering Drawing, by J. Maxton, seven plates and nearly 325 woodcuts; Mining Tools, for the Use of Mine Managers, Agents, Students, &c., by W. Morgans; Atlas to the above, containing 235 illustrations.

A new horticultural Magazine is announced to be shortly commenced, with the title of *The Garden*, under the editorship of Mr. W. Robinson, F.L.S., author of "Hardy Flowers," "Alpine Flowers for English Gardens," &c.

A complete geological and statistical history of Australia by C. E. Meinicke, with a magnificent coloured map by A. Petermann, appears as a supplementary number of Petermann's "Mittheilungen."

The Ven. Archdeacon Pratt has reprinted a lecture on "The Descent of Man in Connection with the Hypothesis of Development," delivered at the Dalhousie Institute, Calcutta, on July 28, in which the Darwinian doctrine of evolution is vigorously combated.

The High Wycombe Natural History Society has resolved upon a new course of action suggested by the fact that its meetings had become pleasant social gatherings rather than in any way furthering the pursuit of natural science. In future the meetings will be held at the house of the President, the Rev. T. H. Browne, F.G.S., and will partake more of the nature of classes for the study of certain subjects. A loss in the number of members is expected, but it is hoped that those who remain will benefit by the change. Other local societies would do well to adopt a somewhat similar arrangement. The Quarterly Magazine of the above body is discontinued.

Assistant-Surgeon Verchere, of the Indian Army, has suggested, says the *Medical Times and Gazette*, that some experiments should be made with reference to meteorological influences on sickness and health. Medical Meteorology in India is still all but an unknown science, and, as at present studied, is useless to medical practitioners. The long range of "readings" tells us nothing; but a register of the effects of meteorological conditions on the men selected for the purpose, with all conditions of exposure, &c., taken into account, and compared with the average sickness of a corps for the same period, would teach us more in a few months than yards of meteorological tables. We understand that the Sanitary Commissioners of India are favourable to the proposal of Mr. Verchere, and it will therefore, probably, be carried out.

The *Wigtonshire Free Press* says that the foundation of a lake dwelling has been discovered by Mr. Charles Dalrymple, Kineliar Lodge, Aberdeenshire, on a small circular island at the south end of the Black Loch, Castle-Kennedy. On removing the surface soil, a circle of stones was discovered, the diameter of which was between 50 and 60 feet. On digging deeper through the stratum of forced earth and stones, three feet thick, what appeared to be a different and older layer of soil was reached. Among this black earth were found wood ashes, bits of calcined bones, and flat stones placed contiguously. Immediately below the stones, at the depth of a few inches, an artificial flooring was discovered, formed of the trunks of oak and alder trees. At this point the level of the loch was reached, and the influx of water prevented further excavations in a downward direction. In 1865-6, by the draining of Dowalton Loch, in the same county, several lake-dwellings were exposed; in the spring of this year, when the White Loch of Castle-Kennedy, which is now in connection with the Black Loch by a short canal, was being dragged with a net for trout, the net brought up a canoe of ancient make. In all likelihood it was the ferry-boat, or one of several perhaps, used by the lake-dwellers.

There is a volcanic eruption going on in the Hawaian Islands at Maunalva, but its exact site has not been recognised. From Kowa the lava was seen at night to rise to a height of several hundred feet in a column. The eruption is supposed to be near the locality of that of 1868, while others think it is nearer the summit of the mountain, on the scene of the great eruption of 1859. On September 6, an eruption took place on the southern slope of Maunalva.

The Constantinople earthquake is now known to have originated in the southern region of the island of Scio, where it began strongly, growing weaker towards its northern circumference. At the Dardanelles it was much sharper than at Rodosto, while at Boorgas, on the Black Sea, it was very slight, and further on at Varna was not felt.

Another small place to be marked soon as a big one is Chimbote on the coast of Peru. Its harbour, the finest in the South Pacific, can shelter the navies of the world. It was a great town in the times of the Incas, as remains of a colossal aqueduct will show. Near it are coal mines. It has been abandoned and neglected on account of the difficulties of access, but a railway is now to be constructed to the fertile interior at a cost of 6,400,000*l*.

To the map of Bolivia must be added the small town of Calama in the new mining district of Caracolas.

Experimental farms are now being extended in the Madras presidency—a most essential step for agricultural improvement and practical instruction.

The Government of Madras has been ordered to furnish special information on the Neilgherry nettle fibre plant.

On the 5th July a most destructive typhoon attacked Hiogo, in Japan.

In the same presidency, in the Parambalore district, a man-eating tiger has appeared, and killed four men, so that the Government has taken him into consideration, and placed a price of 30*l*. on his head.

The Island of Gorgona, off the coast of Choco, is much complained of by ship captains for its electric storms, and its irregular currents. It has held this reputation since the time of Pizarro.

A valuable discovery of workable lead ore is announced from Jersey.

The latest report from Tasmania in regard to the experiments for introducing salmon and trout into that country, shows that while the success of the cultivation of both is extremely probable, the existence of trout of large size is unmistakeable.

Coal has been found in large quantities on the banks of a stream flowing into the Godavery, about 224 miles from Juggianet, and ninety-six from Budrachellum. It is close to the surface, and it is extremely probable that fresh deposits will be found in the adjacent British territory.

It is to be noted that on the night of the 21st of August a very severe earthquake was felt at Callao, in Peru, at 8.32 p.m. The undulations were from N.W. to S.E. The shock was of fifteen seconds' duration. It was also felt severely at Cero, Azul, and Pisco. The sea, which previously had been unusually calm, suddenly became very rough, and a strong southerly wind set in. For two days the sea remained very rough at Cero Azul. The observations were confirmed by the steamship *Colon*. The shock severely shook the ship while it lasted. It was felt six miles to the westward of Chala Point at 8.50 p.m. ship time, and the sea almost immediately thereafter became agitated.

Measures are being taken by the Chilian Congress to prohibit the destruction of timber, particularly in the neighbourhood of springs. The timber districts of the provinces of Llanquihue, Valdivia, Chiloe, and of the Magellan territory are exempted from the law.

Cocos Island, in lat. 5° 30′ N. in the Pacific Ocean, about 600 miles west of the Columbian coast, has now for some years been occasionally occupied by treasure seekers on a legend of a treasure buried by buccaneers. At present it is again abandoned, but it is alleged a new expedition is organised. The island is not flat, as stated in many newspapers, but is volcanic, and 2,000 feet high. It is covered with timber and scrub, and being visited by frequent and heavy rains is always green. The place is riddled with shafts, some 150 feet deep. It produces nothing eatable.

The valuable timber so abundant in the North Island of New Zealand is deserving of a better fate than to be cut down wholesale and used as firewood. The rimu, or red pine, is most valuable for furniture and all ornamental work; the matai, or black pine, is more brittle and heavy than the other, but will take a most beautiful polish; whilst the totara, another so-called pine (for they are none of them Coniferæ), is easily worked both green and dry. There is also the rata, "that wonderful vegetable production forming itself out of numberless vines, which first receive their support from some full-grown tree, then enclose it in a deadly embrace, and gradually expel the remains of their foster parent as their own growing demands for space require to be satisfied, then finally uniting themselves form a solid tree, with all the characteristics of bark, sap and heart, roots, trunk, and branch." This rata is almost the toughest wood known, and is used in many places for the cogs of wheels, &c. Besides these there are many others, especially the makia, which when thoroughly dry would turn or break the edge of the best axe ever produced in Sheffield, which are now only cut down for firewood as occasion requires.

THE GEOGNOSY OF THE APPALACHIANS AND THE ORIGIN OF CRYSTALLINE ROCKS *

IN coming before you this evening my first duty is to announce the death of Prof. William Chauvenet. This sad event was not unexpected, since, at the time of his election to the presidency of the Association, at the close of our meeting at Salem in August 1869, it was already feared that failing health would prevent him from meeting with us at Troy, in 1870. This, as you are aware, was the case, and I was therefore called to preside over the Association in his stead. In the autumn of 1869, he was compelled by illness to resign his position of Chancellor of the Washington University of St. Louis, and in December last died at the age of fifty years, leaving behind him a record to which Science and his country may point with just pride. During his connection of fourteen years with the Naval Academy at Annapolis he was the chief instrument in building up that institution, which he left in 1859 to take the chair of Astronomy and Mathematics at St. Louis, where his remarkable qualities led to his selection, in 1862, for the post of Chancellor of the University, which he filled with great credit and usefulness up to the time of his resignation. It is not for me to pronounce the eulogy of Prof. Chauvenet, to speak of his profound attainments in astronomy and mathematics, or of his published works, which have already taken rank as classics in the literature of these sciences. Others more familiar with his field of labour may in proper time and place attempt the task. All who knew him can however join with me in testifying to his excellences as a man, an instructor, and a friend. In his assiduous devotion to scientific studies he did not neglect the more elegant arts, but was a skilful musician, and possessed of great general culture and refinement of taste. In his social and moral relations he was marked by rare elevation and purity of character, and has left to the world a standard of excellence in every relation of life which few can hope to attain.

In accordance with our custom it becomes my duty in quitting the honourable position of President, which I have filled for the past year, to address you upon some theme which shall be germane to the objects of the Association. The presiding officer, as you are aware, is generally chosen to represent alternately one of the two great sections into which the members of the Association are supposed to be divided, viz , the students of the natural-history sciences on the one hand, and of the physico-mathematical and chemical sciences on the other. The arrangement by which, in our organisation, geology is classed with the natural history division, is based upon what may fairly be challenged as a somewhat narrow conception of its scope and aims. While theoretical geology investigates the astronomical, physical, chemical, and biological laws which have presided over the development of our earth, and while practical geology or geognosy studies its natural history, as exhibited in its physical structure, its mineralogy and its palæontology, it will be seen that this comprehensive science is a stranger to none of the studies which are included in the plan of our Association, but rather sits like a sovereign, commanding in turn the services of all.

As a student of geology, I scarcely know with which section of the Association I should to-day identify myself. Let me endeavour rather to mediate between the two, and show you somewhat of the two-fold aspect which geological science presents, when viewed respectively from the stand-points of natural history and of chemistry. I can hardly do this better than in the discussion of a subject which for the last generation has afforded some of the most fascinating and perplexing problems for our geological students; viz., the history of the great Appalachian mountain chain. Nowhere else in the world has a mountain system of such geographical extent and such geological complexity been studied by such a number of zealous and learned investigators, and no other, it may be confidently asserted, has furnished such vast and important results to geological science. The laws of mountain structure, as revealed in the Appalachians by the labours of the brothers Henry D. and William B. Rogers, of Lesley and of Hall, have given to the world the basis of a correct system of orographic geology,† and many of the obscure geological problems of Europe become plain when read in the light of our American experience. To discuss even in the most summary manner all of the questions which the theme suggests, would be a task too long for the present occasion, but I shall endeavour to-night in the first place to bring before you certain facts in the history of the physical structure, the mineralogy and the palæontology of the Appalachians; and in the second place to discuss some of the physical, chemical, and biological conditions which have presided over the formation of the ancient crystalline rocks that make up so large a portion of our great eastern mountain system.

* Address of Prof. T. Sterry Hunt on retiring from the office of President of the American Association for the Advancement of Science; abridged from the "American Naturalist."

† Amer. Jour. Sci., II. xxx. 406.

I. THE GEOGNOSY OF THE APPALACHIAN SYSTEM.—The age and geological relations of the crystalline stratified rocks of eastern North America have for a long time occupied the attention of geologists. A section across northern New York, from Ogdensburg on the St. Lawrence to Portland in Maine, shows the existence of three distinct regions of unlike crystalline schists. These are the Adirondacks to the west of Lake Champlain, the Green Mountains of Vermont, and the White Mountains of New Hampshire. The lithological and mineralogical differences between the rocks of these three regions are such as to have attracted the attention of some of the earlier observers. Eaton, one of the founders of American geology, at least as early as 1832, distinguished in his Geological Text-book (2nd edition) between the gneiss of the Adirondacks and that of the Green Mountains. Adopting the then received divisions of primary, transition, secondary and tertiary rocks, he divided each of these series into three classes, which he named carboniferous, quartzose, and calcareous; meaning by the first schistose or argillaceous strata such as, according to him, might include carbonaceous matter. These three divisions in fact corresponded to clay, sand, and lime-rocks, and were supposed by him to be repeated in the same order in each series. This was apparently the first recognition of that law of cycles in sedimentation upon which I afterwards insisted in 1863.* Without, so far as I am aware, defining the relations of the Adirondacks, he referred to the lowest or carboniferous division of the primary series the crystalline schists of the Green Mountains, while the quartzites and marbles at their western base were made the quartzose and calcareous divisions of this primary series. The argillites and sandstones lying still farther westward, but to the east of the Hudson River, were regarded as the first and second divisions of the transition series, and were followed by its calcareous division, which seems to have included the limestones of the Trenton group; all of these rocks being supposed to dip to the westward, and away from the central axis of the Green Mountains. Eaton does not appear to have studied the White Mountains, or to have considered their geological relations. They were, however, clearly distinguished from the former by C. T. Jackson in 1844, when, in his report on the geology of New Hampshire, he described the White Mountains as an axis of primary granite, gneiss, and mica-schist, overlaid successively, both to the east and west, by what were designated by him Cambrian and Silurian rocks; these names having, since the time of Eaton's publication, been introduced by English geologists. While these overlying rocks in Maine were unaltered, he conceived that the corresponding strata in Vermont, on the western side of the granitic axis, had been changed by the action of intrusive serpentines and intrusive quartzites, which had altered the Cambrian into the Green Mountain gneiss, and converted a portion of the fossiliferous Silurian limestones of the Champlain valley into white marbles.† Jackson did not institute any comparison between the rocks of the White Mountains and these of the Adirondacks; but the Messrs. Rogers in the same year, 1844, published an essay on the geological age of the White Mountains, in which, while endeavouring to show their Upper Silurian age, they speak of them as having been hitherto regarded as consisting exclusively of various modifications of granitic and gneissoid rocks, and as belonging "to the so-called primary periods of geologic time."‡ They however considered that these rocks had rather the aspect of altered palæozoic strata, and suggested that they might be, in part at least, of the age of the Clinton division of the New York system; a view which was supported by the presence of what were at the time regarded by the Messrs. Rogers as organic remains. Subsequently, in 1847,§ they announced that they no longer considered these to be of organic origin, without however retracting their opinion as to the palæozoic age of the strata. Reserving to another place in my address the discussion of the geological age of the White Mountain rocks, I proceed to notice briefly the

* Amer. Jour. Sci., II. xxxv. 166.

† Geology of New Hampshire, 160-162.

‡ Amer. Jour. Sci., II. i. 411.

§ Ibid, II. v. 116.

distinctive characters of the three groups of crystalline strata just mentioned, which will be shown in the sequel to have an importance in geology beyond the limits of the Appalachians.

1. *The Adirondack or Laurentide Series*—The rocks of this series, to which the name of the Laurentian system has been given, may be described as chiefly firm granitic gneisses, often very coarse-grained, and generally reddish or grayish in colour. They are frequently hornblendic, but seldom or never contain much mica, and the mica-schist (often accompanied with staurolite, garnet, andalusite, and cyanite), so often characteristic of the White Mountain series, are wanting among the Laurentian rocks. They are also destitute of argillites, which are found in the other two series. The quartzites, and the pyroxenic and hornblendic rocks, associated with great formations of crystalline limestone, with graphite, and immense beds of magnetic iron ore, give a peculiar character to portions of the Laurentian system.

2 *The Green Mountain Series.*—The quartzo-feldspathic rocks of this series are to a considerable extent represented by a fine-grained petrosilex or eurite, though they often assume the form of a true gneiss, which is ordinarily more micaceous than the typical Laurentian gneiss. The coarse-grained, porphyritic, reddish varieties common to the latter are wanting to the Green Mountains, where the gneiss is generally of pale greenish and grayish hues. Massive stratified diorites, and epidotic and chloritic rocks, often more or less schistose, with steatite, dark-coloured serpentines and ferriferous dolomites and magnesites, also characterise this gneissic series, and are intimately associated with beds of iron ore, generally a slaty hematite, but occasionally magnetite. Chrome, titanium, nickel, copper, antimony, and gold are frequently met with in this series. The gneisses often pass into schistose micaceous quartzites, and the argillites, which abound, frequently assume a soft, unctuous character, which has acquired for them the name of talcose or nacreous slates, though analysis shows them not to be magnesian, but to consist essentially of a hydrous micaceous mineral. They are sometimes black and graphitic.

3. *The White Mountain Series.*—This series is characterised by the predominance of well-defined mica-schists, interstratified with micaceous gneisses. These latter are ordinarily light-coloured from the presence of white feldspar, and though generally fine in texture, are sometimes coarse-grained and porphyritic. They are less strong and coherent than the gneisses of the Laurentian, and pass, through the predominance of mica, into mica-schists, which are themselves more or less tender and friable, and present every variety, from a coarse gneiss-like aggregate down to a fine-grained schist, which passes into argillite. The micaceous schists of this series are generally much richer in mica than those of the preceding series, and often contain a large proportion of well-defined crystalline tables belonging to the species muscovite. The cleavage of these micaceous schists is generally, if not always, coincident with the bedding, but the plates of mica in the coarser-grained varieties are often arranged at various angles to the cleavage and bedding-plane, showing that they were developed after sedimentation, by crystallisation in the mass, a circumstance which distinguishes them from rocks derived from the ruins of these, which are met with in more recent series. The White Mountain rocks also include beds of micaceous quartzite. The basic silicates in this series are represented chiefly by dark-coloured gneisses and schists, in which hornblende takes the place of mica. These pass occasionally into beds of dark hornblende-rock, sometimes holding garnets. Beds of crystalline limestone occasionally occur in the schists of the White Mountain series, and are sometimes accompanied by pyroxene, garnet, idocrase, sphene, and graphite, as in the corresponding rocks of the Laurentian, which this series, in its more gneissic portions, closely resembles, though apparently distinct geognostically. The limestones are intimately associated with the highly micaceous schists, containing staurolite, andalusite, cyanite, and garnet. These schists are sometimes highly plumbaginous, as seen in the graphitic mica-schist holding garnets in Nelson, New Hampshire, and that associated with cyanite in Cornwall, Conn. To this third series of crystalline schists belong the concretionary granitic veins abounding in beryl, tourmaline, and lepidolite, and occasionally containing tinstone and columbite. Granitic veins in the Laurentian gneisses frequently contain tourmaline, but have not, so far as is yet known, yielded the other mineral species just mentioned.*

II. THE ORIGIN OF CRYSTALLINE ROCKS.—We now approach the second part of our subject, namely, the genesis of the crystalline schists. The origin of the mineral silicates, which make up a great portion of the crystalline rocks of the earth's surface, is a question of much geological interest, which has been to a great degree overlooked. The gneisses, mica-schists, and argillites, of various geological periods do not differ very greatly in chemical constitution from modern mechanical sediments, and are now very generally regarded as resulting from a molecular re arrangement of similar sediments formed in earlier times by the disintegration of previously existing rocks not very unlike them in composition; the oldest known formations being still composed of crystalline stratified deposits presumed to be of sedimentary origin. Before these the imagination conceives yet earlier rocks, until we reach the surface of unstratified material, which the globe may be supposed to have presented before water had begun its work. It is not, however, my present plan to consider this far off beginning of sedimentary rocks, which I have elsewhere discussed.*

Apart from the clay and sand-rocks just referred to, whose composition may be said to be essentially quartz and aluminous silicates, chiefly in the forms of feldspars and micas, or the results of their partial decomposition and disintegration, there is another class of crystalline silicated rocks which, though far less important in bulk than the last, is of great and varied interest to the lithologist, the mineralogist, the geologist, and the chemist. The rocks of this second class may be defined as consisting in great part of the silicates of the protoxyd bases, lime, magnesia, and ferrous oxyd, either alone, or in combination with silicates of alumina and alkalies. They include the following as their chief constituent mineral species:—Pyroxene, hornblende, olivine, serpentine, talc, chlorite, epidote, garnet, and triclinic feldspars, such as labradorite. The great types of this second class are not less well defined than the first, and consist of pyroxenic and hornblendic rocks, passing into diorites, diabases, ophiolites and talcose, chloritic and epidotic rocks. Intermediate varieties resulting from the association of the minerals of this class with those of the first, and also with the materials of non-silicated rocks, such as limestones and dolomites, show an occasional blending of the conditions under which these various types of rocks were formed.

The distinctions just drawn between the two great divisions of silicated rocks are not confined to stratified deposits, but are equally well marked in eruptive and unstratified masses, among which the first type is represented by trachytes and granites, and the second by dolerites and diorites. This fundamental difference between acid and basic rocks, as the two classes are called, finds its expression in the theories of Phillips, Durocher, and Bunsen, who have deduced all silicated rocks from two supposed layers of molten matter within the earth's crust, consisting respectively of acid and basic mixtures; the trachytic and pyroxenic magmas of Bunsen. From these, by a process of partial crystallisation and eliquation, or by commingling in various proportions, those eruptive rocks which depart more or less from the normal types are supposed by the theorists of this school to be generated.† The doctrine that these eruptive rocks are not derived directly from a hitherto uncongealed nucleus, but are softened and crystallised sediments, in fact that the whole of the rocks at present known to us have at one time been aqueous deposits has, however, found its advocates. In support of this view, I have endeavoured to show that the natural result of forces constantly in operation tends to resolve the various igneous rocks into two classes of sediments, in which the two types are, to a great extent, preserved. The mechanical and chemical agencies which transform the crystalline rocks into sediments, separate these more or less completely into coarse, sandy, permeable beds on the one hand, and fine clayey impervious muds on the other. The action of infiltrating atmospheric waters on the first and more silicious strata, removes from them lime, magnesia, iron-oxyd, and soda, leaving behind silica, alumina, and potash—the elements of granitic, gneissic, and trachytic rocks. The finer and more aluminous sediments, including the ruins of the soft and easily abraded silicates of the pyroxene group, resisting the penetration of the water, will, on the contrary, retain their alkalies, lime, magnesia, and iron, and thus will have the composition of the more basic rocks.‡

A little consideration will, however, show that this process, although doubtless a true cause of differences in the composition of

* Hunt, Notes on Granitic Rocks; Amer. Jour. Sci. III. i. 182.

* Amer. Jour. Sci., II. I. 25.
† Hunt on Some Points of Chemical Geology, Quar. Jour. Geol. Soc., XV. 489.
‡ Quar. Jour. Geol. Soc., XV. 489; also, Amer. Jour. Sci., II. xxx. 133.

sedimentary rocks, is not the only one, and is inadequate to explain the production of many of the varieties of stratified silicated rocks, such are serpentine, steatite, hornblende, diallage, chlorite, pinite, and labradorite, all of which mineral species form rock masses by themselves, frequently almost without admixture. No geological student will now question that all of these rocks occur as members of stratified formations. Moreover, the manner in which serpentines are found interstratified with steatite, chlorite, argillite, diorite, hornblende, and feldspar rocks, and these, in their turn, with quartzites and orthoclase rocks, is such as to forbid the notion that these various materials have been deposited, with their present composition, as mechanical sediments from the ruins of pre-existing rocks; a hypothesis as untenable as that ancient one which supposed them to be the direct results of plutonic action.

There are, however, two other hypotheses which have been proposed to explain the origin of these various silicated rocks, and especially of the less abundant, and, as it were, exceptional species just mentioned. The first of these supposes that the minerals of which they are composed have resulted from an alteration of previously existing minerals, often very unlike in composition to the present, by the taking away of certain elements and the addition of certain others. This is the theory of metamorphism by pseudomorphic changes, as they are called, and is the one taught by the now reigning school of chemical geologists, of which the learned and laborious Bischof, whose recent death science deplores, may be regarded as the great exponent. The second hypothesis supposes that the elements of these various rocks were originally deposited as, for the most part, chemically formed sediments, or precipitates; and that the subsequent changes have been simply molecular, or, at most, confined in certain cases to reactions between the mingled elements of the sediments, with the elimination of water and carbonic acid. It is proposed to consider briefly these two opposite theories, which seek to explain the origin of the rocks in question respectively by pseudomorphic changes in pre-existing crystalline rocks, and by the crystallisation of aqueous sediments, for the most part chemically-formed precipitates.

Mineral pseudomorphism, that is to say, the assumption by one mineral substance of the crystalline form of another, may arise in several ways. First of these is the filling up of a mould left by the solution or decomposition of an imbedded crystal, a process which sometimes takes place in mineral veins, where the processes of solution and decomposition can be freely carried on. Allied to this, is the mineralisation of organic remains, where carbonate of lime or silica, for example, fills the pores of wood. When subsequent decay removes the woody tissue, the vacant spaces may, in their turn, be filled by the same or another species.* In the second place, we may consider pseudomorphs from alteration, which are the result of a gradual change in the composition of a mineral species. This process is exemplified in the conversion of feldspar into kaolin by the loss of its alkali and a portion of silica, and the fixation of water, or in the change of chalybite into limonite by the loss of carbonic acid and the absorption of water and oxygen.

The doctrine of pseudomorphism by alteration as taught by Gustav Rose, Haidinger, Blum, Volger, Rammelsberg, Dana, Bischof, and many others, leads them, however, to admit still greater and more remarkable changes than these, and to maintain the possibility of converting almost any silicate into any other. Thus, by referring to the pages of Bischof's *Lehrbuch der Geognosie*, it will be found that serpentine is said to exist as a pseudomorph after augite, hornblende, olivine, chondodrite, garnet, mica, and probably also after labradorite, and even orthoclase. Serpentine rock or ophiolite is supposed to have resulted, in different cases, from the alteration of hornblende-rock, diorite, granulite, and even granite. Not only silicates of protoxyds and aluminous silicates are conceived to be capable of this transformation, but probably also quartz itself; at least, Blum asserts that meerschaum, a closely related silicate of magnesia, which sometimes accompanies serpentine, results from the alteration of flint, while, according to Rose, serpentine may even be produced from dolomite, which we are told is itself produced by the alteration of limestone. But this is not all—feldspar may replace carbonate of lime, and carbonate of lime feldspar, so that, according to Volger, some gneissoid limestones are probably formed from gneiss by the substitution of calcite for orthoclase. In this way we are led from gneiss or granite to limestone, from limestone to dolomite, and from dolomite to serpentine, or more directly from granite, granulite, or diorite to serpentine at once, without passing through the intermediate stages of limestone and dolomite, till we are ready to exclaim in the words of Goethe:—

"Mich ängstiet das Verfängliche
Im widrigen Geschwätz,
Wo Nichts verharret, Alles flieht,
Wo schon verschwunden was man sieht,"

which we may thus translate:—"I am vexed with the sophistry in their contrary jargon, where nothing endures, but all is fugitive, and where what we see has already passed away."

By far the greater number of cases on which this general theory of pseudomorphism by a slow process of alteration in minerals, has been based, are, as I shall endeavour to show, examples of the phenomenon of mineral envelopment, so well studied by Delesse in his essay on pseudomorphs,* and may be considered under two heads:—first, that of symmetrical envelopment, in which one mineral species is so enclosed within the other that the two appear to form a single crystalline individual. Examples of this are seen when prisms of cyanite are surrounded by staurolite, or staurolite crystals completely enveloped in those of cyanite, the vertical axes of the two prisms corresponding. Similar cases are seen in the enclosure of a prism of red in an envelope of green tourmaline, of allanite in epidote, and of various minerals of the pyroxene group in one another. The occurrence of muscovite in lepidolite, and of margarodite in lepidomalene, or the inverse, are well known examples, and, according to Scheerer, the crystallisation of serpentine around a nucleus of olivine is a similar case. This phenomenon of symmetrical envelopment, as remarked by Delesse, shows itself with species which are generally isomorphous or homœomorphous, and of related chemical composition. Allied to this is the repeated alternation of crystalline laminæ of related species, as in perthite, the crystalline cleavable masses of which consist of thin alternating layers of orthoclase and albite.

Very unlike to the above are those cases of envelopment in which no relations of crystalline symmetry nor of similar chemical constitution can be traced. Examples of this kind are seen in garnet crystals, the walls of which are shells, sometimes no thicker than paper, enclosing in different cases crystalline carbonate of lime, epidote, chlorite, or quartz. In like manner, crystalline shells of leucite enclose feldspar, hollow prisms of tourmaline are filled with crystals of mica or with hydrous peroxyd of iron, and crystals of beryl with a granular mixture of orthoclase and quartz, holding small crystals of garnet and tourmaline, a composition identical with the enclosing granitic veinstone.† Similar shells of galenite and of zircon, having the external forms of these species, are also found filled with calcite. In many of these cases the process seems to have been first the formation of a hollow mould or skeleton crystal (a phenomenon sometimes observed in salts crystallising from solutions), the cavity being sometimes filled with other matters. Such a process is conceivable in free crystals found in veins, as for example, galenite, zircon, tourmaline, beryl, and some examples of garnet, but is not so intelligible in the case of those garnets imbedded in mica-schist, studied by Delesse, which enclosed within their crystalline shells irregular masses of white quartz, with some little admixture of garnet. Delesse conceives these and similar cases to be produced by a process analogous to that seen in the crystallisation of calcite in the Fontainebleau sandstone; where the quartz grains, mechanically enclosed in well-defined rhombohedral crystals, equal, according to him, sixty-five per cent. of the mass. Very similar to these are the crystalloids with the form of orthoclase, which sometimes consist in large part of a granular mixture of quartz, mica, and orthoclase, with a little cassiterite, and in other cases, contain two thirds their weight of the latter mineral, with an admixture of orthoclase and quartz. Crystals with the form of scapolite, but made up, in a great part, of mica, seem to be like cases of envelopment, in which a small proportion of one substance in the act of crystallisation, compels into its own crystalline form a large portion of some foreign material, which may even so mask the crystallising element that this becomes overlooked, as of secondary importance. The substance which, under the name of houghite, has been described as an altered spinel, is found by analysis to be the mixture of völlknerite with a variable proportion of spinel, which in some specimens, does not exceed eight per cent., but to which, nevertheless, these crystalloids appear to owe their more or less complete octohedral form.‡

(*To be continued.*)

* Hunt on the Silification of Fossils, Canadian Naturalist, N. S., I. 46.

* Annales des Mines, V. xvi. 317-392.

† Report Geol. Survey of Canada, 1866, p. 189.

‡ Rpt. Geol. Sur. of Canada, 1866, pp. 189, 213. Amer Jour. Sci, III. i. 183.

INSTRUCTIONS FOR OBSERVERS, AT THE ENGLISH GOVERNMENT ECLIPSE EXPEDITION, 1871

II.—Polariscopic Observations

THE chief points to which observers of polarisation should direct their attention appear to be :—

A. What is the nature of the outlying corona ?

B. Can the radial polarisation of the circumsolar corona be traced down to the photosphere, or, if not, how low ?

C. Is secondary atmospheric polarisation traceable ? and if so, does the plane change during totality ?

A. We might suppose this to be due—

(1) to circumsolar matter (though at a great distance from the sun) reflecting light,

(2) to circumsolar matter in the state of self-luminous gas,

(3) to circumlunar matter diffracting and, to a certain extent, reflecting light (most improbable),[1]

(4) to lofty atmospheric haze or cloud, of excessive tenuity, diffracting light.

The light ought to be, for

(1) strongly and radially polarised,

(2) unpolarised,

(3 and 4) insensibly or all but insensibly polarised.

Hence polarisation observations would only serve to discriminate between (1) on the one hand, and (2), (3), or (4) on the other.

From the faintness of the object and its considerable extent, the naked eye, armed with a polariscope, might be best. If a telescope be used, it should be of quite low power, and the aperture as large as the breadth of the pupil multiplied by the magnifying-power.

Suppose the polariscope be Savart's, the quartz plates being thick enough (if the naked eye be used) to give bands as narrow as, say, 20′ diameter.

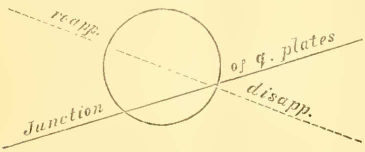

Fig. 1.

Let the observer rotate the polariscope till the bands, if any, seen on the dark moon disappear ; then, *without rotating the instrument round its axis*, let him *incline* the axis so as to point at the outlying corona in different directions round the sun, and notice whether the bands spring into existence ; and if so, let him sweep *round* the sun, noticing what lies *outside* the clearly circumsolar corona of 5′ or so height, and let him notice particularly by estimation the direction, relatively to the bands, of the radius vector of the region where they are most vivid, or, better, the azimuth of both radius and bands. He should also specify, *provided he can do so with certainty*, whether the bands were black-centred or white-centred. He should also state in his account, and verify the statement by an observation made at leisure before or after totality, whether his Savart is constructed (or set) so as to have the bands *parallel or perpendicular* to the principal plane of the Nicol.

A very useful adjunct to a Savart's polariscope would be a glass reflector, or else a tourmaline, placed so as to cover a small segment of the field of view near the edge. On account of the possible difficulty of illuminating the reflector in the peculiar circumstances of a total eclipse, a tourmaline would seem to be preferable. It should be placed for the naked eye at the least distance of distinct vision—for a telescope, in or in front of the eye-piece, where a real image is formed so as to be seen distinctly—the axis of the tourmaline being parallel to the edge or chord of the segment, and the bands being set perpendicular to this chord. In the event of rotation during the observation, the whole should be rotated together. The question whether the bands are bright-centred or dark-centred, which, in the case of slight polarisation, is difficult to decide, would thus be replaced by the simpler question, whether the bands in the field were of the same character as in the segment (*i.e.*, bright being a prolongation of bright, and dark of dark) or of opposite character.

The observer should previously have practised on the blue sky, rotating his Savart till the bands disappear, and noticing to what degree they are brought back by small changes of pointing without rotation, so as to be prepared for what he is liable to from secondary atmospheric polarisation during totality.

Should only very feeble bands be seen in the outer corona, such as might possibly be attributable to atmospheric polarisation operating through small changes of pointing, it would be well for control to rotate the instrument *a little* till bands are fairly visible on the disc of the moon, and notice whether on passing to the outer corona, *in whatever direction*, the bands, instead of being reinforced, tend rather to be drowned in white light. Should luminous beams or dark rifts be seen in the outer corona, so as to exhibit contrast of light and shade in close proximity, a good opportunity will be afforded of testing whether the light of the outer corona is polarised or not. If it be polarised, then on rotating the Savart, so as to make the bands cut at various indications the boundary of light and shade, the bands will in certain azimuths of the Savart be stronger on the luminous than on the dark side of the edge of the beam or rift. If it be unpolarised, then, whatever be the azimuth of the Savart, the bands will be rather drowned in white light than reinforced on passing from the dark to the luminous side of the edge.

But Savart's and other colour-polariscopes, which are admirable for detecting a slight polarisation in light which is not particularly feeble, break down when the difficulty arises from the feebleness of the light rather than the slightness of the polarisation. In such cases a simple double-image prism, with a diaphragm-tube, is better. Unless those who have seen total eclipses can decide from trial (suppose on the clear sky after sunset, or at night when illuminated by the moon), combined with

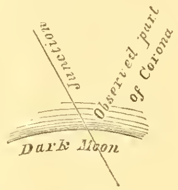

Fig. 2.

their memory of the degree of illumination of the outer corona, it might be well that the observer should be provided with and should try both instruments.

B. For this a telescope will be required with a magnifying power of, say, 16 or 20. A biquartz seems the best instrument, placed at the common focus of the eye-piece (which should be positive) and objective, and combined with a Nicol's prism, or, if it can be procured, a thoroughly good tourmaline. A tourmaline might be placed over the eye-hole, whereas a Nicol might have to be placed in the body of the eye-piece, which, however, is no particular disadvantage if properly done.

Let it be ascertained by previous trial how much a Nicol must be turned from the position in which the two halves are purple alike to make the tints contrast more vividly. Say it is 30°. Suppose the observer on the line of central shadow, so that the limits of disappearance and reappearance will be on opposite ends of a diameter. The biquartz and Nicol have been relatively set so that the line of junction is in the plane of polarisation of light extinguished by the Nicol, turn them together before totality 30° (or whatever other angle may have been fixed on) to either side of the diameter of disappearance, and, pointing the telescope to the place of disappearance (Fig. 1), await totality without dazzling the eye. The moment the sun is covered, apply the eye to the telescope, and notice whether there is a vivid contrast of colour right and left of the line of junction of the quartz plates *all the way down to the dark moon* (Fig. 2), or only in the higher parts of the circumsolar corona.

Be ready to repeat the observation before reappearance, with the telescope pointed to the place of reappearance : and meanwhile, if time permits, repeat Prazmouski's observation by pointing the telescope, without rotation of the analyser, so that the line of junction bisects the moon, and noticing whether the semicircles of the corona are purple alike where they abut on the junction, and what is the order of colours in the semicircle on receding from the junction. A record as to which is which of the two halves of the biquartz should be carefully preserved.

Should secondary atmospheric polarisation be so strong as to throw doubt on the results (which may be judged of by noticing the light on the dark moon), it would be well to rotate the analyser till the two halves seen on the dark moon are purple alike, and then alter the pointing of the telescope, and repeat Prazmouski's observation.

It will be observed that the same general principles apply to the elimination of atmospheric polarisation, whether the polariscope employed be a Savart's polariscope, a polariscope with quartz wedges, or a biquartz polariscope.

C. This is of little intrinsic interest, its chief use being to clear up possible doubts as to the results obtained by the observers of A and B. Should there be an observer not otherwise employed, he might be deputed to observe the direction of the Savart's bands on disappearance, both on the dark moon and the surrounding sky, and whether this direction changes during totality. Also it should be specified in which pair of opposite quadrants they were black-centred and in which white-centred. Should this be found impossible or uncertain (the instrument being unprovided with the adjunct mentioned above), the Savart might be used as a simple Nicol by turning it end for end, so that the quartz plates are next the eye; and with this the plane of polarisation might be roughly determined by means of the azimuth of the principal plane of the Nicol when the light most nearly disappears.

Should registration of the azimuth be attempted, the Savart would be fixed so as not to be reversible. In that case the observer might be provided with a double-image prism and diaphragm-tube for separate use in case of need.

Stoppage of stray light in a telescope designed for polarisation

The want of this appears to have occasioned some difficulty at the last eclipse.

The simplest way is by a stop, with a hole just large enough to contain the image of the object-glass. Such exists in the erecting eye-piece, where an image of the object-glass is formed in the body of the eye-piece. It exists too, in a Gregorian or Cassegrainian telescope, where the stoppage is imperative. But in an ordinary refracting telescope, with an inverting eye-piece, the eye-hole (from certain motives of convenience) is larger than in front of (*i. e.* nearer the object-glass than) the bright circle, or image of the object-glass; and unless the tube is sufficiently provided with stops, when a faint object near a bright one is looked at, light from the bright, reflected from the inside of the tube, is liable to enter the field of view. Large instruments are provided with stops; but I fancy smaller instruments are sometimes turned out without them. This should be looked to.

The observer may test the correctness of stopping by taking out the eye-piece, inserting a paper disc with a central hole of the size of the field-glass, turning the instrument nearly but not quite to a bright object, as well as to points more distant from the bright object, and noticing whether the side of the tube, even when viewed in a direction grazing the edge of the hole, is properly dark, so that only the edges of the stops are seen.* On the other hand, the stops should not obstruct a clear view of the object-glass as seen through the hole representing the field-glass, or they will render the outer portions of the object-glass useless.

General Remarks

I consider the observation recommended by Mr. Ranyard (see NATURE, Aug. 24, 1871), very important, IF, after what Prazmouski and Ranyard have done, the point be still deemed doubtful. Prazmouski's observation seems to have been beautifully devised and executed, but carelessly described. It is only by conjecture that I can make sense and harmony with what is known, out of his observations as described by himself. But I think that Mr. Ranyard has at least shown that our conjectural interpretation of Prazmouski's observation is the right one; and if so, the point seems settled.

It is for this reason that, in lieu of No. 3, first half, I proposed something new. What becomes of the magnesium, &c., which the spectroscope reveals low down in the gigantic puffs which the sun emits? The hydrogen must surely carry the magnesium, &c., with it to the higher regions, though the magnesium, &c., would soon be condensed, and so would not be detected by the spectroscope. These substances would exist in the form of an exceedingly fine haze or dust. I use the two words, "haze" to denote a filmy cloud of molten "dust" of solid matter. This haze or dust is capable of detection, and, according to my interpretation, *has* been detected, by polarisation; and it is interesting to know how low down it can be detected. Mr. Stoney's speculations as to layers are utterly inapplicable here, as they imply a state of tranquillity quite unlike what we now know to exist, at any rate in connexion with the puffs.

I don't know why, in the second half of No. 3, Mr. Ranyard prescribes placing the line of junction *across* a sector or rift, *if* by that he means *turning* the eye-piece carrying the quartz plates so that the line is perpendicular with the corona to the sector. It would be more likely to yield results if it cut it obliquely, as represented for the corona in Fig. 2. But probably he only means pointing the telescope so that the junction cuts the rift. If the observer notices contrasting colours, he may *then* proceed to determine the plane of polarisation. G. G. S.

* If reflection occurs from the part of the tube so near the eye as not to appear *within* the field, it will not signify much.

SCIENTIFIC SERIALS

THE *Journal of the Quekett Microscopical Club.* No. 16. October 1871. "Microscopic Work and Conjectural Science," being the address of the President (Lionel S. Beale, M.B., F.R.S.), for the year 1871. This address is chiefly occupied in combating the method, presumed to have been adopted, of depreciating one kind of scientific investigation in order to elevate another, and attacks without ceremony those who would elevate physical science to the disparagement of microscopical observation.—"On the Examination of the Surface Markings of Diatoms by the Oxy-calcium Light," by N. E. Green. The writer of this paper details his examination of such diatoms as Isthmia, Biddulphia, Triceratium, Pleurosigma, &c., as opaque objects by high powers, as one-sixth Ross and one-twelfth Gundlach, through the agency of the oxy-calcium light. The conclusion at which he has arrived is, that the markings on all the above, except Pleurosigma, resemble "craters," the surface "being studded with rows of small shallow craters, the sharp edges of which projected slightly above, while the centres seemed to be below the surface." In Pleurosigma a different structure of the surface was observed. "The lime light brought out most distinctly the bead-like character of its markings; they stood out in bold relief like rows of Indian corn."—The Inaugural Address of the South London Microscopical and Natural History Club, by R. Braithwaite, M.D., F.L.S., is principally devoted to suggestions on the vast field for observation at the disposal of the microscopist.—"On Nucleated Sporidia," by M. C. Cooke, M.A. After describing the general structure which prevails in the genus Peziza of Ascomycetous Fungi, the writer details his method of mounting sections for the microscope in pure glycerine. The nucleated sporidia, so prevalent in this genus, are affirmed to be so affected by this method that in a short time all traces of the nuclei are lost, and the object of the paper is to indicate the doubtful value of nucleated sporidia in specific characters. The true nature of such nuclei and their uses are said to be obscure.

IN the *Revue Scientifique*, Nos. 13—18, are many valuable articles. Further reports are given of the proceedings of the Edinburgh meeting of the British Association, and a translation of Prof. T. Sterry Hunt's address to the Indianopolis meeting of the American Association. We have also a memoir of M. Lartet by M. G. de Mortillet; Helmholtz's paper on the rapidity of propagation of electro-dynamical actions; report of M. Chauveau's lectures on the physiology of virulent maladies; a lecture by M. Claude Bernard on the method and principle of physiology; a translation of P. Secchi's paper on the solar protuberances from the *Atti dell' Academia ponitificia de nuovi Lincei*; a biographical sketch of Haidinger by M. Fouqué; reports of the proceedings of the various scientific institutions in France and Belgium; and translations of lectures delivered at the Royal Institution, University of Edinburgh, &c., by Prof. Tyndall, Dr. Carpenter, Dr. Laycock, and others.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, October 23.—The greater part of the communications read at this meeting were devoted to chemical subjects. Of mathematical papers only one was presented—namely, a continuation of M. Chasles' memoir on the determination of a series of groups of a certain number of points on a geometrical curve.—A note was read by M. J. Bertrand on the mathematical theory of dynamical electricity, and a memoir by M. E. Mathieu on the integration of equations to the partial differences of mathematical physics.—M. du Moncel presented some observations relating to a recent communication by M. Ruhmkorff upon some experiments in magneto-electric induction, in which he claimed to have already ascertained and published facts analogous to those of the German author.—M. P. A. Favre read a continuation of his thermic researches upon the electrolysis of the hydracids.—A fifth letter from Father Secchi on the various aspects of the protuberances and other remarkable parts of the surface of the sun was read, in which he describes the results of simultaneous observations made by himself at Rome, and by M. Tacchini at Palermo.—M. Secchi also presented a note on a new method of observing the eclipses and passages of Venus, by means of a spectroscopic apparatus modified by having at a distance of about 20 centimetres in front of the spectroscope, an additional prism having its refringent angle parallel to the fissure.—The chemical papers were as follows:—a theory of simple reactions limited by inverse action, and an application of the same to the transformations of phosphorus, by M. J. Lemoine.—Researches in chemical statics, by M. Stas, containing a discussion of the phenomena which occur in the precipitation of dilute solutions of salts of silver by hydrochloric, hydrobromic, and hydriodic acids, and by chlorides, bromides, and iodides. This paper contains some results of great importance in the analysis of bodies containing silver.—The conclusion of the second part of M. Berthelot's investigation of the ammoniacal salts.—A note on the transformation of glucoses into monatomic and hexatomic alcohols, by M. G. Bouchardat, communicated by M. A. Wurtz. The author acts upon the glucoses by means of an amalgam of sodium. He describes its action upon glucose and sugar of milk.—A note on the hexabromide and hexachloride of silicium, by M. C. Friedel, also presented by M. A. Wurtz; and a note on the method of determining the gases evolved by an explosion of nitroglycerine, by M. L. L'Hote, presented by General Morin. From the researches of the last-mentioned author it appears that 1 gramme of nitroglycerine produces 284 cub. centim. of gas, containing by volume 45·72 of carbonic acid, 20·36 of binoxide of nitrogen, and 33·92 of nitrogen.—M. Elie de Beaumont called attention to some specimens of native phosphate of lime from Caylux and Cajare, and noticed the importance of these deposits for agricultural purposes. M. Combes also remarked upon this subject.—M. Chapelas presented a note on a remarkable meteor observed during the night of the 19th October.

PHILADELPHIA

Academy of Natural Sciences, May 9.—The President, Dr. Ruschenberger, in the chair. — Prof. Cope demonstrated some anatomical points of importance in the classification of some of the Siluroids of the Amazon, noticing first those which have no swimming bladder, but having the post-temporal bone pierced in a sieve-like manner, forming minute tympana; these he characterised as *Otocinclus*. Others having huge swim-bladders, gun-boat style of shape. No adipose fin; the back naked. No lyre plate, indicated as *Zathorax*. A third, body protected by bony shields above. No adipose fin; the scapular arch dermoössified and lyre-shaped below; indicated as *Physopyxis lyra* A fourth, shielded all over its sides, with the under lip turned back, genus *Corydoras*. A fifth, where the under lip is separated, except at the ends, forming loops, named *Brochis*. In the sixth, where the lips are separated from the beard distally forming chin beards, indicated as *Dianema*.

May 16.—Dr. Carson, Vice-President, in the chair.—"Remains of Mastodon and Horse in North Carolina."—Prof. Leidy exhibited two photographs, received from Prof. W. C. Kerr, State Geologist of North Carolina, representing some remains of *Mastodon americanus* found in that State. One of the specimens represented is that of the greater part of the lower jaw of a mature male, retaining both incisor tusks and the last two molar teeth. The latter, with their angular lobes separated by deep angular and nearly unobstructed valleys, are quite characteristic of the species. The incisors are an inch and three-fourths in diameter. The last molar has four transverse pairs of lobes and a well-developed heel. The penultimate molar has three transverse pairs of lobes. The specimen was obtained from gravel overlying the miocene marl near Goldsboro', Lenoir Co., N.C. An isolated last lower molar of the same species, represented in company with the jaw, was obtained in Pitt Co.—Prof. Leidy also exhibited a specimen of an upper molar teeth, which Mr. Timothy Conrad had picked up from a pile of miocene marl at Greenville, Pitt Co., N.C. He suspected, from its size and intricacy in the folding of the enamel of the isles at the middle of the triturating surface, that the tooth belonged to the post-pliocene *Equus complicatus*, and was an accidental occupant of the miocene marl. It may, however, belong to a Hipparion of the miocene period, but the imperfection of the specimen at its inner part prevented its positive generic determination.

BOOKS RECEIVED

ENGLISH.—A Manual of the Anatomy of Vertebrated Animals: Prof. Huxley (Churchill).—A Synonymic Catalogue of Diurnal Lepidoptera: W. F. Kirby (Van Voorst).—Description of an Electric Telegraph: Sir Francis Ronald (Williams and Norgate).—Spiritual and Animal Magnetism: Prof. J. G. Zerffi (Hardwicke).—An Elementary Treatise on Statics: J. W. Mulcaster (Taylor and Francis).

FOREIGN.—(Through Williams and Norgate.)—Verhandlungen des internationalen Congress für Alterthumskunde u. Geschichte zu Bonn.

DIARY

THURSDAY, NOVEMBER 2.

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.

CHEMICAL SOCIETY, at 8.—On Anthraflavic Acid: W. H. Perkin.

LONDON INSTITUTION, at 7.30.—On Michael Faraday; the Story of his Life: Dr. J. H. Gladstone, F.R.S.

FRIDAY, NOVEMBER 3.

GEOLOGISTS' ASSOCIATION, at 8.—On the Old Land Surfaces of the Globe Prof. Morris.

MONDAY, NOVEMBER 6.

LONDON INSTITUTION, at 4.—On Elementary Physiology (II.): Prof. Huxley, LL.D., F.R.S.

ANTHROPOLOGICAL INSTITUTE, at 8.—On the Order of Succession of the several Stone Implement Periods in England: J. W. Flower, F.G.S.—Notes on some Archaic Structures in the Isle of Man: A. L. Lewis.

TUESDAY, NOVEMBER 7.

SOCIETY OF BIBLICAL ARCHÆOLOGY, at 8.30.—On the Religious Belief of the Assyrians: H. Fox Talbot.

HACKNEY SCIENTIFIC ASSOCIATION, at 7.30.—Conversazione.

ZOOLOGICAL SOCIETY, at 9.—Report on Recent Additions to the Society's Menagerie: The Secretary.—On the Recent Ziphoid Whales, with a description of the Skeleton of *Berardius arnouxii*: W. H. Flower, F.R.S.—On the Habits of the Nose-horned Viper (*Vipera nasicornis*): Herbert Taylor Ussher, C.M.Z.S.

WEDNESDAY, NOVEMBER 8.

GEOLOGICAL SOCIETY, at 8.—Notes on the Diamond Gravels of the Vaal, in South Africa: G. W. Stow.—On the Geology of the Diamond Fields of South Africa: Dr. John Shaw.—Notes on some Fossils from the Devonian Rocks of the Witzenberg Flats, Cape Colony: Prof. T. Rupert Jones.

THURSDAY, NOVEMBER 9.

LONDON MATHEMATICAL SOCIETY, at 8.—On the Partition of an Even Number into two Primes: J. J. Sylvester, F.R.S.

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THURSDAY, NOVEMBER 9, 1871

THE ORIGIN OF GENERA *

ALTHOUGH it is now two years since the publication of Prof. Cope's "fragmentary essay," as he modestly terms it, bearing the above title, it may not be out of place, in the present stage of the theory of Evolution, to give our readers some idea of its scope. It ought to be in the possession of every naturalist. Although already so condensed that anything like an analysis of it is impossible, the following tabular sketch may serve to give our readers an idea of the mode in which the Origin of Genera is treated :—

I. Relations of allied genera.
  First ; in adult age.
  Second ; in relation to their development.
    α. On exact parallelism.
    β. On inexact or remote parallelism.
    γ. On parallelism in higher groups.
    δ. On the extent of parallelisms.

II. Of retardation and acceleration in generic characters.
  First ; metamorphoses in adult age.
    α. The developmental relations of generic and specific characters.
    β. Probable cases of transition.
    γ. Ascertained cases of transition.
  Second ; earlier metamorphoses.
    δ. The origin of inexact parallelisms.

III. Relations of higher groups.
    α. Of homologous groups.
    β. Of heterology.
    γ. Of mimetic analogy.

IV. Of natural selection.
    α. As affecting class and ordinal characters.
    β. As affecting family characters.
    γ. As affecting generic characters.
    δ. As affecting specific characters.
    ε. On metaphysical species.

V. Of epochal relations.

Professor Cope considers that the laws which have regulated the successive creation of organic beings are of two kinds. The first, that which has impelled matter to produce numberless ultimate types from common origins ; the second, that which expresses the mode or manner in which the first law has executed its course, from its commencement to its determined end, in the many cases before us.

"That a descent, with modifications, has progressed from the beginning of the creation is exceedingly probable. The best enumerations of facts and arguments in its favour are those of Darwin, as given in his various important works, 'The Origin of Species,' &c. There are, however, some views respecting the laws of development on which he does not dwell, and which it is proposed here to point out.

"In the first place, it is an undoubted fact that the origin of genera is a more distinct subject from the origin of species than has been supposed.

"A descent with modification involves continuous series of organic types through one or many geologic ages, and the co-existence of such parts of such various series at one time as the law of mutual adaptation may permit.

"These series, as now found, are of two kinds: the uninterrupted line of specific, and the same uninterrupted line of generic characters. These are independent of each other, and have not, it appears to the writer, been developed *pari passu*. As a general law, it is proposed to render highly probable that the same specific form has existed through a succession of genera, and perhaps in different epochs of geologic time.

"With regard to the first law of development as above proposed, no one has found means of discovering it, and perhaps no one ever will. It would answer such questions as this. What necessary coincidence of forces has resulted in the terminus of the series of fishes in the perches as its most specialised extreme? or, of the batrachia, in the fresh-water frogs, as its ultimum? or, of the thrushes, among birds, as their highest extreme? in a word, what necessity resulted in man as the crown of the mammalian series, instead of some other organic type? Our only answer and law for the questions must be, the will of the Creator.

"The second law of modes and means has been represented to be that of natural selection by Darwin. This is, in brief, that the will of the animal applied to its body in the search for means of subsistence and protection from injuries gradually produces those features which are evidently adaptive in their nature. That, in addition, a disposition to a general variation on the part of species has been met by the greater or less adaptation of the results of such variation to the varying necessities of their respective situations. That the result of such conflict has been the extinction of those types that are not adapted to their immediate or changed conditions, and the preservation of those that are" (pp. 4, 5).

In the chapter "On the relations of nearly allied genera," he gives no less than eight "examples of exact parallelism."* We select one at random as illustrating the large number of facts he brings to bear on the subject of which he treats. "The Cervidæ of the Old World are known to develop a basal snag of the antler at the third year; a majority of those of the New World never develop it, except in abnormal cases in the most vigorous maturity of the most Northern Cariacus: while the South American Subulo retains to adult age the simple horn of the second year of Cervus. Among the higher Cervidæ, Rusa and Axis never assume characters beyond an equivalent of the fourth year of Cervus. In Dama, on the other hand, the characters are assumed more rapidly than in Cervus; its third year corresponding to the fourth of the latter. Among American deer there is the Blastocerus, whose antlers are identical with those of the fourth year of Cariacus.

"Now, individuals of the genus Cervus of the second year do not belong to Subulo, because they have not as yet their mature dentition. Rusa, however, is identical with those Cervi whose dentition is complete before they gain the antlers of the fifth year. When the first trace of a snag appears on one beam of *Cariacus virginianus*, the

* "On the Origin of Genera." By Edward D. Cope, A.M., Corresponding Secretary of the Academy of Natural Sciences of Philadelphia. Pp. 80. 1869. (Philadelphia: Merrihew and Son. London: Trübner and Co.)

* The author applies the term *exact parallelism* to the relation of genera which are simply steps in one and the same line of development; while *incomplete parallelism* is applied to that of those where one or more characters intervene in the maturity of either the lower or higher genera to destroy identity.

dentition includes the full number, but there remain $\frac{1}{3}$ milk molars much worn and ready to be shed. Perhaps the snag is developed before these are displaced. If so the Cariacus is never a Subulo; but there can be little doubt that the young Blastocerus belongs to that genus before its adult characters appear."

From the examples of inexact parallelism we select the second and eighth.

"In both perissodactylous and artiodactylous mammalia certain types develop their family character of canines at the earliest appearance of dentition; others, not till a comparatively late period of life (Equus); and the extreme genera never produce them" (p. 14).

"In most serpents the left lung is never developed; in such the pulmonary artery, instead of being totally wanting, remains as a posterior aorta bow, connected with the aorta by a ductus botalli; serpents without left lung being, therefore, identical in this respect with the embryonic type of those in which that lung exists."

Under the head of "adult metamorphoses," in the second chapter, Prof. Cope explains his law of retardation and acceleration. It consists "in a continual crowding backwards of the successive steps of individual development, so that the period of reproduction, while occurring periodically with the change of the year, falls later and later in the life-history of the species, conferring upon its offspring features in advance of those possessed by its predecessors. This progressive crowding back of stages is not, however, supposed to have progressed regularly On the contrary, in the development of all animals, there are well-known periods when the most important transitions are accomplished in an incredibly short space of time (as the passage of man through the stages of the aorta bows and the production of limbs in the Batrachia Anura); while other transitions occupy long periods, and apparently little progress is made" (p. 37).

On these and other similar grounds, the author concludes, that "the transformation of genera may have been rapid and abrupt, and the intervening periods of persistency very long. As the development of the individual, so the development of the genus" (p. 38).

To the question—Has any such transition from genera to genera ever been seen to occur? Prof. Cope answers in the affirmative, and gives eleven probable and six ascertained cases, for the details of which we must refer to pp. 42—46.

Passing for want of space over the third and fourth chapters, we arrive at the concluding one, "On Epochal Relations, or those Measuring Geological Time," which abounds in valuable matter. The comparisons of different faunæ "indicate that an inherent difference between the types of a continent exists at the present time, though the difference is subordinated to a universal distribution of the higher groups throughout the earth. Has this state of things existed for any long period, or is it the result of different progress in the same group since the human period? Thus the present fauna of Australia was preceded in the post-pliocene and pliocene by forms possessing similar peculiarities, and belonging to the same classes: that is, by herbivorous and carnivorous marsupials and monotremes, and by Varanid Sauria, all of greater size than their predecessors.

"The same fact is well known of the Neotropical region, its present peculiar Edentata having been preceded by giants of the same type in the post-pliocene and pliocene."

In the Nearctic, the later Palæarctic, and the Palæotropical regions, the existing genera were similarly represented by pre-existing types, sometimes wonderfully developed.

"Prior to these faunæ another state of things has, however existed. North America has witnessed a withdrawal of a Neotropical fauna, and the Palæarctic the retreat of an Ethiopian type. During the post-pliocene in North America, Neotropical genera were to Neartic as 12 to 29, as the record now stands. In the pliocene beds of Pikermi (Greece) antelopes, giraffes, rhinoceros, hippopotamus, huge manis, monkeys, monitors, and other genera and species of African relationship, are the prevailing forms, and still earlier a strong mingling of Nearctic and more of Neotropical types abounded in the Palæarctic" (p. 77).

We have, then, three important terms from which to derive a theory of the creation:—(1) The existing six faunæ bear in many of their parts developmental relations to one another; (2) They were preceded immediately by faunæ similar to them in each case, but more remotely by faunæ like those now in existence; and (3) the Southern Hemisphere is a geologic stage behind the Northern one in progress, as is shown by its perfection in types extinct in the Northern, and by its inferiority in modern types prevalent in the Northern.

For a fuller demonstration of the last point we must refer our readers to pp. 78, 79 of this valuable monograph.

G. E. D.

MISS NIGHTINGALE ON LYING-IN INSTITUTIONS

Introductory Notes on Lying-in Institutions. By Florence Nightingale. Pp. 110. (Longmans, Green, and Co. 1871.)

MISS NIGHTINGALE tells us the story of this book somewhat as follows:—The Committee of the Nightingale Fund, with the view of extending the usefulness of their Institution for training nurses, entered into an arrangement with St. John's House and King's College Hospital, by which a special ward was set apart for the reception of poor women in childbed, and steps were taken for training midwifery nurses to be employed among the poor in their own houses.

After the ward had been in use for several years, the Committee were made aware that there had been many deaths among cases admitted; this led to inquiry, and the ward was closed.

The Committee being still desirous of continuing this special branch of their work, Miss Nightingale deemed it advisable to inquire into the whole subject of puerperal mortality, and the result is now before us in a form which we can all understand, and we will venture to say that to the generality of readers the facts will bear the aspect of an unwelcome revelation. These facts have been drawn from the Registrar-General's reports, from reports of public institutions in the United Kingdom and over most European countries, affording relief to poor women in their need, both at home and in lying-in institutions, and also from records of private practice.

They show that, while the death-rates for all England from diseases and accidents peculiar to childbirth amount to 4·83 per 1,000, they exceed this amount whenever women pass within the walls of lying-in hospitals—increasing to 5, 6, 7, and in one instance to above 19 per 1,000. If we confine our attention to puerperal diseases, we find that, while the death-rate for all England from these is 1·61 per 1,000, it mounts up in workhouses and other lying-in establishments to 3·3, 3·9, 4·1, and 14·3 per 1·000. In King's College Hospital lying-in ward, the puerperal disease death-rate was nearly 29½ per 1,000. By using Dr. Lefort's data, which give the death-rates from all causes at home and in hospital, in various European countries, it is shown that the approximate death-rate at home is 4 7 per 1,000, while in lying-in institutions it is no less than 34 per 1,000.

Miss Nightingale discusses the causes of these immense death-rates, which, she reminds us, occur among women undergoing not a diseased, but a perfectly natural condition, among whom a death "is little short of a calamity," and "almost a subject for an inquest." We cannot enter into the discussion, but we can say distinctly what is the impression produced by the evidence. It affords another illustration of the danger of unenlightened philanthropy. Some one takes pity on poor suffering women, and forthwith builds an hospital for them or gets it built, without a thought, apparently, of what organic laws of human nature he is about to violate. Nature takes no account of his good intentions, but just goes on, as Miss Nightingale has elsewhere said, "to levy her own cess in her own way."

The practical result of the whole discussion is that lying-in establishments, as at present managed, are destructive of human life, and should be forthwith closed, and that poor women should, as a rule, be attended at home.

The case, however, is not altogether hopeless; and Miss Nightingale proceeds to show how an institution for training midwives and midwifery nurses can be planned and managed without risk. The whole secret consists in assimilating the establishment to home conditions, whatever the cost may be. The evidence shows that in such an institution there would be no more risk than at home. The difficulty, as it appears to us, would be in the cost and in the perfection of management required, which could only be attained by persons practically conversant with physiological laws. But, at the same time, there can be no question of the superior advantages for training which such an institution would afford. This portion of the book is illustrated by plans of existing hospitals, and of the proposed training school. It contains a large amount of valuable detail in small compass, well worthy the attention of the medical profession and the public at large; concluding with an appeal to women, desirous of entering on medical studies, to make this department of practice their own.

The book, as its title implies, is tentative, and there is prefixed to it a quaint dedication to "the shade of Socrates' mother," including a call for help to "the questioning shade of her son, that I, who write, may have the spirit of questioning aright, and that those who read may learn not of me but of themselves." If this Socratic spirit of "questioning aright" were more cultivated, we should have fewer philanthropic mistakes, and science would be less troubled than it has been of late by dogmatic assertions and crude speculations.

OUR BOOK SHELF

Text-Book of Geometry. Part I. By T. S. Aldis, M.A., Senior Mathematical Master, Manchester Grammar School. (Deighton, Bell, and Co.)

We are much pleased with this book as a good text-book for teaching geometry. It is evidently the work of one who has been at the pains to consider well what are the difficulties which the average pupil encounters. It is the work, too, of one who has seen what the fault of the school teaching of geometry has hitherto been, and who is determined, as far as lies in his power, to remedy it. The evil of school-teaching has been that Euclid has been learned by rote, or when things have not been so bad as that, its propositions have been regarded too much as only abstract truths, which neither have been elucidated by, nor have been used to elucidate natural phenomena or the ordinary things of life. Mr. Aldis supplies this defect by an admirable series of examples and exercises appended to each proposition, calculated to give a practical turn to the whole study in the mind of a beginner, and to familiarise him early with the idea that he can really make use of the subject, and can give it a vast variety of application. Mr. Aldis frequently gives more than one demonstration of the same proposition. This also is very useful in teaching, inasmuch as it practically informs the pupil that the truths of geometry are independent of any particular demonstration of them, and gets him into the habit of approaching any problem from more than one point of view. The present is a first instalment. It contains pretty nearly what is in Euclid's first four books. J. S.

Populäre Wissenschaftliche Vorträge. Von H. Helmholtz. 2tes Heft. (Braunschweig: Verlag von F. Wieweg. London: Williams and Norgate.)

This part of Helmholtz's essays reminds us in many respects of Tyndall's lectures—in their clear and eloquent language, eminently adapted for popular comprehension, their freedom from technical expressions, except where these are unavoidable, and in the original mode in which well-known facts are dealt with and used to illustrate profound scientific truths. The work contains six lectures, of which three are devoted to recent advances in the theory of vision, one to the correlation of the physical forces, one to the conservation of force, and the last to the objects and advances of science. In the three lectures devoted to the eye, whilst extolling its perfection as an instrument in the mode in which we use it, he points out its various defects; the blind spot, the blind lines and striæ corresponding to the vessels, its incapacity to focus equally red and violet rays, the want of uniformity in its refraction as indicated by the lines that appear to proceed from a star, &c. He discusses the various colours of the spectrum, and represents this not in the mode usually adopted of a circle with segments of various sizes corresponding to the several primary colours, but as a triangle, of which green, violet, and red occupy the angles, and blue, yellow, and purple the sides, white having an eccentric position near the yellow. Violet, which he was formerly indisposed to regard as a primary colour, he again admits, and he seems inclined to advocate, as best explaining the phenomena of colour-blindness, the views of Young: that there are special nerves for perceiving red, green, and violet rays, an opinion that is less surprising in view of Brown Sequard's conclusions in regard to the number of channels for special sensations contained in the spinal cord, and which is also supported by the remarkable specialisation shown by Helmholtz himself to occur in the branches of the auditory nerve indicated by the phenomena of certain defects of hearing. The chapters on the correlation of the physical forces and the conservation of force, subjects that are now familiar to most scientific Englishmen, are very interesting, as being, to use the German phrase, amongst the original path-breaking essays on these subjects. H. P.

Notes on the Food of Plants. By Cuthbert C. Grundy, F.C.S. (London: Simpkin, Marshall, and Co., 1871.)

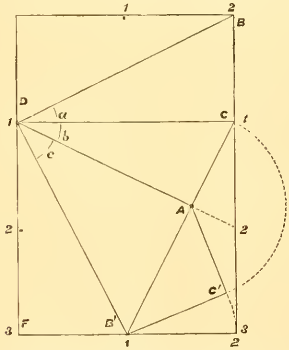
THIS is a useful elementary sketch of the form and manner in which food is obtained by plants. Faults in it there are. Thus, notwithstanding the conclusive experiments of Prillieux and Duchartre, proving that plants have no power of absorbing moisture through their leaves, and the author's own reference to this now established fact in the preface, we still find the assertion (p. 14) that "the leaves withdraw from the atmosphere aqueous vapour." The statement (p 25) that the sap descends in dicotyledonous plants *through* the bark is not strictly correct; and a Fellow of the Chemical Society ought not to have described (p. 23) carbonic acid as "carbon dioxide combined with water." These blemishes apart, this little book may be recommended to those who desire an explanation of the mode in which vegetable organisms are built up from inorganic materials, and who are unable to devote the time to the more elaborate works of Mr. Johnson, "How Crops Grow" and "How Crops Feed." The portion relating to the effect on crops of different soils strikes us as the best.

LETTERS TO THE EDITOR

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

Proof of Napier's Rules

As the following graphical construction is easily executed, representing to the eye the figure usually employed for the proof of Napier's rules of the parts of right-angled triangles in spherical geometry, it will perhaps remove difficulties from their proof for beginners, like those which Mr. W. D. Cooley's work on "Elementary Geometry" must, from his description of some interesting parts of its contents in NATURE of the 19th of October, have proposed to itself to meet, and to render at least as easily accessible as possible to the inquiring student in mathematics.



BF is a rectangular card, measuring two inches by three inches in the sides, and divided by the lines DB, DC, DA, DB', and B'C in the directions shown in the figure, and in such a manner that the three corners of the rectangle are completely cut away by the last two, and by the first of these lines; while DC and DA are only cut or scored lightly in the card, so as to allow the remaining three triangles, DBC, DCA, DAB', to be folded towards each other, until, DB and DB' coinciding, they form a solid angle of three faces at the point D. The property possessed by this solid angle, that the inclination of the two faces, DCB, DCA, to each other is a right angle (the angle shown at C' in the base, AB'C' of the solid angle), and that the base AB'C' of the resulting tetrahedron cuts the two faces ADC, ADB', perpendicularly (or at right angles to their common intersection DA) in the line AC, AB', so that the plane angle A of the plane right-angle triangle B'AC' is also the inclination between those faces, or the angle of the right-angled spherical triangle formed by the intersection of a sphere, about the centre D, with the three planes meeting each other at that point, affords a ready proof of all Napier's rules, excepting that connecting the two angles of a right-angled spherical triangle, from the simple definitions of the trigonometrical "ratios" of plane angles.*

Calling the angles of the faces which meet together at the point D, as shown in the figure a, b, c, opposite to the spherical angles A, B, C, formed by the inclination of the other two faces to each other, these angles, and those of inclination of the faces are, respectively, the sides and angles of a right-angled spherical triangle, whose right angle is C, its hypothenuse is c, and the angle A, between b and c is equal to the plane angle A, of the right-angled triangle AB'C'.

Taking, firstly, as the radius, DA, equal to unity, AC (or AC'), and AB' are the tangents of b and c; and the right-angled triangle AC'B' gives the rule,

$$\frac{\tan b}{\tan c} = \cos A \text{ ; or } \cos A = \tan b \cdot \cot c \qquad (1)$$

Taking, in the next place, DB, (or BB'), as the radius, equal to unity; BC (or B'C'), and B'A are the sines; and DC, DA are the cosines of the angles a and c. In the first case the right-angled triangle AB'C' affords the ratio

$$\frac{\sin a}{\sin c} = \sin A \text{ ; or } \sin a = \sin c \cdot \sin A \text{ ;} \qquad (2)$$

And in the second case we obtain from the right-angled triangle ADC the rule

$$\cos c = \cos a \cdot \cos b \qquad (3)$$

The rules for the angle B, corresponding to (1) and (2) for the angle A, are simply obtained from them by transposing in them the sides and angles aA for bB; thus—

$$\cos B = \tan a \cdot \cot c \qquad (4)$$
$$\sin b = \sin c \cdot \sin B \qquad (5)$$

Finally, dividing (1) by (5), a rule for connecting together the two angles of the right-angled spherical triangle is found as follows:—

$$\cos A \div \sin B = \frac{\tan b}{\tan c} \div \frac{\sin b}{\sin c} = \frac{\cos c}{\cos b} = \cos a, \text{ by (3);}$$
$$\text{or } \cos A = \cos a \sin B \qquad (6)$$

If, as in Napier's rules, the two sides and the differences from 90° of the two angles and of the hypothenuse arranged in their natural order round the triangle are regarded as constituting its five parts, it will be seen that all the above consequences may be included in the two rules known as Napier's rules, that the sine of the middle (that is, of any chosen) part is equal to the product of the tangents of the two adjacents, as well as to the product of the cosines of the two opposite parts.

As a rule to assist the memory, the laconic brevity and completeness of Napier's formula possess a most uniquely felicitous, and, happily for mathematicians, a not unfrequently enduring charm. But should the student desire to divest himself of their artificiality, and to retrace for himself the steps of the demonstration upon which any one example of these rules is based, he must first draw a solid tetrahedron ABCD, in which the facial angles at A, C, are as represented in the figure, but as they cannot all be correctly shown on account of the embarrassing effects of the perspective in the drawing, right angles. By having recourse to a model, on the other hand, which may very readily be cut from a card like that illustrated in the above description, and folded so as to form the solid figure required for their demonstration, all the cases of Napier's rules may be exhibited, and proved, almost as speedily, and satisfactorily to a learner's apprehension in solid geometry, as the definitions of the simple trigonometrical ratios of plane angles, and the least complicated relations connecting together the parts of plane triangles may be made intelligible to him; and that by a plain series of immediate deductions from the figure, which his familiarity with the processes of plane trigonometry will already have taught him very easily to supply.

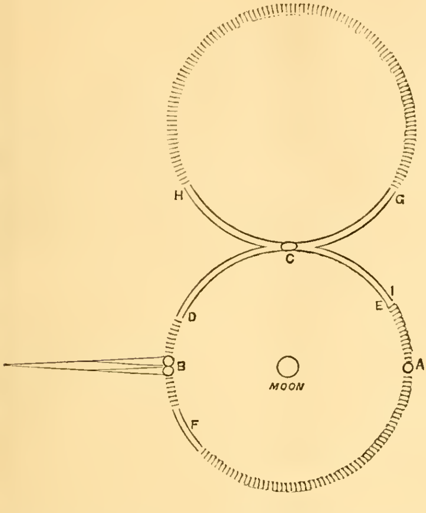
Newcastle-on-Tyne, Oct. 30 A. S. HERSCHEL

* Another similar property, with a somewhat less important application of the same tetrahedron, is described in the *Quarterly Journal of Mathematics* for October 1862, p. 306.

Remarkable Paraselene seen at Highfield House on October 25th, 1871

THE phenomenon first became visible at 7h 12m P.M., and finally vanished at 7h 33 P.M. The upper portion of a halo of

22° 30′ radius marked DCE, together with a detached portion of F, had the moon for its centre; at the apex of this circle was the apex of another of similar dimensions, HCG, whose centre was about 45° above the moon. On the horizontal level of the moon on either side were mock moons, AB, and immediately above the moon within the same circle was an oval mock moon, C.



Both A and C, though very apparent, were nevertheless not brilliant, the grandeur of the phenomenon centering in the double mock moon B; this was so brilliant that it attracted immediate attention, and that portion nearest the moon was sensibly orange-red. At first it appeared as one large mock moon (twice as broad as it was long), and at $7^h\ 19^m$ divided into two with a thin dark band between. Whilst the two moons (touching each other) were visible, each had a tail of 10° or more in length, and these were included within a gigantic tail of 25° long, considerably more brilliant, but colourless, and contrasting much with the orange-red of the mock moons. At the time of the phenomenon a fog spread over the valley, and overhead were strong cirri in parallel bands. The temperature was 37°·2, and on the grass 30°·6.

The moon shone brightly and the sky was cloudless near her throughout the whole time. At $7^h\ 33^m$ a cloud of considerable density obscured both the moon and the phenomenon.

E. J. Lowe

Structure of Lepidodendron

I may, perhaps, notwithstanding the editorial injunction to the contrary, be permitted to make one remark by way of addition to what I said in my last letter on this subject. I have been favoured by Mr. J. T. Young with the inspection of some Lepidodendroid stems from the Lancashire coal-fields. These are somewhat different from any others which I have seen, and are probably similar to those Prof. Williamson is working with. At any rate they enable me to understand, what otherwise I have failed to comprehend, namely, the three structures which Prof. Williamson sees in the vascular axis of these plants. In Mr. Young's specimens a transverse section of the vascular axis exhibits (1) the investing cylinder, (2) a zone of larger scalariform vessels, (3) a central irregular mass of vertically disposed rows of scalariform cells with transversely truncate ends. Suppose the transverse septa separating these cells absorbed, as probably eventually they would have been, and the rows of cells become scalariform vessels. I see no reason therefore to lead me to alter my views upon this matter, or to look upon 2 and 3 as forming more than one central structure distinct from 1, the investing cylinder.

W. T. Thiselton Dyer

Is Blue a Primary Colour?

No exception can be taken against Dr. Aitken's argument in your number for Oct. 12. The colours of the substances he experimented on could not be regarded as simple. But he does not consider how loosely all names of colours must be applied in common language. The colours of most blue pigments, especially in thin washes, no doubt contain a large proportion of green. But let the colour of the blue salvia, or that of the pigment called French blue or ultramarine (often given as the best example of true blue) be tested in conjunction with the purest yellows (even with the almost greenish yellow of the pigment called lemon-yellow) and the two will be found perfectly complementary. This is the colour of Newton's indigo rays, which he himself in his colour circle put opposite to his yellow. In fact, in good English, not only sea-greenish blues, like the colour of Newton's blue part of the spectrum, or that of the pigment called azure or cæruleum, but even the colour of the violet itself, is properly called blue. Witness Milton's "beds of violets blue." The violet of the spectrum is in truth little more than a pure blue diluted with white by reason of the fluorescence of the retina, as recent researches have shown. (See J. J. Müller's paper in *Poggendorff's Annalen*, March and April last.) I must, therefore, protest against substituting a fanciful term like violet for the good English blue, as the designation of a simple colour-sensation. It is hard enough to make artists believe that yellow is not a simple colour. To tell them the same of its complementary blue would add to their disgust, and not unreasonably.

William Benson

Mr. Aitken in his letter in Nature, Oct. 12, seems to confound primary with pure colours; it is true they are pure in a certain sense, but in what sense is fully explained in Prof. J. C. Maxwell's lecture, given in Nature, vol. iv. p. 13. All the experiments mentioned by Mr. Aitken merely prove that the blue colours we commonly see are mixed ones; but the same is the case with almost all the colours we see, while any tint of the spectrum, whether primary or not, may be had pure, *i.e.*, consisting of homogeneous light. Likewise colours which appear just the same to the eye may be made of very different components.

T. W. Backhouse

A Shadow on the Sky

On the 21st of last August, being at Zermatt, Switzerland, I witnessed from the balcony of the *salle-à-manger* of the Hotel du Mont Cervin a very remarkable appearance. The sun had recently set, and, as I was intensely enjoying the view of that extraordinary mountain, the Matterhorn, I saw its shadow thrown upon the clear sky in the most distinct manner. It was the exact figure of a cone lying obliquely, with its apex somewhat in an upward direction, and its base taking its origin from the S.S.E. side of the mountain. The cone was well defined, the edges of the shadow being sharp and regular. The moon was, from our point of view, at this time behind the Matterhorn. I immediately acquainted some gentlemen, who were at supper in the *salle-à-manger*, with this interesting appearance, and all were much struck with it. My son, Marshall Hall, had just retired to rest, having to be up at two the next morning, in order to make a new ascent in this locality; but I called him out into the garden to enjoy with me this striking scene. The deep, distinct shadow added to the weird effect always produced by this extraordinary mountain, and it so impressed me that I thought the phenomenon might be worth recording in your journal.

Brighton, Oct. 23 Charlotte Hall

A Plane's Position

This question is becoming one *de gustibus*, and its further discussion will probably be profitless. I retain my opinion, and am content with the few who side with me. In the two finest treatises on astronomy published during the present century, Herschel's "Outlines of Astronomy," and Grant's "History of Physical Astronomy," the word position is used as I use it. Not systematically, I admit; for Herschel sometimes wrote "situation" where I should write "position." Grant in one place deals somewhat definitively with the word, for at p. 258 he writes, "The position of Saturn's ring is usually determined by the inclination of its plane to the ecliptic and the longitude of its ascending node," the longitude of this node being defined, as all astronomers know, by the *direction* of the line of nodes, not by its *actual place*.

By-the-bye, Sir John Herschel is sometimes very careful to use the words "actual place" where my critics contend that the word "position" would be sufficiently definitive.

It seems overlooked that I pointed out in the beginning that "position" was often but erroneously used as synonymous with "place." It is not my fault if this error appears in the technical use of the word "position" in some mathematical treatises. I say again with Colonel Mannering, *Abusus non tollit usum*—"The abuse of anything doth not abrogate the lawful use thereof." It was a *lapsus calami* of mine to say that "position" could not be misunderstood. It could be, for it has been misused.

Prof. Hirst is quite right in saying I should be unable to describe the aspect of a horizontal plane. I should not think of trying to. He says, however, that Mr. Wilson would unhesitatingly pronounce its aspect *vertical*. (Does it look vertically *up* or vertically *down*?) What would Mr. Wilson assign—unhesitatingly or otherwise—as the aspect of the "prime vertical"?

Has a true plane (as distinguished from a plane face of a solid) one aspect or two? It has one *position* or *situation*, and one *place* or *location*, but I conceive that it has two aspects.

Mr. Laughton seems quite unaware of Sir J. Herschel's repeated use of the word "tilt."

His comment on my remark about the books which I have written is unworthy. He must surely perceive that I only sought to indicate how much occasion I had had to consider the subject of plane-position; more occasion, I think, than any of my critics, save Prof. Hirst, the weight of whose opinion I recognise fully, though I cannot agree with him. But I have *not* felt free to use the word "position" so systematically as I should wish, precisely because of its misuse to indicate *place*. I have only been able to use it where there could be no fear of that wrong meaning being assigned to it.

As I claim no credit for the invention of any word for indicating plane position, and as I could not take from Mr. Laughton that which is not his—the credit for Hamilton's word "aspect"—perhaps I may be permitted to say that if I am "pertinacious" (as Mr. Laughton asserts) there is nothing personal in my pertinacity. It is not my custom to admit that I am wrong when I consider that I am right.

[My objections to the word "aspect" are confirmed by Mr. Wilson's letter. I wrote that the word could not be used in the sense indicated, "unless a new and artificial meaning were assigned to it." Mr. Wilson obligingly proves this by assigning to it just such a meaning. "The aspect of a plane is the direction of its normal," it would seem. Now no special objection need be urged against this definition, if it is to be confined rigidly within the limits of mathematical text-books. The definition is strange and artificial no doubt; but it is nothing new to see the familiar and natural banished from such works. As a writer on astronomy, however, I must decline to accept the proposed usage, which seems to me altogether objectionable. If I write respecting the celestial equator-plane that "its position is at right angles to the polar axis of the heavens," I find that I am understood; but I am sure my readers would be very much perplexed if I wrote that "the aspect of the equator-plane is the direction of the polar axis." Again, I should be understood, I think, if I said that "the positions of two hour-planes determine the direction of the polar axis," or that "the directions of the polar axis and the vertical determine the position of the meridian-plane." But if I wrote "aspect" where I have here written "position," I scarcely know what my readers would think.

By the way, what would be the "aspect" of the meridian-plane according to the proposed usage? Would it be "east" or "west"? The normal to that plane would lie east and west; but we could not hear of an "east-and-west" aspect without thinking of certain "clear stories towards the south-north, lustrous as ebony."

I am bound to point out, however, though I may seem to weaken my position by doing so, that a very eminent authority long since used the word "aspect" in the sense suggested by Mr. Laughton. In one of his well-known "Letters to a Lady," on quaternions, Sir W. R. Hamilton uses the words "position," "slope," "ledge," and "aspect," to express the relations which I have called respectively "place," "slope," "aspect," and "position." (See Nichol's "Cyclopædia of the Physical Sciences," 2nd edition, p. 708.) I apprehend, however, that he lays no special stress on this verbiage. He had used the word "position" for "place," and this left him without any word to indicate position. Besides, his illustrative plane is the surface of a desk, and a surface may be conceived to have an aspect definable by the direction of its normal, but a geometrical plane is two-faced.]*

This is my last letter on the present subject—unless one of your correspondents should employ arguments showing me to be in error, in which case I shall crave two lines of your space to admit as much. RICHD. A. PROCTOR

Brighton, Nov. 3

P.S.—Let it be noticed that the question is not how the word "position" has been used by some, but how it ought to be used by all.

I CANNOT agree with Mr. Wilson that "aspect" is *exactly* the word wanted. The same wall has *two* aspects; if a southern, then also a northern aspect on the other side. In fact the word seems adapted, according to its common usage, to express the "sense" (*sens*), as well as the direction of the plane's normal, whereas I take it that the word sought for should express the direction only without connoting the "sense."

I think a word sometimes used by geologists would be, if we dare use it, exactly the word. As they speak of the *lie* of strata, defined (with respect to the horizon) by its two elements, *strike* and *dip*, so geometers might well speak of the *lie* of a plane; but would our English language permit us to say that "two lies determine one direction," and "two directions determine one lie"? I fear the moral connotation of the word, although an etymological accident, is too ugly.

If we are reduced to coin a new word, I would suggest that the Latin root "pand" (spread), would afford for a plane the fitting analogue of the root "reg" (rule, make straight), for a line, and so the word "dispansion" would be the analogue of "direction." "Parallel planes have the same dispansion." "Two dispansions determine one direction, and two directions determine one dispansion." Will not the neatness of this mode of expressing Mr. Wilson's test propositions atone for the strangeness of the word?

The word "aspect," however, is too good to be rejected from geometrical science, though I believe its chief use will be found beyond the domain of pure geometry. Should it not be appropriated to cases where the plane presents different aspects to the portions of space on either side of it? For instance, if two bodies revolve in the same or parallel planes, their orbits might be said to have the same or contrary aspects, according as the bodies revolve in the same or contrary directions, and so the positive aspect of a planet's orbit would determine, not only the "lie" or "dispansion" of the plane of the orbit, but also the direction of revolution in that orbit. So, too, the statement that all the planetary orbits have nearly the same aspect, would imply not only that their planes nearly coincide, but also that they all revolve in the same direction. I cannot help thinking that Mr. Proctor would find his account in adopting this sense of the word "aspect" in his astronomical writings, especially since he might, as Dr. Hirst suggests, retain the word where he has hitherto employed it, by simply qualifying it with an appropriate adjective. (Would the adjective "azimuthal" satisfy him?

May I conclude with a question which I have often wished to propound? What is the proper English equivalent for the French "*sens*"? English mathematicians generally seem shy of using the word "sense," while, to use the word "direction" as well for the "*sens*" as the "direction" of a line, is very awkward and inconvenient. The difficulty, I imagine, is the same as appears to me almost fatal to the word "lie" proposed above, namely, that the proposed technical use diverges too widely from the familiar use of the word. Is not the superior flexibility of the German language in the formation of new terms in part due to a less degree of fastidiousness in this respect?

Harrow, Nov. 6 ROBT. B. HAYWARD

AFTER all, I fear the word "aspect" is not quite the right thing. What is wanted is a word to express "plane-direction;" something in the plane, and not looking out from it. And I am not sure that the compound word "plane-direction," which is not ambiguous nor colloquial, will not be better even than "aspect."

We should then have axioms on planes analogous to those on straight lines: that planes may have the same or different plane-directions: that intersecting planes have different plane-directions; and conversely.

Parallel planes will be defined as those which have the same plane-direction.

* The matter between brackets was written on October 27.—ED.

With this word it is easier to state the theorems, "two line-directions determine one plane-direction," and its reciprocal, than with the other. "Two directions determine one aspect," is hard.

If this discussion has not gone on too long perhaps some of your correspondents will criticise this suggestion and compare it with "aspect." It is desirable that the best word possible should be chosen. J. M. W.

Science and Art Examinations

THE subject of Science and Art Examinations by the Department of Science and Art is one which really requires looking up, and I wish to make one or two suggestions and remarks as to the mode of examination.

In the first place, take the examination itself. The candidates make their appearance at the appointed time and place. Their forms are given them, and their places assigned to them. Now the candidate is told to write on both sides of the form, thus leaving no back pages on which to do his rough calculation. Blotting-paper in 1870 was not allowed; but in 1871 the Department fixed a sheet to the bottom of each form in such a position that it was very difficult to make use of it; much time—time that was of the utmost consequence to the candidate—being lost in doing so. This, of course, stopped him from doing so much work, and so lessened his chance of success. This may be all very well for the Department so far as it affects grants on results; but what about the unfortunate student who is made the victim of this very arbitrary custom?

Then again for the questions set. In all the papers the questions set were very difficult. "The Department" having, without any notice, raised the standard of examination, the subjects of questions set in the first stage of mathematics were placed in the syllabus a stage higher, viz., the second stage. Then in chemistry (inorganic) the standard was considerably raised. The questions in this subject are very unfair in the opinion of many persons who have seen them. Take the following:—

"HONOURS 1871

"Describe the process of manufacturing sulphuric acid, as carried on in an alkali works, illustrating the various chemical changes by equations, and, as far as possible, the constitution of the compounds formed by graphic formulæ."

Now about the sulphuric acid part, or about the equations, I have nothing to say; but when the question requires a knowledge of graphic formulæ I protest against it. Graphic formulæ are not in sufficient use to warrant their introduction into an examination—thus enforcing their general adoption whether right or wrong; and I do not think the examiner should be allowed to enforce his peculiar views—the views taken by himself and a few other chemists—into the great system of Science examination in the country, thus compelling it to be learnt by any person wishing to compete.

Now for the results. The results of the examinations for 1871 are very unsatisfactory, and a very high ratio is shown of failures, and second classes to first classes obtained. This, of course, must lessen the amount of money to be paid on results by the Department, and a report was circulating a short time ago, to the effect that "The examiners, after having made their reports, had the papers returned to them, with an instruction to reduce the number of successful candidates, as an intimation had been given by a right hon. gentleman that the amount of grant due upon those papers must be reduced 20,000*l*. The examiners were thus obliged to eliminate half the names from their lists." The question was asked by Mr. Dixon, M.P., in the House of Commons, whether this was or was not true, and Mr. Forster, M.P., denied it. But, previous to that, a provincial local secretary, hearing the rumour, wrote to ask the Department if it were true, and received a reply saying it was true, and that instead of the amount being 20,000*l*. it was 40,000*l*. (The Department's letter can be produced.) Now I would suggest that the Department reform these matters referring to the forms, blotting-paper, questions, and results, and that if they do not do so that the House of Commons take the matter up and do justice to Science teachers and students. HENRY UHLGREN

New Zealand Forest Trees

IN the last number of NATURE is a paragraph relating to some New Zealand woods, which the writer observes are "deserving of a better fate than to be cut down wholesale and used as firewood." Five timber trees are mentioned, of which the native names only are given.

Knowing that it is the province of NATURE to give as accurate information as possible on all points with which it deals, I send you the botanical names of four of these New Zealand trees. The Rimu or red pine is probably *Dacrydium cupressinum* Soland, a tree 80 or more feet high, the fleshy cup of the fruit of which is eatable. *D. laxifolium* Hk. fil., a small creeping bush, is also known occasionally as Rimu. The Mataii or black pine is *Podocarpus spicata* Br., likewise a large tree, and having an eatable fruit. The Totara is *Podocarpus totara* A. Cunn., a tree about 60 feet high, producing a durable and close-grained wood much valued in the islands, and, like the others, having an eatable drupe. These trees are all more or less abundant in the Northern and Middle islands, and all belong to the natural order Coniferæ, though we are told in the paragraph referred to that "none of them are Coniferæ."

The Rata, "that wonderful vegetable production forming itself out of numberless vines," &c., is referable to some species of *Metrosideros*. *M. robusta* A. Cunn, and *M. florida* Sm., are both known as Rata, but the hard and very dense wood usually known under that name is mostly derived from *M. robusta*. This, however, is not a climbing plant, but an erect tree 50 or 60 feet high; therefore the plant referred to in the paragraph before us is probably *M. florida*. The Makia I do not know, but its extreme hardness would seem to indicate it as belonging to the same order as the last, namely the Myrtaceæ.

JOHN R. JACKSON

Kew, Nov. 7

The Glacial Drift at Finchley

A FURTHER examination of the railway cutting at the Finchley and Hendon Station shows that the glacial beds now revealed there have a greater thickness and range than I at first imagined. On Saturday last I visited the place in company with Dr. Hicks, of Hendon, a gentleman well-known for his researches in the Cambrian formation. Above the blue clay, and right up within a few inches of the vegetable soil, we found drift fossils. With an interruption here and there from the underlying London clay, these chalky glacial beds, consisting of blue (Oxford?) clay, blueish clay with flints, marl, sand, and gravel (in no regular descending order), have an average thickness of 30 feet. They are open for about 500 yards, and they might perhaps be traced farther north-west, towards the Dollis Brook Viaduct. Dr. Hicks and I afterwards visited Mr. Plowman's Manor brick-fields, a little south-east of the railway station; here too we found fossils in the brick-earth.

From what has transpired during the last few weeks, it would seem that the Muswell Hill deposit need no longer figure in geological literature as an outlier, at a long distance from the general deposit; and Londoners may in future find glacial drift without much difficulty about Highgate, Finchley, Whetstone, and Barnet. I am indebted to Professor Morris for the information that the Great Northern Cemetery at Barnet lies almost wholly in the glacial clay. The forthcoming Survey memoir upon the drift in this district is looked for by London geologists with much interest. HENRY WALKER

100, Fleet Street, E.C., Nov. 7

*ON THE ORIGIN OF INSECTS**

THE metamorphoses of this group have always seemed to me one of the greatest difficulties of the Darwinian theory. In most cases the development of the individual reproduces to a certain extent that of the race, but the motionless, imbecile, pupa cannot represent a mature form. Fritz Müller considers that the wingless Blattidæ probably most closely represent the original insect stock; Haeckel is inclined rather to the Pseudo-Neuroptera. I feel great difficulty in conceiving by what natural process an insect with a suctorial mouth like that

* Abstract of a paper read before the Linnean Society, Nov 2, 1871, by Sir John Lubbock, Bart., M.P., F.R.S.

of a gnat or butterfly could be developed from a powerfully mandibulate type like the Orthoptera, or even from the Neuroptera. M. Brauer has recently suggested that the interesting genus *Campodea* is, of all known existing forms, that which probably most nearly resembles the parent insect stock. He considers that the grub form of larva is a retrograde type, in which opinion I am unable to concur, though disposed to agree with M. Brauer on the first point. M. Brauer in coming to this conclusion relies partly on geological considerations; partly on the fact that larvæ, more or less resembling *Campodea*, are found among widely different groups of insects. I think there are other considerations which offer considerable support to this view. No one, so far as I know, has yet attempted to explain, in accordance with Mr. Darwin's views, such a life history as that, for instance, of a butterfly, in which the mouth is first mandibulate and then suctorial. A clue to the difficulty might, I think, be found in the distinction between developmental and adaptive changes, to which I called the attention of the Society in a previous memoir. The larvæ of insects are by no means mere stages in the development of the perfect animal. On the contrary, they are subject to the influence of Natural Selection, and undergo changes which have reference entirely to their own requirements and condition. It is evident then that, while the embryonic development of an animal in the egg gives us an epitome of its specific history, this is by no means the case with species in which the immature forms have a separate and independent existence. Hence, if an animal when young pursues one mode of life, and lives on one kind of food, and subsequently, either from its own growth in size and strength, or from any change of season, alters its habits or food, however slightly, immediately it becomes subject to the action of distinct forces; Natural Selection affects it in two different, and it may be very distinct, manners, gradually leading to differences which may become so great as to involve an intermediate period of change and quiescence.

There are, however, peculiar difficulties in those cases in which, as among the Lepidoptera, the same species is mandibulate as a larva and suctorial as an imago. From this point of view, however, *Campodea* and the *Collembola* (*Podura*, &c.) are peculiarly interesting. There are among insects three principal types of mouth, firstly, the mandibulate, secondly, the suctorial, and thirdly, that of *Campodea*, and the *Collembola* generally, in which the mandibles and maxillæ are attached internally, and though far from strong, have some freedom of motion, and can be used for biting and chewing soft substances. This type is intermediate between the other two. Assuming that certain representatives of such a type found themselves in circumstances which made a suctorial mouth advantageous, those individuals would be favoured by Natural Selection in which the mandibles and maxillæ were best calculated to pierce or prick, and their power of lateral motion would tend to fall into abeyance, while, on the other hand, if powerful masticatory jaws were an advantage, the opposite process would take place.

There is yet a third possibility—namely, that during the first portion of life the power of mastication should be an advantage, and during the second that of suction, or *vice versâ*. A certain kind of food might abound at one season and fail at another; might be suitable for the animal at one age and not at another: now in such cases we should have two forces acting successively on each individual, and tending to modify the organisation of the mouth in different directions. It will not be denied that the ten thousand variations in the mouth parts of insects have special reference to the mode of life, and are of some advantage to the species in which they occur. Hence no believer in Natural Selection can doubt the possibility of the three cases above suggested, and the last of which seems to explain the possible origin of species which are mandibulate in one period of life and not in another. The change from the one condition to the other would no doubt take place contemporaneously with a change of skin. At such times we know that, even when there is no change of form, the temporary softness of the organs often precludes the insect from feeding for a time, as, for instance, is the case in the silkworm. When, however, any considerable change was involved, this period of fasting would be prolonged, and would lead to the existence of a third condition, that of pupa, intermediate between the other two. Since other changes are more conspicuous than those relating to the mouth, we are apt to associate the pupa state with the acquisition of wings, but the case of the Orthoptera (grasshoppers, &c.) is sufficient proof that the development of wings is perfectly compatible with continuous activity. So that in reality the necessity for rest is much more intimately connected with the change in the constitution of the mouth, although in many cases no doubt the result is accompanied by changes in the legs, and in the internal organisation. It is, however, obvious that a mouth like that of a beetle could not be modified into a suctorial organ like that of a bug or a gnat, because the intermediate stages would necessarily be injurious. Neither, on the other hand, for the same reason could the mouth of the Hemiptera be modified into a mandibulate type like that of the Coleoptera. But in *Campodea* and the *Collembola* we have a type of animal closely resembling certain larvæ which occur both in the mandibulate and suctorial series of insects, and which possesses a mouth neither distinctly mandibulate nor distinctly suctorial, but constituted on a peculiar type capable of modification in either direction by gradual changes without loss of utility.

If these views are correct, the genus *Campodea* must be regarded as a form of remarkable interest, since it is the living representative of a primæval type from which not only the *Collembola* and *Thysanura* but the other great orders of insects have all derived their origin.

CHARLES BABBAGE

DIED THE 20TH OF OCTOBER, 1871

THERE is no fear that the worth of the late Charles Babbage will be over-estimated by this or any generation. To the majority of people he was little known except as an irritable and eccentric person, possessed by a strange idea of a calculating machine, which he failed to carry to completion. Only those who have carefully studied a number of his writings can adequately conceive the nobility of his nature and the depth of his genius. To deny that there were deficiencies in his character, which much diminished the value of his labours, would be useless, for they were readily apparent in every part of his life. The powers of mind possessed by Mr. Babbage, if used with judgment and persistence upon a limited range of subjects, must have placed him among the few greatest men who can create new methods or reform whole branches of knowledge. Unfortunately the works of Babbage are strangely fragmentary. It has been stated in the daily press that he wrote eighty volumes; but most of the eighty publications are short papers, often only a few pages in length, published in the transactions of learned societies. Those to which we can apply the name of books, such as "The Ninth Bridgewater Treatise," "The Reflections on the Decline of Science," or "The Account of the Exposition of 1851," are generally incomplete sketches, on which but little care could have been expended. We have, in fact, mere samples of what he could do. He was essentially one who began and did not complete. He sowed ideas, the fruit of which has been reaped by men less able but of more thrifty mental habits.

It was not time that was wanting to him. Born as long ago as the 26th of December, 1792, he has enjoyed a working life of nearly eighty years, and, though within the last few years his memory for immediate events and persons was rapidly decaying, the other intellectual powers seemed as strong as ever. The series of publications which constitute the real record of his life commenced in 1813 with the preface to the Transactions of the Analytical Society, a small club established by Babbage, Herschel, Peacock, and several other students at Cambridge, to promote, as it was humorously expressed, the principles of pure D-ism, that is, of the Leibnitzian notation and the methods of French mathematicians. Until 1822 Mr. Babbage's writings consisted exclusively of memoirs upon mathematical subjects, which, however little read in the present day, are yet of the highest interest, not only because they served to awaken English mathematicians to a sense of their backward position, but because they display the deepest insight into the principles of symbolic methods. His memoir in the "Cambridge Philosophical Transactions" for 1826, "On the Influence of Signs in Mathematical Reasoning" may be mentioned as an admirable example of his mathematical writings. In this paper, as in many other places, Mr. Babbage has expressed his opinion concerning the wonderful powers of a suitable notation in assisting the human mind.

As early as 1812 or 1813 he entertained the notion of calculating mathematical tables by mechanical means, and in 1819 or 1820 began to reduce his ideas to practice. Between 1820 and 1822 he completed a small model, and in 1823 commenced a more perfect engine with the assistance of public money. It would be needless as well as impossible to pursue in detail the history of this undertaking, fully stated as it is in several of Mr. Babbage's volumes. Suffice it to say that, commencing with 1,500*l*., the cost of the Difference Engine grew and grew until 17,000*l*. of public money had been expended. Mr. Babbage then most unfortunately put forward a new scheme for an Analytical Engine, which should indefinitely surpass in power the previously-designed engine. To trace out the intricacies of negotiation and misunderstanding which followed would be superfluous and painful. The result was that the Government withdrew all further assistance, the practical engineer threw up his work and took away his tools, and Mr. Babbage, relinquishing all notions of completing the Difference machine, bestowed all his energies upon the designs of the wonderful Analytical Engine. This great object of his aspirations was to be little less than the mind of a mathematician embodied in metallic wheels and levers. It was to be capable of any analytical operation, for instance solving equations and tabulating the most complicated formulæ. Nothing but a careful study of the published accounts can give an adequate notion of the vast mechanical ingenuity lavished by Mr. Babbage upon this fascinating design. Although we are often without detailed explanations of the means, there can be little doubt that everything which Mr. Babbage asserted to be possible would have been theoretically possible. The engine was to possess a kind of power of prevision, and was to be so constructed that intentional disturbance of all the loose parts would give no error in the final result.

Although for many years Mr. Babbage entertained the intention of constructing this machine, and made many preparations, we can hardly suppose it capable of practical realisation. Before 1851 he appears to have despaired of its completion, but his workshops were never wholly closed. It was his pleasure to lead any friend or visitor though these rooms and explain their contents. No more strange or melancholy sight could well be seen. Around these rooms in Dorset Street were the ruins of a life time of the most severe and ingenious mental labours perhaps ever exerted by man. The drawings of the machine were alone a wonderful result of skill and industry; cabinets full of tools, pieces of mechanism, and various contrivances for facilitating exact workmanship, were on every side, now lying useless.

Mr. Babbage's inquiries were not at all restricted to mathematical and mechanical subjects. His work on the "Economy of Manufactures and Machinery," first published in 1832, is in reality a fragment of a treatise on Political Economy. Its popularity at the time was great, and, besides reprints in America, translations were published in four Continental languages. The book teems with original and true suggestions, among which we find the system of Industrial Partnerships now coming into practice. It is, in fact, impossible to overpraise the work, which, so far as it goes, is incomparably excellent. Having assisted in founding the Statistical Society of London in 1834, Mr. Babbage contributed to their Transactions a single paper, but as usual it was a model research, containing a complete analysis of the operations of the Clearing House during 1839. It was probably the earliest paper in which complicated statistical fluctuations were carefully analysed, and it is only within the last few years that bankers have been persuaded by Sir John Lubbock to recognise the value of such statistics, and no longer to destroy them in secret. In this, as in other cases, many years passed before people generally had any notion of the value of Mr. Babbage's inquiries; and there can be little doubt that, had he devoted his lofty powers to economic studies, the science of Political Economy would have stood by this time in something very different from its present pseudo-scientific form.

Perhaps the most admirable of all his writings was the Ninth Bridgewater Treatise, an unexpected addition to that well-known series, in which Mr. Babbage showed the bearing of mathematical studies upon theology. This is one of the few scientific works in which the consistency of natural laws with breaches of continuity is clearly put forth. That Power which can assign laws can set them aside by higher laws. Apart from all particular theological inferences, there can be no question of the truth of the views stated by Babbage; but the work is hardly more remarkable for the profundity of its philosophy than for the elevated and eloquent style in which it was written, although as usual an unfinished fragment.

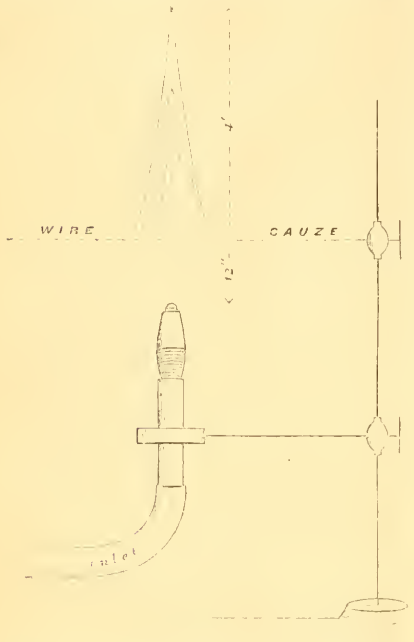
Of all Mr. Babbage's detached papers and volumes, it may be asserted that they will be found, when carefully studied, to be models of perfect logical thought and accurate expression. There is, probably, not a sentence ever penned by him in which lurked the least obscurity, confusion, or contradiction of thought. His language was clear, and lucid beyond comparison, and yet it was ever elegant, and rose at times into the most unaffected and true eloquence. We may entertain some fear that the style of scientific writing in the present day is becoming bald, careless, and even defective in philosophic accuracy. If so, the study of Mr. Babbage's writings would be the best antidote.

Let it be granted that in his life there was much to cause disappointment, and that the results of his labours, however great, are below his powers. Can we withhold our tribute of admiration to one who throughout his long life inflexibly devoted his exertions to the most lofty subjects? Some will cultivate science as an amusement, others as a source of pecuniary profit, or the means of gaining popularity. Mr. Babbage was one of those whose genius urged them against everything conducive to their immediate interests. He nobly upheld the character of a discoverer and inventor, despising any less reward than to carry out the highest conception which his mind brought forth. His very failures arose from no want of industry or ability, but from excess of resolution that his aims should be at the very highest. In these money-making days can we forget that he expended almost a fortune on his task? If, as people think, wealth and luxury are corrupting society, should they omit to honour one of whom it may be truly said, in the words of Merlin, that the single wish of his heart was "to give them greater minds"?

A NEW FORM OF SENSITIVE FLAME

MR. PHILIP BARRY, of Cork, has sent the following account of a new and very beautiful sensitive flame to Prof. Tyndall:—"It is in my experience the most sensitive of all sensitive flames, though from its smaller size is not so striking as your vowel flame. It possesses the advantage that the ordinary pressure in the gas mains is quite sufficient to develop it. The method of producing it consists in igniting the gas (ordinary coal gas) not at the burner but some inches above it, by interposing between the burner and the flame a piece of wire gauze.

"With a pressure of $\frac{7}{10}$ths at the burner, I give a sketch of the arrangement I adopted, the space between burner and gauze being two inches. The gauze was about seven



inches square, resting on the ring of the retort-stand—ordinary window-blind wire-gauze 32 meshes to the lineal inch. The burner was Sugg's steatite pin-hole burner, the same as used for vowel flame.

"The flame is a slender cone about four inches high, the upper portion giving a bright yellow light, the base being a non-luminous blue flame. At the least noise this flame roars, sinking down to the surface of the gauze, becoming at the same time almost invisible. It is very active in its responses, and being rather a noisy flame, its sympathy is apparent to the ear as well as the eye.

"To the vowel sounds it does not appear to answer so discriminately as the vowel flame. It is extremely sensitive to A, very slightly to E, more so to I, entirely insensitive to O, but slightly sensitive to U.

"It dances in the most perfect manner to a small musical snuff box, and is highly sensitive to most of the sonorous vibrations which affect the vowel flame, though it possesses some points of difference."

NOTES

THE following telegram has been received from the English Government Eclipse Expedition:—"On board the *Mirzapore*, Malta, Saturday, November 4. We have arrived here in safety. All the members of the Eclipse Expedition are quite well, no thanks, however, to the weather, which during the voyage has been very bad. It was so bad that there was no possibility of practising with the instruments. Last night Mr. Lockyer, at the request of all on board the *Mirzapore*, gave a scientific lecture with experiments. You may form some idea of the novel character with which the lecture was invested when I state that it was blowing half a gale at the time."

SIR RODERICK MURCHISON has appointed Professor Archibald Geikie, of Edinburgh, his literary executor, and has left him a legacy of 1,000*l*. The Professor will write Sir Roderick's life, for which the deceased baronet had collected ample materials. Sir Roderick has also bequeathed to each of the professors at Jermyn Street a little remembrance of 100*l*. To the institution itself he has left the diamond snuff-box and the magnificent Siberian avanturine vase, mounted on a porphyry pedestal, presented to him by the late Emperor of Russia. He has not been unmindful of the scientific societies with which he has been so long connected. To the Geological and Geographical Societies he has bequeathed legacies of 1,000*l*. each, for the purpose of furthering the cause of science by rewarding men of science by prizes or otherwise as may be deemed proper. To old associates with him in his work he has likewise left legacies as expressions of his regard. Besides that to Mr. Geikie, sums of 350*l*. are appropriated for Prof. John Morris, Prof. T. Rupert Jones, Mr. Trenham Reeks, and Mr. Bates, and a sum of 100*l*. to Mr. C. W. Peach. We believe also that in the event of the failure of some of the heirs designated in the will, considerable sums are to go to various charitable and scientific institutions.

IN addition to the appointments to the governing bodies of the public schools, made by the Senate of the University of London, which we announced last week, the Council of the Royal Society has made the following:—Prof. P. M. Duncan, for Charterhouse; Prof. Tyndall, for Harrow; Prof. Henry J. Smith, for Rugby; Sir James Paget, Bart., for Shrewsbury; and the Rev. Prof. Price, for Winchester School.

PROF. P. M. DUNCAN, F.R.S., of King's College, has been appointed Lecturer on Geology to the India Civil Engineering College, Cooper's Hill.

WE learn from the *Pall Mall Gazette* that a mixed Committee has been appointed by the authorities of the War-Office, to conduct an inquiry into the safety of gun-cotton, and to make the necessary experiments. The committee will also be required to collect evidence with regard to its value as an explosive agent; and generally to pronounce as to the suitability and safety of the material for use in torpedoes, breaching stockades, mining, &c. The Committee consists of Colonel Younghusband, R.A., president; Colonel Milward, R.A., Colonel Gallwey, R.E., Lieutenant-Colonel Nugent, R.E., Captain Field, R.N., Dr. Odling, F.R.S., Mr. H. Bauerman, and Mr. G. Bidder, C.E. The question of the safety of the new explosive "Lithofracteur," which a German firm is anxious to be permitted to make in this country, has also been referred to the same Committee.

MR. G. M. SEABROKE, the Temple Observer at Rugby, states, in a letter to the *Times*, for the information of those who possess telescopes of moderate aperture, that Encke's comet is now within their reach. It has been examined at the Rugby Observatory with an $8\frac{1}{4}$ in. aperture, and was very plainly seen. It has somewhat the shape of a fan, and there is a marked condensation on the eastern side, being the leading portion of the comet. It would probably now be seen with a much smaller aperture than that mentioned above, and, as it is approaching us, small telescopes will probably soon show it.

THE German Astronomical Society has recently held its triennial meeting at Stutgart, under the presidency of Prof. Otto Struve. The gathering was eminently a social one; after papers read in the morning, they adjourned for excursions in the afternoon, one day visiting the birth-place of Kepler, a small town about an hour by rail from Stutgart. The inhabitants, who have recently erected a bronze statue to their great fellow townsman, decorated it with flowers for the occasion.

THE Scientific Societies have now mostly commenced their winter session. The greater number held their first meeting either last or during the present week. The first meeting of the Royal Society for the season is on November 16.

THE Annual General Meeting of the five Academies which constitute the Institute of France was held on the 25th of October, the anniversary of the day on which the Institute was established by the famous Directory suppressed by the first Napoleon. The third Napoleon, by an Imperial decree, changed the day of the anniversary meeting from that instituted by the Republic to his *fête* day, the 15th of August. Last year the meeting was not held, and on the present occasion the original date has been resumed. The presidency of the Institute is filled each year by the president of one of the five academies in rotation, the Académie des Sciences, Académie Française, Académie des Sciences Morales et Politiques, Académie des Beaux Arts, and Académie des Inscriptions et Belles Lettres. This year it is occupied by M. Jules Simon, president of the Académie Française, to which belong M. Thiers himself and four of his colleagues in the Government, including M. Simon. The annual address for the Académie des Sciences was delivered by General Morin, and dealt chiefly with military science, especially with the inventions of the great artillery officer General Piobert.

MR. J. J. MURPHY delivered the opening address to the Belfast Natural History and Philosophical Society for the current session. It was occupied chiefly with a *résumé* of the most important fresh applications of applied science during the year.

MR. RUTHERFORD, of New York, the most eminent American amateur astronomer, and especially known for his magnificent photographs of celestial bodies, has lately presented to Mr. Brothers, the English astronomical photographer, three superb negatives of the moon—one representing her in the first quarter, one when full, and one in the third quarter; and it is proposed to publish these in a volume containing about one hundred pages of descriptive letterpress. The work will also contain a map of the moon, as we see her, and a chart, on the stereographic projection, showing the true shape and the relative dimensions of all the chief lunar features. The letterpress, map, and stereographic chart will be prepared by Mr. Proctor; the photographs by Mr. Brothers. The work will be got out on a magnificent scale, and sold at a guinea and a half to subscribers.

MESSRS. TRÜBNER announce the proposed publication of a new magazine, *The Pioneer*; a monthly journal of Sociology, Psychology, and Biology. The great aim which the *Pioneer* has in view will be "the expression of truly philosophic principles, and their application to human progress and welfare. The opinions of all will be treated with respect when expressed with the clearness and force arising from strong conviction." The subjects of "Psychic Force" and Anthropology are especially alluded to in the prospectus as coming within the range of the proposed serial.

THE Geological Expedition to the Rocky Mountain region under the charge of Dr. Hayden, to which we have already made brief allusion, according to *Harper's Weekly*, had reached Fort Hall, Idaho, on the 18th of September. After completing the survey of the Yellow Stone Valley, the party left Fort Ellis on the 5th of September, passing down Gallatin Valley to the Three Forks, and thence by the Jefferson to its very source, exploring many of its branches, and pursuing a direction nearly parallel to that which the party had traversed in the June previous. The valleys of the Gallatin, Madison, and Jefferson forks of the Missouri, with all the little branches, were found occupied by industrious farmers and miners—a contrast quite striking to the doctor, who, twelve years ago, in exploring that same region, met with not a single white inhabitant. The Rocky Mountain Divide was crossed at the Horse Plain Creek, from which the party passed over into Medicine Lodge Creek, following this down into the Snake River Plain. An interesting fact observed was the occurrence of two species of trout in great quantities in streams such as Medicine Lodge, Camas, and other creeks all sinking into the plains after a course of from fifty to seventy-five miles. The trout appeared to be of the same two species in all, although the waters had no apparent connection. The party expected to leave Fort Hall, and to proceed to Fort Bridger by way of Soda Springs, Bear Lake, and Evanston, and there to disband the scientific corps returning to the East.

IN a very important paper on the "Estimation of Antimony," published in the *Chemical News*, Hugo Tamm calls the attention of chemists to a new phenomenon which the author describes under the name of "Hygraffinity." This phenomenon was discovered in a peculiar compound of antimony—bigallate of antimony. This compound is totally insoluble in water, and yet it possesses a powerful affinity for moisture, which it absorbs rapidly from the air after being dried at the temperature of 100° Cent. Most powders and precipitates, as it is well known, dried at that temperature, absorb moisture on exposure to the atmosphere, but this is a purely physical phenomenon due to porosity. On the contrary, in the case of gallate of antimony, chemical affinity is at work, and this precipitate, after exposure to the air for two or three hours, actually absorbs two equivalents of water. In a word, this insoluble substance has as much affinity for moisture as deliquescent salts. But one of the most curious features in connection with this extraordinary phenomenon is that on being dried at 100° Cent., bigallate of antimony loses the two equivalents of water which it had absorbed from the air, and that on being left exposed once more to the atmosphere, it reabsorbs the same amount of moisture. This interesting experiment may be repeated indefinitely.

IN the *Comptes Rendus* for August and in the *Philosophical Magazine*, M. Angström gives an analysis of the spectra which are observed in connection with hydrogen, and criticises the conclusions of M. Wüllner "that hydrogen has no less than four and oxygen no less than three distinct spectra." He explains that the spectrum lines of hydrogen (as observed by Plücker in rare hydrogen) spread out in disruptive discharges when the tension of the gas is increasing, and end by uniting so as to form a continuous spectrum. With regard to M. Wüllner's second spectrum of hydrogen, he points out that it is no other than the spectrum observed by M. Berthelot and ascribed by him to *acetylene*. Also, by a comparison of wave-lengths for sulphur and for M. Wüllner's third hydrogen-spectrum, he shows this to be in all probability the spectrum of sulphur. M. Angström also points out the close agreement between one of the oxygen spectra of M. Wüllner and the spectrum of oxide of carbon, and his tables show also a very close agreement between another of these oxygen spectra and the spectrum of chlorine, and concludes that neither oxygen nor hydrogen has more than one spectrum.

PROF. YOUNG has communicated to the *Philosophical Magazine* a catalogue of more than a hundred bright lines in the spectrum of the chromosphere, in which the observed lines are referred to the scales of Kirchhoff's and of Angström's maps. Of the seventy new lines which are given in this list, there are two which are proved to belong to the chromosphere, and not to be due to the exceptional elevation of matter to heights where it does not properly belong. No less than twenty of these lines are due to the metal titanium, and show the presence of titanium vapour in the prominences and chromosphere.

THE cultivation of beet-root sugar in France has now risen to an industry of the first importance. It employs more than 400 manufactories, and the process of manufacture is each year brought to a higher state of perfection. There are in France three or four journals specially devoted to subjects connected with the manufacture, its cultivation, its sale, the machinery required, the chemistry of the process, &c.

THE Fourth Annual Report is published of the Trustees of the Peabody Museum of American Archæology and Ethnology at Cambridge, U.S.A. Two important series of explorations have been carried out in the course of the past year on behalf of the Museum, by the Rev. F. O. Dunning in Eastern Tennessee, and by Dr. Berendt in Central America, resulting in valuable acquisitions to its collections. The Museum has also been enriched during the year by the gift of the "Charles Hammond Collection" from the towns of Chatham and Rochester, Cape Cod, and by a very valuable series of about 125 objects from the conservator of the Christy collection in London, consisting of original specimens and casts from Les Eyzies, La Madelaine, and Le Moustier, in the department of Dordogne, France. The Report is accompanied by a set of comparative measurements of crania from Peru, presented by Mr. Squier, of those from the mounds of Kentucky obtained by Mr. Lyon, and from the mounds of Florida.

THE Annual *Conversazione* of the Royal Society of Victoria was held on August 14, when the president, Mr. R. L. J. Ellery, delivered an address, in which he referred especially to the scientific results of the eclipse of last winter, and the preparations making in Australia for observing the eclipse of next month, to Prof. Heis's observations on the correspondence of auroral phenomena in the southern and northern hemispheres, to Dr. von Mueller's botanical researches in the colony, to the very important subject, economically, to the colony of the preservation of meat, and to Prof. Tyndall's germ theory of disease.

THE Report is published of the Annual Meeting of the Academy of Sciences of Vienna, held on the 30th of May, 1871, containing a review of the proceedings of the various departments of the Academy during the past year. The Academy has also issued its "Almanack," with list of home, foreign, and honorary members.

A SUPPLEMENT to the Sixth and Seventh Annual Report of the "Verein für Erdkunde" at Dresden, by D. Abendroth, contains a very interesting series of maps, illustrating the extent of geographical knowledge of the world possessed at different periods from A.D. 1350 to 1566.

A WORK has come out in Holland which particularly interests those who are engaged in the treatment of sewage manure. It is by M. J. A. C. Eschauzler, and gives all the results of the centuries of experience in the Netherlands. It is copiously illustrated.

WE are informed that the German translation of Tylor's "Primitive Culture" is not by Dr. Spengel, but conjointly by Herr Spengel and Herr Poske.

A NEW class for civil engineering has been formed in the Presidency College, Calcutta.

THE Madras Government has allowed 200*l.* for the expense of bringing the Assistant Government Astronomers to England to learn celestial photography.

*THE GEOGNOSY OF THE APPALACHIANS AND THE ORIGIN OF CRYSTALLINE ROCKS**

II.

THE characteristic examples already given of symmetrical and asymmetrical envelopment are cited from a great number of others which might have been mentioned. Very many of these are by the pseudomorphists regarded as results of partial alteration. Thus, in the case of associated crystals of andalusite and cyanite, Bischof does not hesitate to maintain the derivation of andalusite from the latter species by an elimination of quartz; more than this, as the andalusite in question occurs in a granite-like rock, he suggests that itself is a product of the alteration of orthoclase. In like manner the mica, which in some cases coats tourmaline, and in others fills hollow prisms of this mineral, is supposed to result from a subsequent alteration of crystallised tourmaline. So in the case of shells of leucite filled with feldspar, or of garnet enclosing epidote or chlorite or quartz, a similar transformation of the interior is supposed to have been mysteriously effected, while the external portion of the crystal remains intact. Again the aggregates of tinstone, quartz and orthoclase having the form of the latter, are, by Bischof and his school, looked upon as results of a partial alteration of previously formed orthoclase crystals. It needed only to extend this view to the crystals of calcite enclosing sand-grains, and regard these as the result of a partial alteration of the carbonate of lime. There is absolutely no proof that these hard crystalline substances can undergo the changes supposed, or can be absorbed and modified like the tissues of a living organism. It may, moreover, be confidently affirmed that the obvious facts of envelopment are adequate to explain all the cases of association upon which this hypothesis of pseudomorphism by alteration has been based. Why the change should extend to some parts of a crystal and not to others, why in some cases the exterior of the crystal is altered, while in others the centre alone is removed and replaced by a different material, are questions which the advocates of this fanciful hypothesis have not explained. As taught by Blum and Bischof, however, these views of the alteration of mineral species have not only been generally accepted, but have formed the basis of the generally received theory of rock-metamorphism.

Protests against the views of this school have, however, not been wanting. Scheerer, in 1846, in his researches in Polymeric Isomorphism,† attempted to show that iolite and aspasiolite, a hydrous species which had been looked upon as resulting from its alteration, were isomorphous species crystallising together, and, in like manner, that the association of olivine and serpentine in the same crystal, at Snarum in Norway, was a case of envelopment of two isomorphous species. In both of these instances he maintained the existence of isomorphous relations between silicates in which 3HO replaced MgO. He hence rejected the view of Gustav Rose that these serpentine crystals were results of the alteration of olivine, and supported his own by reasons drawn from the conditions in which the crystals occur. In 1853 I took up this question, and endeavoured to show that these cases of isomorphism described by Scheerer entered into a more general law of isomorphism pointed out by me among homologous compounds differing in their formulas by nM_2O_2 (M = hydrogen or a metal). I insisted, moreover, on its bearing upon the received views of the alteration of minerals, and remarked, "The generally admitted notions of pseudomorphism seem to have originated in a too exclusive plutonism, and require such varied hypotheses to explain the different cases, that we are led to seek for some more simple explanation, and to find it, in many instances, in the association and crystallising together of homologous and isomorphous species."‡ Subsequently, in 1860, I combated the view of Bischof, adopted by Dana, that "regional metamorphism is pseudomorphism on a grand scale," in the following terms:—

"The ingenious speculations of Bischof and others, on the possible alteration of mineral species by the action of various saline and alkaline solutions, may pass for what they are worth, although we are satisfied that by far the greater part of the so-called cases of pseudomorphism in silicates are purely imaginary, and, when real, are but local and accidental phenomena. Bischof's notion of the pseudomorphism of silicates like feldspars and py-

* Address of Prof. T. Sterry Hunt on retiring from the office of President of the American Association for the Advancement of Science; abridged from the "American Naturalist."

† Pogg. Annal., lxviii. 319.

‡ Ibid.

roxenes, presupposes the existence of crystalline rocks, whose generation this neptunist never attempts to explain, but takes his starting-point from a plutonic basis."

I then asserted that the problem to be solved in regional metamorphism is the conversion of sedimentary strata, "derived by chemical and mechanical agencies from the ocean waters and pre-existing crystalline rocks into aggregations of crystalline silicates. These metamorphic rocks, once formed, are liable to alteration only by local and superficial agencies, and are not, like the tissues of a living organism, subject to incessant transformations, the pseudomorphism of Bischof."*

I had not, at that time, seen the essay by Delesse on pseudomorphs already referred to, published in 1859, in which he maintained views similar to those set forth by me in 1853 and 1860, declaring that much of what had been regarded as pseudomorphism had no other basis than the observed associations of minerals, and that often "the so-called metamorphism finds its natural explanation in envelopment." These views he ably and ingeniously defended by a careful discussion of the whole range of facts belonging to the history of the subject.

My own expression of opinion on this question, in 1853, had been privately criticised, and I had been charged with a want of comprehension of the question. It was, therefore, with no small pleasure, that I not only saw my views so ably supported by Delesse, but read the language of Carl Friedrich Naumann, who in 1861 wrote to Delesse as follows, referring to his essay just noticed :—

"You have rendered a veritable service to science in restricting pseudomorphs to their true limits, and separating what had been erroneously united to them. As you have remarked, envelopments have, for the most part, nothing in common with pseudomorphs, and it is inconceivable that they have been united by so many mineralogists and geologists. It appears to me, moreover, that they commit an analogous error when they regard gneisses, amphibolites, &c., as being, all of them, the results of metamorphic epigenesis, and not original rocks. It is precisely because pseudomorphism has been so often confounded with metamorphism that this error has found acceptance. I only admit a pseudomorph where there is some crystal the form of which has been preserved. There are very many metamorphic substances which are, in no sense of the word, pseudomorphs. Had the name of crystalloid been chosen instead of pseudomorph, this confusion would certainly have never found its way into the science. I think, with you, that the envelopment of two minerals is most generally explained by a contemporaneous and original crystallisation. Secondary envelopments, however, exist, and such may be called pseudomorphs or crystalloids, if they reproduce exactly the form of the crystal enveloped, whether this last still remains, or has entirely disappeared."†

It is unnecessary to remark that the view of Delesse and Naumann—viz. : that the so-called cases of pseudomorphism, on which the theory of metamorphism by alteration has been built, are, for the most part, examples of association and envelopment, and the result of a contemporaneous and original crystallisation—is identical with the view suggested by Scheerer, and generalised by myself long before, when, in 1853, I sought to explain the phenomena in question by "the association and crystallising together of homologous and isomorphous species."

Later in 1862, I wrote as follows :—

"Pseudomorphism, which is the change of one mineral species into another, by the introduction or the elimination of some element or elements, presupposes metamorphism (*i.e.*, metamorphic or crystalline rocks), since only definite mineral species can be the subjects of this process. To confound metamorphism with pseudomorphism, as Bischof and others after him have done, is therefore an error. It may be further remarked, that, although certain pseudomorphic changes may take place in some mineral species, in veins and near the surface, the alteration of great masses of silicated rocks by such a process is as yet an unproved hypothesis."‡

Thus this unproved theory of pseudomorphism, as taught by Bischof, does not, even if admitted to its fullest extent, advance us a single step toward a solution of the problem of the origin of the various silicates, which, singly or intermingled, make up beds in the crystalline schists. Granting, for the sake of argument, that serpentine results from the alteration of olivine or labradorite, and steatite or chlorite from hornblende, the origin of these anhydrous silicates, which are the subjects of the supposed change, is still unaccounted for. The explanation of this shortsightedness is not far to seek ; as already remarked, Bischof, although a professed neptunist, starts from a plutonic basis. When the epigenic origin of serpentine and its related rocks was first taught, these were regarded as eruptive and unstratified, and it was easy to imagine intruded masses of dioritic and feldspathic rocks, which had become the subjects of alteration. As, however, the progress of careful investigation in the field has shown the stratified character of these serpentines, diallage-rocks, steatites, &c., and their intercalation among limestones, argillites, quartzites, gneisses, and mica-schists, and even among feldspathic and hornblendic strata, we are forced to reject, with Naumann, the notion of their epigenic derivation, and to regard them as original rocks.

This view brings us face to face with the problem of metamorphism as defined by me in 1860* (see *ante*). We must either admit that these crystalline schists were created as we find them, or suppose that they were once sands, clays, marls, &c. ; in a word, sediments of chemical and mechanical origin, which by a subsequent process have been consolidated and crystallised. Whence, then, come these silicates of magnesia, lime, and iron, which are the sources of serpentine, hornblende, steatite, chlorite, &c.? This is the question which I proposed in that same year, when, after discussing the results of my examinations of the tertiary rocks near Paris containing layers of a hydrous silicate of magnesia related to talc in composition, among unaltered limestones and clays, I remarked that it is evident "such silicates may be formed in basins at the earth's surface, by reactions between magnesian solutions and dissolved silica ;" and, after some further discussion, said, "further inquiries in this direction may show to what extent certain rocks composed of calcareous and magnesian silicates may be directly formed in the moist way."† Subsequently, in a paper on "The Origin of some Magnesian and Aluminous Rocks," printed in the "Canadian Naturalist" for June 1860,‡ I repeated these considerations, referring to the well-known fact that silicates of lime, magnesia, and iron-oxyd are deposited during the evaporation of natural waters, including those of alkaline springs and of the Ottawa River. Having described the mode of occurrence of the magnesian silicate sepiolite, in the Paris basin, and the related quincite, containing some iron-oxyd and disseminated in limestone, I suggested that while steatite has been derived from a compound like sepiolite, the source of serpentine was to be sought in another silicate richer in magnesia ; and, moreover, that chlorite, unless the result of a subsequent reaction between clay and carbonate of magnesia, was directly formed by a process analogous to that which (according to Scheerer) has, in recent times, caused the deposition from waters of neolite, a hydrous alumino-magnesian silicate, approaching to chlorite in composition,§ "the type of a reaction which formerly generated beds of chlorite in the same way as those of sepiolite or talc." Delesse, subsequently, in 1861, in his essay on Rock-Metamorphism, insisted upon the sepiolites or so called magnesian marls, as probably the source of steatite, and suggested the derivation of serpentine, chlorite, and other related minerals of the crystalline schists, from deposits approaching these marls in composition.‖ He recalled, also, the occurrence of chromic oxyd, a frequent accompaniment of these magnesian minerals, in the hydrated iron ores of the same geological horizon with the magnesian marls in France. Delesse did not, however, attempt to account for the origin of these deposits of magnesian marls, in explanation of which I afterwards verified Bischof's observations on the sparing solubility of silicate of magnesia, and showed that silicate of soda, or even artificial hydrated silicate of lime, when added to waters containing magnesian chlorid or sulphate, gives rise, by double decomposition, to a very insoluble magnesian silicate.¶

To explain the generation of silicates like labradorite, scapolite, garnite, and saussurite, I suggested that double aluminous silicates allied to the zeolites might have been formed, and subsequently rendered anhydrous. The production of zeolitic minerals observed by Daubrée at Plombières and Luxeuil by the action of a silicated alkaline water on the masonry of ancient Roman baths, was appealed to by way of illustration. It had

* Amer. Jour. Sci., II. xxx. 135.
† Bull. Soc. Geol. de France, II. xviii. 678.
‡ Descriptive Catalogue, Crystalline Rocks of Canada, p. 80, London Exhibition, 1862 : also Dublin Quar. Jour., July 1863, and Amer. Jour. Sci., II. xxxvi. 218.

* Amer. Jour. Sci., II. xxx. 135.
† Ibid., II. xxix. 284 ; also II. xl. 49.
‡ Ibid., II. xxxii. 286.
§ Pog. Annal., lxxi. 288.
‖ Etudes sur le Metamorphisme, 4to, pp. 91. Paris, 1861.
¶ Amer. Jour. Sci., II. xl. 49.

there been shown by Daubrée that the elements of the zeolites had been derived in part from the waters, and in part from the mortar, and even the clay of the bricks, which had been attacked, and had entered into combination with the soluble matters of the water to form chabazite. I, however, at the same time pointed out another source of silicated minerals, upon which I had insisted since 1857, viz., the reaction between silicious or argillaceous matters and earthy carbonates in the presence of alkaline solutions. Numerous experiments showed that when solutions of an alkaline carbonate were heated with a mixture of silica and carbonate of magnesia, the alkaline silicate formed acted upon the latter, yielding a silicate of magnesia, and regenerating the alkaline carbonate; which, without entering into permanent combination, was the medium through which the union of the silica and the magnesia was effected. In this way I endeavoured to explain the alteration, in the vicinity of a great intrusive mass of dolerite, of a gray Silurian limestone, which contained, besides a little carbonate of magnesia and iron-oxyd, a portion of very silicious matter, consisting apparently of comminuted orthoclase and quartz. In place of this, there had been developed in the limestone, near its contact with the dolerite, an amorphous greenish basic silicate, which had seemingly resulted from the union of the silica and alumina with the iron-oxyd, the magnesia, and a portion of lime. By the crystallisation of the products thus generated it was conceived that minerals like hornblende, garnet, and epidote might be developed in earthy sediments, and many cases of local alteration explained. Inasmuch as the reaction described required the intervention of alkaline solutions, rocks from which these were excluded would escape change, although the other conditions might not be wanting. The natural associations of minerals, moreover, led me to suggest that alkaline solutions might favour the crystallisation of aluminous silicates, and thus convert mechanical sediments into gneisses and mica-schists. The ingenious experiments of Daubrée on the part which solutions of alkaline silicates, at elevated temperatures, may play in the formation of crystallised minerals, such as feldspar and pyroxene, were posterior to my early publications on the subject, and fully justified the importance which, early in 1857, I attributed to the intervention of alkaline silicates in the formation of crystalline silicated minerals.*

While, however, there is good reason to believe that solutions of alkaline silicates or carbonates have been efficient agents in the crystallisation and molecular re-arrangement of ancient sediments, and have also played an important part in the local alteration of sedimentary strata which is often observed in the vicinity of intrusive rocks, it is clear to me that the agency of these solutions is less universal than was once supposed by Daubrée and myself, and will not account for the formation of various silicated rocks found among crystalline schists, such as serpentine, hornblende, steatite, and chlorite. When I commenced the study of these crystalline strata, I was led, in accordance with the almost universally received opinion of geologists, to regard them as resulting from a subsequent alteration of palæozoic sediments, which, according to different authorities, were of Cambrian, Silurian, or Devonian age. Thus in the Appalachian region, as we have already seen, they have, on supposed stratigraphical evidence, been successively placed at the base, at the summit, and in the middle of the Lower Silurian or Champlain division of the New York system. A careful chemical examination among the unaltered palæozoic sediments, which in Canada were looked upon as the stratigraphical equivalents of the bands of magnesian silicates in these crystalline schists, showed me, however, no magnesian rocks except certain silicious and ferruginous dolomites. From a consideration of reactions which I had observed to take place in such admixtures in presence of heated alkaline solutions, and from the composition of the basic silicates which I had found to be formed in silicious limestones near their contact with eruptive rocks, I was led to suppose that similar actions, on a grand scale, might transform these silicious dolomites of the unaltered strata into crystalline magnesian silicates.

Further researches, however, convinced me that this view was inapplicable to the crystalline schists of the Appalachians; since, apart from the geognostical considerations set forth in the previous part of this paper, I found that these same crystalline strata hold beds of quartzose dolomite and magnesian carbonate, associated in such intimate relations with beds of serpentine, diallage, and steatite, as to forbid the notion that these silicates could have been generated by any transformations or chemical re-arrangement of mixtures like the accompanying beds of quartzose magnesian carbonates. Hence it was that already, in 1860, as shown above, I announced my conclusion that serpentine, chlorite, and steatite had been derived from silicates like sepiolite, directly formed in waters at the earth's surface, and that the crystalline schists had resulted from the consolidation of previously formed sediments, partly chemical and partly mechanical in their origin. The latter being chiefly silico-aluminous, took, in part, the forms of gneiss and mica-schists, while from the more argillaceous strata, poorer in alkali, much of the aluminous silicate crystallised as andalusite, staurolite, cyanite, and garnet. These views were reiterated in 1863,* and further in 1864, in the following language, as regards the chemically-formed sediments: "steatite, serpentine, pyroxene, hornblende, and in many cases, garnet, epidote, and other silicated minerals are formed by a crystallisation and molecular re-arrangement of silicates generated by chemical processes in waters at the earth's surface."† Their alteration and crystallisation were compared to that of the mechanically formed feldspathic, silicious, and argillaceous sediments just mentioned.

(*To be continued.*)

* Proc Roy. Soc., May 7, 1857. Amer. Jour. Sci., II. xxiii. 438, and xxv. 289 and 435.

THE RELATIONS BETWEEN ZOOLOGY AND PALÆONTOLOGY‡

MY distinguished predecessor, the late Prof. E. Forbes, appears to have been the first who undertook the systematic study of marine zoology with reference to the distribution of marine animals in space and in time. After making himself well acquainted with the fauna of the British seas to the depth of about 200 fathoms by dredging, and by enlisting the active co-operation of many friends, among whom we find MacAndrew, Barlee, Gwyn Jeffreys, William Thompson, and many others, entering enthusiastically into the new field of natural history inquiry; in the year 1841, Forbes joined Captain Graves, who was at that time in command of the Mediterranean Survey as naturalist. During about eighteen months he studied with the utmost care the conditions of the Ægean and its shores, and conducted upwards of 100 dredging operations at depths varying from 1 to 130 fathoms. In 1843 he communicated to the Cork meeting of the British Association an elaborate report on the mollusca and radiata of the Ægean Sea, and on their distribution as bearing on geology. Three years later, in 1846, he published in the first volume of the "Memoirs of the Geological Survey of Great Britain," a most valuable memoir upon the connection between the existing Fauna and Flora of the British Isles and the geological changes which have affected their area, especially during the epoch of the northern drift. In the year 1859 appeared the "Natural History of the European Seas," by the late Prof. Edward Forbes, edited and continued by Robert Godwin-Austen. In the first hundred pages of this little book Forbes gives a general outline of some of the more important of his views with regard to the distribution of marine forms. The remainder of the book is a continuation by his friend Mr. Godwin-Austen, for before it was finished an early death had cut short the career of the most accomplished and original naturalist of his time. I will give a brief sketch of the general result to which Forbes was led by his labours, and I shall have to point out that, although we are now inclined to look somewhat differently on certain very fundamental points, and, although recent investigations with better appliances and more extended experience have invalidated many of his conclusions, to Forbes is due the credit of having been the first to treat these questions in a broad philosophical sense, and to point out that the only means of acquiring a true knowledge of the *rationale* of the distribution of our present fauna is to make ourselves acquainted with its history, to connect the present with the past. This is the direction which must be taken by future inquiry:—Forbes as a pioneer in this line of research was scarcely in a position to appreciate the full value of his work. Every year adds enormously to our stock of data, and every new fact indicates more and more clearly the brilliant results which are to be obtained by following his methods, and by emulating his enthusiasm and his indefatigable industry. Forbes believed implicitly, along with nearly all the leading naturalists of his time, in the immutability

* Geol. of Canada, pp. 577—581.

† Amer. Jour. Sci., II. xxxvii. 266, and xxxviii. 183.

‡ Abstract of Opening Lecture on Natural History delivered at the University of Edinburgh, Nov. 2, by Prof. Wyville Thomson, F.R.S.

of species. He says:—"Every true species presents in its individuals certain features, specific characters, which distinguish it from every other species: as if the Creator had set an exclusive mark or seal on each type." He likewise believed in specific centres of distribution. He held that all the individuals composing a species had descended from a single progenitor, or from two, according as the sexes might be united or distinct, and that, consequently, the idea of a species involved the idea of the relationship in all the individuals of common descent; and the converse, that there could by no possibility be community of descent except in living beings which possessed the same specific characters. He supposed that the original individual or pair was created at a particular spot where the conditions were suitable for its existence and propagation, and that the species extended and migrated from that spot on all sides, over an area of greater or less extent, until it met with some natural barrier in the shape of unsuitable conditions. No specific form could have more than a single centre of distribution. If its area appeared to be broken up, a patch not in connection with the original centre of distribution occurring in some distant locality, it was accounted for by the formation, through some geological change, after the first spread of the species, of a barrier which cut off part of its area, or by some accidental transport to a place where the conditions were sufficiently similar to those of its original habitat to enable it to become naturalised. No species once exterminated was ever re-created, so that in those few cases in which we find a species abundant at one period over an area, absent over the same area for a time, and recurring at a later period, it must be accounted for by a change in the conditions of the area which forced the emigration of the species, and a subsequent further change which permitted its return. Forbes defined and advocated what he called the law of "representation." He found that in all parts of the world, however far removed, and however completely separated by natural barriers, where the conditions of life are similar, species, and groups of species, occur, which, although not identical, resemble one another very closely; and he found that this similarity existed likewise between groups of fossil remains and between groups of fossils and groups of recent forms. Admitting the constancy of specific characters, these resemblances could not be accounted for by community of descent, and he thus arrived at the generalisation that in localities placed under similar circumstances, similar, though specifically distinct, specific forms were created. These he regarded as mutually representative species. Our acceptance of the doctrines of "specific centres" and of "representation," or at all events the form in which we may be inclined to accept them, depends greatly upon the acceptance or rejection of the fundamental dogma of the immutability of species, and on this point there has been a very great change of opinion within the last ten or twelve years—a change certainly due to the remarkable ability and candour with which the question has been discussed by Mr. Darwin and Mr. Wallace. I do not think that I am speaking too strongly when I say that there is now scarcely a single competent general naturalist who is not prepared to accept some form of the doctrine of evolution. There are no doubt very great difficulties in the minds of many of us in conceiving that, commencing from the simplest living being, the present state of things in the organic world has been produced solely by the combined action of "atavism," the tendency of offspring to resemble their parents closely, and "variation," the tendency of offspring to differ individually from their parents within very narrow limits; and many are inclined to believe that some law, as yet undiscovered, other than the "survival of the fittest" must regulate the existing marvellous system of extreme and yet harmonious modification. Still, it must be admitted that variation is a *vera causa*, probably capable, within a limited period, under favourable circumstances, of converting one species into what, according to our present ideas, we should be forced to recognise as a different species; and such being the case, it is perhaps conceivable that during the lapse of a period of time—still infinitely shorter than eternity—variation may have produced the entire result. The individuals composing a species have a definite range of variation strictly limited by the circumstances under which the group of individuals is placed. Except in man and in domesticated animals, in which it is artificially increased, this individual variation is usually so slight as to be inappreciable except to a practised eye; and any extreme variation which passes the natural limit in any direction clashes in some way with surrounding circumstances, and is dangerous to the life of the individual. The normal or graphic line, or "line of safety," of the species, lies midway between the extremes of variation. If at any period in the history of a species, the conditions of life of a group of individuals of the species are gradually altered; with the gradual change of circumstances the limit of variation is contracted in one direction and relaxed in another, it becomes more dangerous to diverge towards one side, and more desirable to diverge towards the other, and the position of the lines limiting variation is altered. The normal line, the line along which the specific characters are most strongly marked, is consequently slightly deflected, some characters being more strongly expressed at the expense of others. This deflection, carried on for ages in the same direction, must eventually carry the divergence of the varying race far beyond any limits within which we are in the habit of admitting identity of species. But the process must be, so to speak, infinitely slow. It is difficult to form any idea of ten, fifty, or a hundred millions of years; or of the relation which such periods bear to changes taking place in the organic world. We must remember, however, that the rocks of the Silurian system, overlaid by ten miles thickness of sediment, entombing a hundred successive faunæ, each as rich and varied as that of the present day, are themselves teeming with fossils fully representing all the existing classes of animals except the very highest. If it is possible to imagine that this marvellous manifestation of eternal power and wisdom involved in living nature can have been worked out through the law of "descent with modification" alone, we shall certainly require from the physicists the very longest row of cyphers which they can afford. Now, although the admission of a doctrine of evolution must affect greatly our conception of the origin and *rationale* of so-called specific centres, it does not practically affect the question of their existence, or of the laws regulating the distribution of species from these centres by migration, by transport, by ocean currents, by elevations or depressions of the land, or by any other causes at work under existing circumstances. So far as practical naturalists are concerned, species are permanent within their narrow limits of variation, and it would introduce an element of infinite confusion and error if we were to regard them in any other light. The origin of species by "descent with modification" is as yet only a hypothesis. During the whole period of recorded human observation, not one single instance of the change of one species into another has been detected, and, singular to say, in successive geological formations, although new species are constantly appearing, and there is abundant evidence of progressive change, no single case has as yet been observed of one species passing through a series of inappreciable modifications into another.

ON THE OBJECTS AND MANAGEMENT OF PROVINCIAL MUSEUMS *

ALTHOUGH every intelligent person knows more or less what these institutions are, and what they ought to be, there is probably no subject, connected with the modern means of education in natural science, concerning which so much misconception or ignorance is manifested and tolerated as in the Management and Objects of our Provincial Museums. The majority of them throughout England present such examples of helpless misdirection and incapacity as could not be paralleled elsewhere in Europe. Some noteworthy exceptions there are. But generally the managers or guardians of local museums are precisely of this unfit class, and seem to have no more notion of their charge than as mere curiosity-shops, and even display less intelligence than is shown in such shops, where the cupidity or shrewdness of the dealer induces him at least to take due care of, and give a local habitation and a name to, his wares. But in the provincial museums even this care and tittle of information is pertinaciously withheld, and the visitors are left to do the best they can amid the surrounding bewilderment. This is commonly made up of a most puzzling jumble of heterogeneous miscellanies, arranged, or rather scattered, with an equally sovereign contempt for the convenience or instruction of the public, and indeed all in such admired disorder as may most plainly show how Chaos is come again and Confusion can make his masterpiece, and how every specimen added to the heap only tends to increase or perpetuate the miserable derangement. It looks as

* Abstract of an Address to a Meeting of the East Kent Natural History Society, at Canterbury, Oct. 12, 1871, by its Vice-President and Honorary Secretary, George Gulliver, F.R.S.

if the presiding local genius had set his wits to work in order to prove how much time and money might be most effectually expended with the least profit to a knowledge of the natural history, or any history, of the neighbourhood; and indeed for exemplifications of the solution of this knotty point we have too commonly only to appeal to the museum of the place. Instead of methodical illustrations of the natural history and antiquities of the district, we are likely to find a few good things overlaid by such a rabble-rout, such a multifarious and disorderly medley of outlandish and queer odds and ends, as are rather fitted for a laughing-stock than a sober exposition of science. Thus we are met at once in the hall and saloons by such incongruous lots as effigies of double women, elephants' teeth, nose-rings, brain-stones, tomahawks, stuffed alligators, moccasins, New Zealanders' heads, cockatoos, canoes, Babylonish bricks, cocoa nuts, boas, javelins, lions and tigers, calumets, matchlocks, palm-branches, shields, monkey-stones, sugar-canes, Roman cement, Oliver Cromwell's watches, Panama hats, fabricated elephants, walking-stick insects, and numberless other eccentric things of this motley and confounded order. The garniture of Romeo's apothecary's shop, or the countryman's museum on the barn door, would be more instructive or intelligible and less ridiculous or perplexing.

It might be painful or appear invidious to inquire minutely by what means or under whose misconduct so many provincial museums have sunk into their present disgraceful confusion and uselessness; especially as it is little creditable to the intelligence of that community under the tolerance or approval of which this reproachful state of things exists. If the fault be attributed to the apathy or something worse among the majority of the ratepayers, it is one that the friends of popular government should hasten to correct. However this may be, it is enough for us to know that this notorious evil has increased, is increasing, and ought to be diminished; it will otherwise remain a foul blot on and a costly nuisance to the places under such unprofitable infliction. Hence every naturalist and antiquarian, every intelligent and honest member of the community, should be ready to lend his hand cordially to the good work of reform in this direction; more especially as soon as the truth is realised that the difficulty is by no means insuperable, but may be easily removed, is a consummation devoutly to be wished, and would involve no addition to the customary and regular expense. The remedies are sufficiently obvious, and to point out how they should be used, after having described the disorder and the necessity for them, is the object of the present observations. To this end we have in the first place to consider what is desirable and practicable. To instruct ourselves and the rising generation, by means of local museums, in the elements of natural history generally, and in the local examples of it particularly, is obviously both practicable and desirable. For the first purpose, when indigenous specimens are wanting we must get exotic ones; and these should be limited to such typical examples only as are absolutely necessary for the elucidation of fundamental or comprehensive facts; for which purpose anatomical preparations, whether botanical or zoological, are chiefly, but not exclusively, to be esteemed. On the other hand, all and every species belonging to the district should be preserved and displayed so far as they admit it; partly for the knowledge they display of the science, but principally for the information they afford of the natural history of the locality. Antiquarian objects should be treated in a similar spirit. Thus would be collected at one view, or at least under one roof, much of that important knowledge which is within the means and scope of any country museum, so that every visitor to it might easily find therein both pleasure and profit in natural science in general and in the natural features of the locality in particular. The museum would then also be in a condition to fulfil one of its leading offices, as a centre for the meetings, lectures, and conversations on the natural history and antiquities of the district, and in this mode be available for contributions in furtherance of the special objects of local societies, and likely thus to add to the general stock of knowledge. And happily, this is now being regularly ventilated and popularised in such useful publications as the *Zoologist*, the *Field*, and *Land and Water*. When will the *Times* discover the fair and fertile field of instruction in the Provincial Museums, now lying waste for want of culture? NATURE, in a recent notice of certain donations to the Ludlow Museum, has shown a judicious sense of the subject.

But how are you to get the desirable specimens, and what are you to do with them? Most of those wildernesses miscalled Museums already possess a large quantity of objects only awaiting and inviting intelligent attention. This will consist in a careful preparation, display, and description of them. After having been separately grouped under their respective kingdoms—the mineral, vegetable, and animal—they must be arranged according to the method of their natural relations, in their respective classes, orders, families, genera, and species; then accurately numbered, ticketed, and catalogued. Thus the otherwise chaotic mass of particular facts will fall into an orderly method, and be always ready to convey an accurate knowledge to visitors. Still further illustrations will be requisite, especially as regards fundamental and comprehensive phenomena, by preparations to display the essential characters at least of the classes and orders, and of the anatomy and physiology of the members thereof; and one or two careful dissections will be commonly sufficient for this purpose in each order. And now will arise the question, Who is to do all this work? Certainly neither by nor under the direction of "incorporations" of aldermen quite incapable of it can we expect any effectual labour of the kind. But with proper encouragement students of the different departments will, from a pure love of the subjects, not only be found to perform all this but probably more, and without the least expectation of any pecuniary reward. They will surely add important preparations and other objects to the collection, whenever it becomes manifest that such contributions will be duly appreciated and cared for; indeed, with regard to at least one Museum very zealous and skilful naturalists have only been prevented from giving such desirable aid by a knowledge that their work would simply be "missing," smothered, or destroyed, amid the carelessness and the maze of misplaced rubbish there undergoing a like fate, and most significantly and effectually warning them, and others like them, what they have to expect. Fortunately minerals and antiquities are commonly less perishable.

Having discussed what is desirable and practicable, we come to that which is neither one nor the other. And having somewhat irreverently adverted to the rubbish of so many Provincial Museums, a further explanation may be necessary, and the more so as this very accumulation of jumbled and useless materials is the sad *bête noire* of these collections, and so vigilantly intrusive as to force admission and predominance against all reasons of fitness or utility. Any disorderly materials when hurtful by being out of place fall into the character of rubbish, just as any plant is a weed when encroaching injuriously on the legitimate crop. In their proper place they may be very valuable; such they might be in the great general collection of the British Museum, or in a botanical garden. But nobody in his senses can suppose that it is either desirable or practicable for a provincial society to attempt an imitation of the vast and boundless metropolitan institution. This would be simply out of the question, and calculated only to provoke a smile, except peradventure among the guardians of the local museums. Indeed, with all the excellent arrangement, the army of properly paid experts, and immense space and appliances, the British Museum has become so crowded and unwieldy, especially for reference and use concerning British products, that some steps for an extrication of them from the surrounding masses of exotic things has become necessary. But the guardians of the Provincial Museum will reasonably ask, Granting that we have so much rubbish, what are we to do with it? Sell it if you can, or give it away; but by all means get rid of it, and that swiftly; to which end a bonfire might be the best thing. And having thus learned by experience the noxiousness of such rubbish, most resolutely and remorselessly refuse any quarter to it in future. At present this sort of lumber only occupies space and involves expense that might and ought to be employed for more useful and legitimate purposes; and how and why has already been mentioned. At the execution of the sentence many a wailing throe will out, some natural tears be shed, for the o'erfraught heart will speak. The very civil and complacent local genius will meekly plead for his idols, telling you how he loves them, and how some other equally wise and more potent individuals hold the same faith; and above all that the visitors to his temple have ever regarded all those very things with an admiration and delight amounting to veneration. He will refuse to be comforted by your assurance that what he says is no doubt very true, though Punch and Judy and Madame Tussaud may be almost as delightful if not quite as good in their way; but that your way is to show how the Provincial Museum may be made not to suppress or degrade but to develope and elevate the taste of the multitude; and that after all a good museum will sooner or later become more popular than a bad one.

THE SCOTTISH SCHOOL OF GEOLOGY *

I.

FOR the first time in the history of University Education in Scotland, we are to-day met to begin the duties of a Chair specially devoted to the cultivation of Geology and Mineralogy. Though Science is of no country nor kin, it yet bears some branches which take their hue largely from the region whence they sprang, or where they have been most closely followed. Such local colourings need not be deprecated, since they are both inevitable and useful. They serve to bring out the peculiarities of each climate, or land, or people, and it is the blending of all these colourings which finally gives the common neutral tint of science. This is in a marked degree true of Geology. Each country where any part of the science has been more particularly studied, has given its local names to the general nomenclature, and its rocks have sometimes served as types from which the rocks of other regions have been classified and described. The very scenery of the country, reacting on the minds of the early observers, has sometimes influenced their observations, and has thus left an impress on the general progress of the science. As we enter to-day upon a new phase in the history of Geology among us here, it seems most fitting that we should look back for a little at the past development of the science in this country. There was a time, still within the memory of living men, when a handful of ardent original observers here carried geological speculation and research to such a height as to found a new, and, in the end, a dominant shool of Geology.

In the history of the Natural Sciences, as in that of Philosophy, there have been epochs of activity and then intervals of quiescence. One genius, perhaps, has arisen and kindled in other minds the flame that burned so brightly in his own. A time of vigorous research ensued, but as the personal influence waned, there followed a period of feebleness or torpor until the advent of some new awakening. Such oscillations of mental energy have an importance and a significance far beyond the narrow limits of the country or city in which they may have been manifested. They form part of that long and noble record of the struggle of man with the forces of nature, and deserve the thoughtful consideration of all who have joined or who contemplate joining in that struggle. I propose on the present occasion to sketch to you the story of one of these periods of vigorous originality, which had its rising and its setting at Edinburgh—the story of what may be called the Scottish School of Geology. I wish to place before you, in as clear a light as I can, the work which was accomplished by the founders of that school, that you may see how greatly it has influenced, and is even now influencing, the onward march of the science. I do this in no vainglorious spirit, nor with any wish to exalt into prominence a mere question of nationality. Science knows no geographical or political limits. Nor, though we may be proud of what has been achieved for Geology in this little kingdom, can we for a moment shut our eyes to the fact that these achievements are of the past, that the measure of the early promise at the beginning of this century has been but scantily fulfilled in Scotland, and that the state of the science among us here, instead of being in advance, is rather behind the time. And thus I dwell now on the example of our predecessors, solely in the hope that, realising to ourselves what that example really was, we may be stimulated to follow it. The same hills and valleys, crags and ravines, remain around us which gave these great men their inspiration, and still preach to us the lessons which they were the first to understand.

The period during which the distinctively Scottish School of Geology rose and flourished may be taken as included between the years 1780 and 1825—a brief half-century. Previous to that time Geology, in the true sense of the word, can hardly be said to have existed. Steno, indeed, more than a hundred years before, had shown, from the occurrence of the remains of plants and animals imbedded in the solid rocks, that the present was not the original order of things, that there had been upheavals of the sea into dry land and depressions of the land beneath the sea, by the working of forces lodged within the earth, and that the memorials of these changes were preserved for us in the rocks. Seventy years later, another writer of the Italian school, Lazzaro Moro, adopting and extending the conclusions of Steno, pointed to the evidence that the surface of the earth is everywhere worn away, and is repaired by the upheaving power of earthquakes, but for which the mountains and all the dry land would at last be brought beneath the level of the waves.

But none of these desultory researches, interesting and important though they were as landmarks in the progress of science, bore immediate fruit in any broad and philosophic outline of the natural history of the globe. Men were still trammelled by the belief that the date and creation of the world and its inhabitants could not be placed further back than some five or six thousand years, that this limit was fixed for us in Holy Writ, and that every new fact must receive an interpretation in accordance with such limitation. They were thus often driven to distort the facts or to explain them away. If they ventured to pronounce for a natural and obvious interpretation, they laid themselves open to the charge of impiety and atheism, and might bring down the unrelenting vengeance of the Church.

Such was the state of inquiry when the Scottish Geological School came into being. The founder of that school was James Hutton—a man of a singularly original and active mind, who was born at Edinburgh in 1726, and died there in 1797. Educated for the medical profession, but possessed of a small fortune, which gave him leisure for the pursuit of his favourite studies, he eventually devoted himself to the study of Mineralogy. But it was not merely as rare or interesting objects, nor even as parts of a mineralogical system, that he dealt with minerals. They seemed to suggest to him constant questions as to the earlier conditions of our planet, and he was thus gradually led into the wider fields of Geology and Physical Geography. Quietly working in his study here, a favourite member of a brilliant circle of society, which included such men as Black, Cullen, Adam Smith, and Clerk of Eldin, and making frequent excursions to gather fresh data and test the truth of his deductions, he at length matured his immortal "Theory of the Earth," and published it in 1785. Associated with Hutton, rather as a friend and enthusiastic admirer than as an independent observer, was John Playfair, Professor of Natural Philosophy in this University, by whose graceful exposition the doctrines of Hutton were most widely made known to the world. His classic "Illustrations of the Huttonian Theory" is one of the most delightful books of science in our language—clear, elegant, and vivacious—a model of scientific description and argument, which I would most earnestly recommend to your notice. Sir James Hall, another of this little illustrious band, had one of the most inventive minds which have ever taken up the pursuit of science in this country. His merits have never yet been adequately realised by his countrymen, though they are better appreciated in Germany and in France. He was in fact the founder of Experimental Geology, since it was he who first brought geological speculation to the test of actual physical experiment. This he accomplished in a series of ingenious researches, whereby he corroborated some of the disputed parts of the doctrines of his master, Hutton. These were the three chief leaders of the Scottish school; but to their number, as worthy but less celebrated associates, we must not omit to add the names of Mackenzie, Webb Seymour, and Allan.

It would lead me far beyond the allotted hour to attempt any adequate summary of the work achieved by each of these early pioneers of the science. It will be enough for my present purpose if I try to sketch to you what were the leading characteristics of this Scottish School, and what claim it has to be remembered, not by us only, but by all to whom Geology is the subject either of serious study or of pleasant recreation.

Born in a "land of mountain and flood," the geology of the Scottish School naturally dealt in the main with the inorganic part of the science, with the elemental forces which have burst through and cracked and worn down the crust of the earth. It asked the mountains of its birthplace by what chain of events they had been upheaved, how their rocks, so gnarled and broken, had come into being, how valleys and glens had been impressed upon the surface of the land, and how the various strata through which these wind had been step by step built up. It encountered no rocks, like those which had arrested the notice of the early Italian geologists, charged with fossil shells, and corals, and bones of fish, such as still lived in the adjoining seas, and which at once suggested the former presence of the sea over the land. Neither did it meet with deposits showing abundant traces of ancient lakes, and rivers, and land-surfaces, each marked by the presence of animal and plant remains, like those which set Steno and Moro thinking. The rocks of Scotland are as a whole unfossiliferous. It was, therefore, only with the records of physical events, unaided by the testimony of organic remains, that the Scottish geologists had to deal. Their task was to unravel the

* A Lecture delivered at the opening of the clas of Geology and Mineralogy in the University of Edinburgh, by Archibald Geikie, F.R.S., Nov. 6, 1871.

complicated processes by which the rocky crust of the earth has been built up, and by which the present varied contour of the earth's surface has been produced,—to ascertain, in short, from a study of the existing economy of the world, what has been the history of our planet in earlier ages.

Hitherto, while men had been accustomed to believe that the earth was but some 6,000 years old, they sought in the rocks beneath and around them evidence only of the six days' creation or of the flood of Noah. Each new cosmological system was based upon that belief, and tried in various ways to reconcile the Biblical narrative with fanciful interpretations of the facts of Nature. It was reserved for Hutton to declare, for the first time, that the rocks around us can never reveal to us any trace of the beginning of things. He too first clearly and persistently proclaimed the great fundamental truth of Geology, that in seeking to interpret the past history of the earth as chronicled in the rocks, we must use the present economy of nature as our guide. In our investigations, "no powers," he says, "are to be employed that are not natural to the globe, no action to be admitted of except those of which we know the principle. Nor are we to proceed in feigning causes when those appear insufficient which occur in our experience."* This was the guiding principle of the Scottish School, and through their influence it has become the guiding principle of modern Geology.

There were two directions in which Hutton laboured, and in each of which he and his followers constantly travelled by the light of the present order of nature—viz., the investigation of (1) changes which have transpired beneath the surface and within the crust of the earth, and (2) changes which have been effected on the surface itself.

1. That the interior of the earth was hot, and that it was the seat of powerful forces, by which the solid rocks could be rent open and wide regions of land be convulsed, were familiar facts, attested by every volcano and earthquake. These phenomena had been for the most part regarded as abnormal parts of the system of nature; by many writers, indeed, as well as by the general mass of mankind, they were looked upon as Divine judgments, specially sent for the punishment and reformation of the human species. To Hutton, pondering over the great organic system of the world, a deeper meaning was necessary. He felt, as Steno and Moro had done, that the earthquake and volcano were but parts of the general mechanism of our planet. But he saw also that they were not the only exhibitions of the potency of subterranean agencies, that in fact they were only partial and perhaps even secondary manifestations of the influence of the great internal heat of the globe, and that the full import of that influence could not be understood unless careful study was also given to the structure of the rocky crust of the earth. Accordingly, he set himself for years patiently to gather and meditate over data which would throw light upon that structure and its history. The mountains and glens, river-valleys and sea-coasts of his native country, were diligently traversed by him, every journey adding something to his store of materials, and enabling him to arrive continually at wider views of the general economy of nature. At one time we find him in a Highland glen searching for proofs of a hypothesis which he was convinced must be true, and, at their eventual discovery, breaking forth into such gleeful excitement that his attendant gillies concluded he must certainly have hit upon a mine of gold. At another time we read of him boating with his friends Playfair and Hall along the wild cliffs of Berwickshire, again in search of confirmation to his views, and finding, to use the words of Playfair, "palpable evidence of one of the most extraordinary and important facts in the natural history of the earth."

As a result of his wanderings and reflection, he concluded that the great mass of the rocks which form the visible part of the crust of the earth was formed under the sea, as sand, gravel, and mud are laid there now; and that these ancient sediments were consolidated by subterranean heat, and, by paroxysms of the same force, were fractured, contorted, and upheaved into dry land. He found that portions of the rock had even been in a fused state; that granite had erupted through sedimentary rocks; and that the dark trap-rocks or "whinstones" of Scotland were likewise of igneous origin.

When the sedimentary rocks were studied in the broad way which was followed by Hutton and his associates, many proofs appeared of ancient convulsions and re-formations of the earth's surface. It was found that among the hills the strata were often on end, while on the plains they were gently inclined; and the inference was deduced by Hutton that the former series must have been broken up by subterranean commotions before the accumulation of the latter, which was derived from its *débris*. He conjectured that the later rocks would be found actually resting upon the edges of the older. His search for, and discovery of, this relation at the Siccar Point, on the Berwickshire coast, are well described by his biographer Playfair, who accompanied him, and who, dwelling on the impression which the scene had left upon him, adds: "The mind seemed to grow giddy by looking so far into the abyss of time; and while we listened with earnestness and admiration to the philosopher who was now unfolding to us the order and series of these wonderful events, we became sensible how much farther reason may sometimes go than imagination can venture to follow." Sir James Hall afterwards, by a series of characteristically ingenious experiments, showed how the rocks of that coast-line may have been contorted by movements in the crust of the earth under great superincumbent pressure.

Hutton was the first to establish the former molten condition of granite, and of many other crystalline rocks. He maintained that the combined influence of subterranean heat and pressure upon sedimentary rocks could consolidate and mineralise them, and even convert them into crystalline masses. He was thus the founder of the modern doctrines of metamorphism regarding the gradual transformation of marine sediments into the gnarled and rugged gneiss and schist of which mountains are built up. Let me quote the eulogium passed upon this part of his work in an essay by M. Daubrée, which eleven years ago was crowned with a prize by the Academy of Sciences at Paris:—"By an idea entirely new, the illustrious Scottish philosopher showed the successive co-operation of water and the internal heat of the globe in the formation of the same rocks. It is the mark of genius to unite in one common origin phenomena very different in their nature." "Hutton explains the history of the globe with as much simplicity as grandeur. Like most men of genius, indeed, who have opened up new paths, he exaggerated the extent to which his conceptions could be applied. But it is impossible not to view with admiration the profound penetration and the strictness of induction of so clear-sighted a man, at a time when exact observations had been so few, he being the first to recognise the simultaneous effect of water and heat in the formation of rocks, in imagining a system which embraces the whole physical system of the globe. He established principles which, in so far as they are fundamental, are now universally admitted."

(To be continued)

* Hutton's "Theory of the Earth," i. p. 160; ii. p. 549.

SCIENTIFIC SERIALS

Annalen der Chemie und Pharmacie, clix., for July, opens with a concluding communication "On the constitution of the twice substituted benzenes," by E. Ador and V. Meyer. The authors converted sulphanilic acid into bromobenzine-sulphonic acid, and fused the potassium salt of this acid with potassic hydrate. The dihydroxylbenzine produced was found to be resorcin; Meyer and others have proved that resorcin belongs to the 1 : 4 series, and therefore sulphanilic acid must also be regarded as containing the SO_3H and NH_2 in the places 1 and 4 respectively. Sulphanilic acid treated with nitrous acid yields a diazo-derivative $C_6H_4N_2SO_3$, this on boiling with water is converted into phenol-sulphonic acid, which was found to be identical with Kekulé's paraphenolsulphonic acid. At the end of the communication, a valuable table of the twice substituted benzines, showing the place of attachment of the second substituted group is given; it however differs in some respects from the arrangement of other chemists. Ernst and Zwenger have prepared ethyl and amyl gallates by passing hydrochloric acid through a boiling solution of gallic acid in the anhydrous alcohols; at present they have not succeeded in preparing the methyl gallate.—A very exhaustive paper follows "On some substances crystallised from microcosmic salt and from borax," by A. Knop, in which the crystallisation of phosphostannic, phosphozirconic, and phosphoniobic acids from microcosmic salt, and of stannic acid, zirconic acid, noria, and niobic acid from borax are thoroughly discussed.—Lieben and Rossi have prepared "normal valeric acid" by the action of boiling alcoholic potash on butyl cyanide, they find that the valeric acid thus obtained does not agree in properties with either of the acids already known. They have also prepared normal amylic alcohol from the above acid, by heating the calcic valerate with calcic formiate, the valeric aldehyde being converted into amylic alcohol by the action of sodium amalgam. The alcohol obtained boiled at 137°, which is somewhat higher than that of the ordinary alcohol. The normal amylic chloride, bromide, iodide, and acetate have been prepared, all of which possess boiling points higher than those of the compounds obtained from the fermentation alcohol. Normal caproic acid was prepared from amyl cyanide in the same manner as the valeric acid previously described.—A translation of Rossi's paper "On the synthesis of normal propyl alcohol from ethyl alcohol," and also of T. Smith's paper "On the estimation of the alkalies in silicates" follow.—Tollens continues with the seventh contribution on the allyl group, the subject of which is the conversion of allyl alcohol into propyl alcohol; this is accomplished by treating allyl alcohol with solid potash, the temperature being gradually raised to 155°, hydrogen being evolved in the reaction; it was found extremely difficult to purify the propyl alcohol; to obtain conclusive evidence it was converted into propionic acid; some six or eight other bodies are formed in this reaction, such as formic acid, propionic acid, and other higher compounds.—Rinne and Tollens have succeeded in preparing allyl cyanide from the bromide by the repeated action of potassic cyanide, and have converted it into crotonic acid by the action of alcoholic potash; the crotonic acid obtained fused at 72°, and possessed all the properties of crotonic acid as made from allyl cyanide prepared from mustard-oil. By the oxidation of allyl alcohol by chromic acid the authors have obtained formic acid, and small quantities of acrylic acid, no acetic acid being produced.—Fittig contributes a paper "On the alleged dibasic nature of gluconic and lactic acids," being a reply to Hlasiwetz's paper on this subject, Fittig himself considering them monobasic.—The continuation of a paper "On the action of Sulphurous Acid on Platinic Chloride," by K. Birnbaum, follows, several new and complicated salts of this series have been obtained; the reactions seem to proceed in two stages, first a reduction to platinous chloride takes place, and then the substitution of Cl by HSO_3; thus by the action of hydric ammonic sulphite on ammonic chloroplatinate a body of the composition $Pt. \begin{matrix} Cl \\ HSO_3 \end{matrix} \begin{matrix} NH_4 \\ H \end{matrix} So_3 + 4 H_2O$ is obtained.—This number concludes with two short papers by J. Myers. The first is "On the temperature of decomposition of sulphuretted hydrogen," this is placed between 350° and 400°, probably nearer the lower temperature; the second paper is "On sulphuretted hydrogen containing arsenic." Sulphuretted hydrogen, as usually prepared from impure sulphuric acid and ferrous sulphide, contains a gaseous arsenic compound, probably arsenetted hydrogen; the two gases do not react on each other at ordinary temperatures, but when they are heated to the boiling point of mercury, a deposit of arsenious sulphide takes place. The arsenetted hydrogen is probably produced by the action of nascent hydrogen on the arsenic compound existing in the sulphuric acid.

SOCIETIES AND ACADEMIES

London

Royal Microscopical Society, November 1.—W. Kitchen Parker, F.R.S., president, in the chair. Dr. Braithwaite, F.L.S., contributed further remarks on the structure of the Sphagnaceæ or bog-mosses. Confining himself principally to the characters for grouping the numerous species into sub-genera, he advocated the system adopted by Dr. Lindberg of Stockholm, based upon those yielded by the form of the leaves investing certain portions of the stem and divergent branches.—Mr. W. Saville Kent, British Museum, read a paper on Prof. James Clark's Flagellate Infusoria with description of new species. In his communication, Mr. Kent announced the discovery among others of Prof. Clark's minute "collared" types (*Codosiga*, *Bicosœca*, &c.), first made known to the scientific world through the Memoirs of the Boston Society of Natural History for 1866, but not since corroborated by any European naturalist. Of the eleven species noticed by Mr. Kent, five were identified by him with American forms; the remaining six, while referable to corresponding genera, offering well-marked specific distinctions. The whole series are of exceedingly minute size, requiring a magnifying power of 800 diameters and upwards for the recognition of their structural peculiarities, the chief interest attached to them being their striking resemblance to the ultimate cell particles lining the incurrent cavities of sponges, as clearly shown by Prof. Clark in the calcareous, and since demonstrated by Mr. Carter in the siliceous groups. Mr. Kent expressed his dissent from Prof. Clark's views in regard to the nutritive functions of *Monas* and other Flagellata, in the course of his investigations, he having observed the former to engulf food at any portion of its periphery, after the manner of *Amœba*, while in the collar-bearing species, it was intercepted at any portion within the area circumscribed by the base of that organ, there being in no case a distinct mouth as assumed by Prof. Clark. In the discussion that ensued, Mr. Kent assented to the President's suggestion, that the Flagellata, in the possession of one or more lash-like appendages, represented a higher type of organisation than the Foraminifera, and other Rhizopodous Protozoa; and expressed his opinion that the Spongiadæ, as a class, combined the structural characters of the ordinary Rhizopoda and lower Infusoria, having superadded to this a skeletal and aggregated type of organisation essentially their own. Mr. C. Stewart affirming to having observed an appearance of three flagellate appendages to certain cells of *Leucosolenia botryoides* under a magnifying power of about 300 diameters, Mr. Kent accepted his statement as further corroboration of the existence of a membranous collar, which, under an insufficient degree of magnification, presents the aspect attested to by Mr. Stewart. The entire series of Infusorial forms recorded in Mr. Kent's communication were obtained by him from a pond on the estate of Mr. Thos. Randle Bennett, Wentworth House, Stoke Newington.

Entomological Society, November 6.—Prof. J. O. Westwood, F.R.S., vice-president, in the chair. Mr. Davis exhibited a collection of larvæ of Lepidopterous and other insects, beautifully preserved by inflation. Mr. Bond exhibited examples of *Zygæna esculans*, a new British moth, captured by Dr. Buchanan White in Braemar, and *Catocala Fraxini*, recently captured in the Regent's Park; also a singular variety of *Chærocampa elpenor*, in which the central portion of each fore-wing was hyaline.—The Rev. A. Matthews sent for exhibition specimens of *Throscus carinifrons* and *Cryphalus tibiæ*, new, or recently discovered, British Coleoptera.—Mr. M'Lachlan exhibited *Bittacus apterus* from California, recently described by him in the *Entomologists' Monthly Magazine.*—Mr. Howard Vaughan exhibited the dark form of *Triphæna orbona*, from Scotland, known as *T. Curtisii*, and Mr. Lewis made some remarks on the synonymy of this form. Mr. Vaughan also exhibited a nearly black variety of *Arge Galathea*, captured in Kent by Mr. Tarn.—Mr. Miller exhibited an enormous oak-gall from America; also impregnated and unimpregnated eggs of *Libellula flaveola.*—Prof. Westwood exhibited numerous examples of *Formica herculeana*, a gigantic ant not hitherto known as British, found in the proventriculus of an example of *Picus martius*, said to have been shot near Oxford; from the perfect condition of the ants and of the bird which had devoured them, he fully believed in the genuineness of the bird as a British example, an opinion which was not shared by some of the members present. Prof. Westwood also exhibited two male examples of *Papilio Crino* from Ceylon, in one of which some of the veins of the wings were coated with brown hairs, a usual character with the males of some species of *Papilio*, but which had not hitherto been observed in that of *Crino.*—Mr. F. Smith exhibited a *Noctua*, apparently belonging to the genus *Aplecta*, which had been taken alive by Mr. Gwyn Jeffreys at sea, 220 miles from Nova Scotia.—Baron Chandois communicated notes commenting upon Mr. Wollaston's remarks respecting *Eurygnathus parallelus*, a Madeiran beetle described by him, and maintaining its distinctness from *E. Latreillei.*—Mr. Briggs read a paper "On *Zygæna Trifolii* and allied forms," detailing the result of his observations during many years, and arriving at the conclusion that two distinct forms or species had hitherto been confounded in Britain under the name of *Trifolii.*

Linnean Society, November 2.—Mr. G. Bentham, president, in the chair. Sir John Lubbock, Bart., read a paper "On the Origin of Insects," an abstract of which will be found in another column. An interesting discussion followed, in which Mr. George Busk, Mr. A. R. Wallace, Mr. M'Lachlan, Mr. Stainton, and Mr. B. Lowne, took part.—Captain Chimmo, "Notes on the Natural History of the Flying Fish." The author considers that he has established that during flight there is an extra consumption of oxygen by the fish, as shown by an increase of temperature. He finds that life is maintained for a period of from seven to nine minutes out of the water, and states that the fish possesses the power of changing the direction of its course during flight, using its tail as a rudder.

CHESTER

Society of Natural Science, October 25.—President, Rev. Canon Kingsley; treasurer, Mr. Kinsman; hon. secretary, Mr. Manning. The society is divided into three sections: (1) botany, (2) geology, (3) zoology; and numbers nearly 200 members. Mr. Alfred O. Walker read a paper on "Objects and Organisation of Local Natural History Societies."

GLASGOW

Geological Society, October 19.—Mr. Edward A. Wünsch, vice-president, in the chair. The Annual Report and abstract of the accounts for past year showed the society to be in a flourishing condition. — Mr. James Thomson, F.G.S., read a paper "On the Plagiostomous Fishes of the Coal Measures," particularly *Orthacanthus Dechenii* Goldfuss. He observed that Prof. Agassiz, in his "Poissons Fossiles" published in 1837, described the genus *Diplodus* (sp. *gibbosus* and *minutus*) from specimens, chiefly of dissociated teeth, found in the English coal-fields. Subsequently, a well-preserved fish was discovered in Bohemia, and described in 1847 by Goldfuss, who named it *Orthacanthus Dechenii.* In 1848, Prof. Beyrich, of Berlin, described the same fish, and named it *Xenacanthus Dechenii,* founding on the fact that the spine had a greater similarity to *Pleuracanthus* than to *Orthacanthus.* At the meeting of the British Association in Glasgow in 1855, Sir Philip Egerton, from discoveries that had been made in the interval, pointed out that the spines of *Pleuracanthus* and the teeth of *Diplodus* belonged in fact to the same fish. The specimens from which Sir Philip proved this to the Association were obtained from Carluke and Edinburgh. In 1867 Prof. Kner went carefully over the remains of such fishes in the museums of Dresden, Berlin, Breslau, and Vienna. Although none of the specimens found in these museums were complete, yet in some of them he found the teeth of *Diplodus minutus* of Agassiz in position, and from the external aspect of the fossils he accepted Goldfuss's generic name, *Orthacanthus Dechenii.* The specimen which Mr. Thomson now exhibited had been for many years in his collection, and had been provisionally named *Pleuracanthus minutus.* After a careful examination, however, of the microscopic structure both of the teeth and the sh green, he could find no relation between the structure of *Pleuracanthus* and that now exhibited. In the meantime he accepted Prof. Kner's identification, but thought it possible that the discovery of better-preserved specimens would show that the difference of structural character might be due to difference of sex, as he had found to be the case in the recent rays' jaws of *Raia clavata,* both male and female, with the teeth in position, exhibited in support of this view.

PARIS

Academy of Sciences, October 30 — M. P. A. Favre read a continuation of his researches upon the thermal phenomena of electrolysis, containing an account of his investigations upon alkaline bases and sulphates; M. Wurtz presented the continuation of a paper, by M. G. Salet, on the spectra of phosphorus and of the compounds of silicium; and M. Le Verrier communicated a note by M. Diamilla-Muller, on a series of simultaneous magnetic observations which it is proposed to make in various parts of the surface of the globe, on the 15th of October, 1872. This note is accompanied by a table of the absolute magnetic declinations calculated for the above date, at a great number of places in all parts of the eastern hemisphere. —MM. Dumas and Chevreul and General Morin discussed the right of Daguerre to be regarded as the inventor of photography, and asserted the prior claims of Niepce de Saint-Victor.—M. Faye read the conclusion of his memoir on the history and present state of the theory of comets.—M. Delaunay presented a note by M. G. Leveau, giving the elements of the planet Hera (103).—A note was read by M. Barbe, on the uses of dynamite. —M. E. M. Raoult read a note on the transformation of dissolved cane-sugar into glucose, under the influence of light. The exposure lasted from May 12 to October 20.—M. Berthelot communicated the third part of his investigations of the ammoniacal salts, in which he discussed the reciprocal actions of the salts of ammonia and of the other alkalies.—A note was read by MM. A. Scheurer-Kestner and C. Meunier, on the composition and heat of combustion of two Welsh coals (from Bwllf and Powel.)—M. Daubrée communicated a paper on the deposit in which phosphate of lime has lately been discovered in the departments of Tarn-et-Garonne and the Lot.—M. A. Damour presented a note on an idocrase from Arendal, in Norway, containing an analysis of the mineral, and also an analysis of a garnet from Mexico.—M. E. Blanchard communicated a note by M. S. Jourdain, on the reproduction of *Helix aspersa,* in which the author described the arrangement of the reproductive organs and the mode in which their products are brought together.

BOOKS RECEIVED

ENGLISH —The Letters of J. B. Jukes: Edited by his Sister (Chapman and Hall).—A Handbook of the Mineralogy of Cornwall and Devon: J. H. Collins (Longmans).—A Manual of Anthropology, or Science of Man: C. Bray (Longmans).—Note-book of Practical and Solid Geometry: J. H Edgar (Macmillan).—The Admiralty Manual of Scientific Inquiry, 4th edition: Rev. R. Main (J. Murray).—Proceedings of the South Wales Institute of Engineers; Vol. vii, Nos. 2–4.—Insects at Home, being a popular account of British Insects: Rev J. G. Wood (Longmans).

AMERICAN.—Three and Four place Tables of Logarithmic and Trigonometric Functions: J. M. Peirce (Boston, Ginn Brothers).—Seaside Studies in Natural History; Marine Animals of Massachusetts Bay, Radiates: Elizabeth C. Agassiz and Alexander Agassiz (Boston, J. R. Osgood and Co.)

FOREIGN —(Through Williams and Norgate) —Lehrbuch der anorganischen Chemie: Dr. Th. Ph. Büchner; 1te Abtheilung.—Wöhler's Grundriss der organischen Chemie: Dr. R. Fittig; 8te Auflage – Die Zielpunkte der physikalischen Wissenschaft: E. Hagenbach.— Astronomische Tafeln u. Formeln: Dr. C. F. W. Peters.

DIARY

THURSDAY, NOVEMBER 9

LONDON MATHEMATICAL SOCIETY, at 8.—On the Partition of an Even Number into two Primes: J. J. Sylvester, F.R.S.—General Meeting; Election of Council and Officers.

SUNDAY, NOVEMBER 12.

SUNDAY LECTURE SOCIETY, at 4 —Education in India: Jiram Row.

MONDAY, NOVEMBER 13.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.

LONDON INSTITUTION, at 4.—On Elementary Physiology (III.): Prof. Huxley, F.R.S —Nervous Matter; its Structure and Properties: Prof. Huxley, F.R.S.

THURSDAY, NOVEMBER 16.

LONDON INSTITUTION, at 7.30.—The Influence of Geological Phenomena on the Social Life of the People: Harry G. Seeley, F G.S.

ROYAL SOCIETY, at 8.30.

LINNEAN SOCIETY, at 8.—On the Floral Structure of Impatiens fulva, &c.: A. W. Bennett, F L S —Remarks on Dolichos uniflorus: N. A. Dalzell.—Floræ Hongkongensis Supplementum: H. F. Hance, Ph. D.

CONTENTS

NOTICE

We beg leave to state that we decline to return rejected communications, and to this rule we can make no exception. Communications respecting Subscriptions or Advertisements must be addressed to the Publishers, NOT *to the Editor.*

THURSDAY, NOVEMBER 16, 1871

NEW WORKS ON MECHANICS

Lehrbuch der Mechanik in elementarer Darstellung mit Uebungen und Anwendungen auf Maschinen und Bau-Constructionen. Von Ad Wernicke. Vol. I. (Braunschweig, 1871. London: Williams and Norgate.)

Lehrbuch der physikalischen Mechanik. Von Dr. Heinrich Buff. Vol. I. (Braunschweig, 1871. London: Williams and Norgate.)

An Elementary Course of Theoretical and Applied Mechanics. By Richard Wormell. Second Edition. (London, 1871. Groombridge and Sons.)

WERNICKE'S work is intended for pupils in the Prussian industrial schools (*Gewerbeschulen*). The first volume treats of Statics and Dynamics, leaving Hydromechanics for the second. According to the preface, students reading this work should be acquainted with elementary mathematics, including co-ordinate geometry, while a knowledge of the differential calculus is not required. From an English point of view, it is not desirable to draw the line between co-ordinate geometry and the calculus. Even in our universities, not twenty per cent. of the students are acquainted with co-ordinate geometry. It is to be regretted that the proportion is so small; that it is so, is due to the present preposterous system of classical education, that relic of the middle ages which is the bane of our schoolboy days. Almost all English students, however, who learn co-ordinate geometry, generally study both the differential and integral calculus before commencing mechanics. Now intelligent pupils like a text-book of mechanics in which they find scope for exercising all their mathematical knowledge; hence it would appear that for English purposes the line is drawn either too high or too low.

As to the manner in which Wernicke has executed his task, it would be hard to speak too favourably; and notwithstanding the point we have raised, we should hail an English translation as a valuable addition to our standard works on mechanics. One of the best features in the book is that it presents theoretical and practical mechanics not as two distinct subjects, but in that degree of combination which naturally belongs to them.

The first volume of Wernicke's work consists of 500 octavo pages, and is divided into three parts. Part I. discusses the Kinematics of a mathematical point, the inquiry being principally confined to space of two dimensions. The symbol j is here and throughout the work used to denote an acceleration: for example, j_x is the acceleration parallel to the axis of x. This notation (unfamiliar to English readers) has obvious advantages when the more appropriate language of the differential calculus cannot be employed. About fifty examples, many of a practical character, are appended to Part I. Among them is found (Ex. 31) a problem virtually requiring the integration of x^n. The solution given is necessarily roundabout and cumbrous, owing to the restraint which the author has imposed upon his use of mathematics. It may, indeed, be questioned whether a student who is not acquainted with the integral calculus could really profit by a solution which is merely the integral calculus ground down and spoiled.

Part II. is upon the Mechanics of a material particle. We notice here small points in the diagrams which must be useful to the learner. Thus, in a figure where the length of a line is denoted by a symbol, the extremities of a bracket indicate the extremities of the line. Those who use the black-board in teaching will appreciate the advantage of this detail. Take, for example, Fig. 54, which refers to motion in an ellipse about a force in the focus. In this part and the examples appended, the usual proportions relating to the statics and dynamics of forces applied at a single point will be found.

The third part, which treats of the mechanics of a rigid body, occupies four-fifths of the volume. Chap. I. discusses the Composition and Equilibrium of Forces in space; some of the examples require a good deal of honest numerical work, others are well-known questions not involving friction. Chap. II. is on the Centre of Gravity; in this we do not notice much that is unusual, except the excellence of the illustrations. The examples contain problems on the centre of gravity of various useful areas and volumes, the theory of the arch, and many other subjects.

In Chap. III. we have a treatise upon Friction. We miss here an actual description and discussion of a series of experiments from which the laws of friction are established. This omission is to be regretted, because the laws are only approximate, and it is important for the pupil to have materials presented to him from which he can form his own estimate of their correctness. Intelligent pupils would have been pleased to find how true the laws are on the whole, and interested in noting the discrepancies. No good opportunity for introducing and discussing the results of experiments should have been lost in a work of this kind. With this exception, the force of friction has been treated in a manner worthy of its importance; we find its effect upon the various mechanical powers, upon toothed wheels and brakes, and in many other cases, treated in an excellent manner. Chap. IV., on the Motion of a rigid body, very properly commences with the exquisite kinematical theorems of Poinsot. D'Alembert's principle follows, and also a table of moments of inertia, which will be found a useful aid in recollecting these troublesome quantities.

Chap. V., on Elasticity and Rigidity, is certainly the best chapter in the book. Problems connected with the deflection of a beam are among the most interesting questions of mechanics. We have here an exceedingly careful discussion of this subject, not too much encumbered with formulæ. A large number of examples thoroughly worked illustrate this chapter. Every teacher of applied mechanics will find these examples invaluable; they are far better than those on the same subject in any other book with which we are acquainted.

Finally, in estimating the merits of this work, we must recollect that it is a manual for class instruction; it is not, nor does it profess to be, a comprehensive and original treatise, like the great work of Weisbach.

Buff's work, of which the first volume is before us, is of somewhat different character to that of Wernicke. It bears the same marks of painstaking thoroughness which characterise the better class of German works on science.

The illustrations are also unusually good in both books, but while Wernicke's is professedly a mathematical treatise, the work of Buff leans more to the physical aspects of mechanics. There is, however, considerable reference to mathematics in Buff, in fact, he makes free use of the calculus when necessary.

The book consists of thirteen sections :—Section I. is on Rest and Motion ; Section II. on Movement in Space and Time : this contains, in addition to the usual theorems on the motion of a point, a useful article on harmonic motion. Section III. introduces the Composition of Movements ; in this will be found a discussion of experiments upon the trajectory of the bullet from the needle-gun. Section V. commences the subject of Mechanical Work ; we are glad to see in this book the principle of work receives that prominence which it unquestionably deserves. Section VII., on Friction, discusses, among other subjects, Pambour's experiments upon the friction of railway carriages. Section IX., upon the Efficiency of Machines, is admirable, the theory being properly proportioned to the experiments. We find here a full discussion of the subject, without that deluge of formulæ which is so often repulsive to those in search of distinct physical conceptions. Section X. contains what is familiar to us by the term Mechanism ; Section XII. is the most complete account of Centrifugal Force which we have met with in any work ; we have here a physical explanation of the permanent axes, of precession and nutation, of the mode of finding the masses of the heavenly bodies, and of various other matters. Section XIII., upon the Motion of the Pendulum, is a collection of interesting subjects, among them Foucault's pendulum, and a far better account of Cavendish's experiments than is to be found in any English book on mechanics. We are also a little surprised to find the weighing scales treated in this section. The arrangement is novel, and though doubtless much might be said in favour of it, yet we think, on the whole, it is not convenient.

We cordially recommend Buff's treatise to the notice of teachers of natural philosophy.

Mr. Wormell's book, which appears to have been specially intended for the London University examination for B.A. and B.Sc., contains practical and experimental illustrations, in addition to the usual matter. We should gladly welcome a thoroughly good work on the general plan which has been adopted by Mr. Wormell, but the book before us ought to receive careful revision before it is placed in the hands of students. We shall indicate some of the points that we have noticed which require correction. We do so in the belief that a future edition of the work might be made really valuable, and supply a much felt want. Some of the errors are common to this work and other text-books. We can, therefore, only accuse Mr. Wormell of reproducing them, but we cannot allow this excuse on every occasion.

On page 14, we find as follows :—"Any two forces F', F' applied at a point M may be transferred parallel to themselves to any other point M' in the line of direction of the resultant."

This proposition, if true, would assert that the attractions of the earth and sun upon the moon might be transferred to any heavenly body in space which happened to be in the line of direction of the resultant of the forces. The geometrical proof of the composition of parallel forces (p. 33) is meaningless, until the proposition referred to has been properly stated. This blunder is extremely common, it arises from enunciating as a property of forces what is really the definition of a rigid body.

On page 112 we find the following passage :—

"1. When the materials composing the surfaces in contact remain the same, the friction varies as the pressure. Suppose, for example, that a block of wood, having a hole bored in it, rests on a plane inclined at the angle of repose, if lead be poured in the hole, *the screw may be turned so as to incline the plane at a greater angle without causing the body to slide.* By increasing the pressure we increase the friction."

This is very bad ; the statement we have italicised in the second paragraph is entirely erroneous. So serious an error would be quite inexcusable even in one of those for whose use the book has been written.

We should have liked to have seen more experiments upon the mechanical powers cited. A student who reads (p. 94) that in the three sheave pulley-block the power is one-sixth of the load, will naturally be surprised when he finds by trial that the power must be one-fourth of the load ; nor can we find a single word in the book which would enlighten his difficulty. We should also have expected that the author would have replaced the antiquated and useless pulley systems which only exist in manuals, by some compact and useful machines like the differential pulley.

Such are some of the points which we consider to require careful revision before Mr. Wormell's book can be pronounced suitable for the use of students.

OUR BOOK SHELF

Contributions to Botany, Iconographic and Descriptive. By John Miers, F.R.S., F.L.S. Vol. 3, containing a complete Monograph of the Menispermaceæ. Sixty-six litho plates. (London : Williams and Norgate, 1864—1871.)

Mr. Miers's long-promised Monograph of the Menispermaceæ forms the third volume of his valuable "Contributions to Botany." The intimate acquaintance of this veteran botanist with South American plants, and his long study of this particular family, extending over more than twenty years, render his observations peculiarly valuable to all systematic botanists. Although in some important particulars Mr. Miers combats the views of such high authorities as the authors of the "Flora Indica," and those of the "Genera Plantarum," he adduces reasons for his dissent, which will, at least, need careful consideration from all who hereafter write on these plants. Mr. Miers retains, with some modifications, his views of the structure of the different organs in this order published in the Annals of Natural History in 1851, and classifies the genera which constitute it into seven tribes, on characters dependent mainly on the structure of the fruit, and on the position of the cotyledons relatively to the radicle, whether incumbent or accumbent. The establishment of sixty-four distinct genera in the order, instead of the thirty-one admitted by Bentham and Hooker, may be open to criticism, but several of them contain only single species now for the first time described, which appear to be altogether aberrant types of the order. Good plates are always valuable ; and we have here sixty-six, drawn on stone by the author himself, containing careful dissections to illustrate the salient characters of the genera and more important species. This concluding volume of Mr.

Miers's "Contributions to Botany" is no less valuable than any of its predecessors as a record of laborious and conscientious devotion to science. A. W. B.

An Elementary Treatise on Statics. By J. W. Mulcaster F.R.A.S., Military Tutor. (London: Taylor and Francis.),

THIS is a good book without any of that attempt at cramming, too common now in our elementary text-books. It is calculated to give the reader a good grasp of the elements of Statics. It goes over the usual ground, states and proves the principles well and clearly, and contains in each chapter a numerous and excellent series of examples. These examples consist of "graduated and classified groups of problems, each involving distinct statical principles." These, the author says, he finds, and our experience entirely agrees with his, make "an impression on the student's mind otherwise not attainable with problems indiscriminately taken." We gather from the book that it is the production of a good and practical teacher. J. S.

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 Aurora Borealis of Nov. 9 and 10

As the magnificent display of Aurora on the evening of the 10th was witnessed here under very favourable circumstances, and as several of its phases were of unusual occurrence, an abridged account may not be uninteresting.

The Northern Lights were first noticed at about 7.30 G.M.T., the appearance being that of a pale white light, which gradually rose from the N.N.W., until it completely enveloped the Great Bear, but was not sufficiently strong to hide even the faint star near Mizar. Towards 8.40 the auroral mist assumed the more definite form of three broad white bands, stretching across the sky from E. to W., the uppermost band lying just below Vega and Pollux.

At the same time a bank of dense black cloud rose from the N. horizon to the height of η Ursæ, and shot forth dark streamers as far as the upper arch of light, The streamers E. and W. were brighter than the central part, and waves of light moved slowly and at regular intervals from these brighter parts of the horizon, mingling together at the centre of the arch.

At 9.10 a very bright streamer made its appearance.

Up to this time the display had been colourless, but at 9.20 it assumed a greyish tinge, and had extended by 9.25 as far as β Cassiopeiæ.

At 9.30 the western extremity of the arch was of a bright red colour, whilst only a slight appearance of redness was visible in the E.

The aurora then became wonderfully brilliant, and the rapidity of the changes surpassed anything that had been seen here for years. Flashes of light were succeeded by waves, and these in their turn by small detached clouds, which travelled rapidly across the sky. At 9.45 the waves and streamers seemed to converge to a point slightly S.E. of β Andromedæ.

In the square of Pegasus a curiously-formed cloud, in the shape of an enormous bird, suddenly appeared and disappeared several times, sending forth each time streams of light E. and W., as if from its outstretched wings.

At 10 the auroral light was strongest, and then the waves, moving rapidly from the N., appeared to return for a short distance on their path when they had passed a few degrees S. of the zenith, like waves breaking on the sea shore.

At 10.30 two distinct arches of light, the upper one passing through β Andromedæ, the lower one near Polaris, intersected each other E. and W. at an altitude of about 20°.

At 10.40 all colour had disappeared in the west, but a very brilliant red streamer stretched from the E. nearly to the Twins. About this time a thick cloud of elliptic shape was formed between the points N.W. by N. and W. Beneath this cloud was a pale auroral glare, and from its upper side a mass of broad dark streamers rose towards Polaris. At the E. end of the cloud a very broad streamer moved gradually westward, and shortly afterwards a similar streamer formed near the W. and moved in the same direction.

At 10.45 α Arietis was the centre, towards which the new violet-coloured streamers and the waves and flashes tended.

The last-mentioned cloud was then replaced by another similar in form, but situated farther from the E., its outer streamers of a yellowish green colour meeting in Cassiopeia.

At 11 the only colour visible was the violet in the W.

At 11.5 a point S. of γ Pegasi was the centre of motion.

At 11.15 the dark streamers were sharply defined, but extended only a few degrees above the cloud. Ten minutes later the stars below Vega and Ursa minor were completely hidden, and then from 11.25 to 12.15 the aurora gradually died away, leaving only a faint white glare on the N.W. horizon. S. J. PERRY

Stonyhurst College Observatory

ON Friday evening, Nov. 10, I was fortunate enough to witness a brilliant display of the Aurora Borealis, which. if it did not surpass, certainly rivalled, that of Oct. 24, 1870.

At 9h. 20m. G.M.T. the whole sky was literally covered with auroral streaks to within 30° of the southern horizon, all apparently converging to a point near α Andromedæ. The streaks were of a white colour, having a slightly blueish tint (probably caused by the mass of intervening air), and their form, to within 15° of the point of convergence, was perfectly straight. The radial point was shown by an irregular mass of auroral light, from which bright streaks were spread out in every direction, those to the south being much shorter than the streaks to the north or west. The appearance of the sky at the time was that of the outstretched wing of an enormous bird. At 9h. 22m. a rich crimson glare was visible in the S.W., dividing the constellations Pegasus and Cygnus, and at 9h. 25m. a resplendent beam of white light 2° in width was conspicuous in the N.E.; its length was about 50°, and it was nearly parallel in direction with a line joining the stars α Capella and β Aurigæ, but 3° to the left of them. It remained visible for 5m.

At 9h. 25m. 30s. a white luminous meteor (apparently one of the "Leonides") shot swiftly across the constellation Pisces, having a brightness = Sirius, duration 0·5sec., and length of path 10°, left no train or sparks.

At 9h. 32m. the constellation Perseus was overspread by a luminous glare of a reddish colour (known to dyers by the appellation of "ruddy brown,") and which did not disappear for about 10m. At 9h. 34m. the crimson glow reappeared in the S.W. between Cygnus and Pegasus, thereby completing a gorgeous arch about 15° in width, extending from the S.W. to the N.E. horizon, passing over the constellations Cygnus, Lacerta, Perseus, Auriga, and Orion. This crimson belt divided the sky into two halves, that on the north being full of auroral streaks, two columns of which were very conspicuous in the north, passing over Ursa Major and extending nearly to the zenith. A small dark cloud lying horizontally across them divided them into two parts, each of which was distinctly visible.

At 9h. 40m. the streaks had entirely disappeared, being replaced by a diffused auroral glare, similar in appearance to the sky before dawn; but at 10h. the streamers reappeared with equal brilliancy. The radial point had now moved to 2° below β Andromedæ, and was now clearly pointed out by an irregular curve, or hook, about 4° or 5° in diameter, which, although observed at different times during the evening, was never completely formed, as 90° or 120° were always wanting to form a complete circle.

At 10h. 23m. a curious phenomenon presented itself. A small irregular patch of crimson light, about twice the diameter of the moon, appeared over β Trianguli, which slowly and gradually expanded, but after a lapse of about 30s. (when about 15° in diameter), its colour changed to the ordinary bluish white of the aurora, the phenomenon lasting altogether about 2m. At 10h. 25m. a broad *greenish* white band appeared in the N.E.

By this time the centre of convergence had reached β Trianguli, thus showing apparent progressive motion towards the *east* at the rate of about 15° per hour (which is the rate of the rotation of the earth upon its axis). It is worthy of notice that in the auroral displays of October 1870 the same stars formed the radiant, and its motion was in the same direction.

At 10h. 37m. a beautiful crimson beam appeared in Auriga (in the same position previously occupied by the white streak at 9h 35m.) Its length was about 40°, and at 10h. 50m. a gorgeous *triple* streak was visible in the same position, which presented the appearance of a broad crimson ribbon, with a border of *white* on each side. In about five minutes it faded out of sight.

At 11 o'clock the auroral light was again diffused over the whole northern sky, bounded on the south by a bright milky arch extending from the E. to the S.W. by S. horizon, visible for ten minutes.

At 11h. 30m. only a few faint streamers, and at 12 o'clock the arch was again visible to the S.E., but aurora very faint.

During the progress of the display the peculiar undulatory phases noticed last year were particularly observed. The waves of light seemed to chase each other in rapid succession along the radiating streaks, coming into collision at the point of convergence. The semicircular masses surrounding this point appearing as if they occupied a *fixed* position in the sky, and becoming visible to the eye only as the intermittent waves reached them, somewhat analogous to the waves of the ocean dashing against a rock and breaking over it in a mass of white foam.

In conclusion may I venture to suggest the application of photography to auroral phenomena; and perhaps some of your readers might *practically* answer the query, "Can a photograph be taken of an auroral display?"

ROBERT MCCLURE

342, Argyle Street, Glasgow, Nov. 11

THERE was a brilliant display of Aurora Borealis here on the evenings of Thursday and Friday, November 9 and 10—especially the latter night. Towards 7 o'clock a hazy light began to spread itself over the northern sky, near the horizon, not unlike a brilliant twilight. At 8 P.M. two arches were quite distinct, the upper one being well defined, with its apex passing through the head of Ursa Major. Gradually streamers began to pass from this, and by 9h. 15m. the scene was simply gorgeous. I do not remember ever seeing the streamers so expanded—more like flames, nor possessing such intense whiteness, so much so, that the evening was almost as light as if the moon had been shining. After proceeding from the upper arch, their course was most rapid to the zenith—apparently passing at times behind clouds, then suddenly emerging—where a magnificent whirling motion was formed, which kept changing in true Protean fashion. A grand, though somewhat dingy, red haze next appeared in the west, which gradually ascended towards the zenith, when it disappeared. Meanwhile flashes of light, resembling summer lightning, darted upwards from about 45° from all directions, and not least from the south—the N.W. heavens assuming a muddy green colour. About a quarter-past ten P.M. the aurora gradually diminished, especially the upper arch, and streamers from it. Then the lower arch began to give off streamers, but these were short and of short duration, though of considerable brightness. The display of Friday, if it fell short of those of October 23 and 24, 1870, in point of brilliant colours, surpassed them in some respects—*e.g.* extent of streamers, and brilliancy of light. Barometer corrected and reduced 29·472: Temperature 32.°

THOMAS FAWCETT

Blencowe School, Cumberland

THERE was a very bright Aurora here last night: the streamers were white, with a red glow in some places. At about ten there was that beautiful and rare phenomenon—a "corona" of streamers converging at the zenith. The barometer was about 29·6. This morning is fine, with the barometer rising.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, Nov. 11

THERE have been two magnificent auroral displays on the nights of the 9th and 10th inst. That on the 9th commenced at 10 o'clock, and continued with little interruption until 12.45; and last night from 9.40 until 12 o'clock. Both displays were in the north and north-west, and at times the streamers reached the zenith, but I did not observe them to pass beyond that point. The colours were varied; at one time of a beautiful crimson, at another a greenish white. Last night's display was the most interesting, but not so brilliant as that of the previous night. The aurora made its first appearance by an undefined redness in the north; it then gradually developed into a crimson, and assumed the shape of a vertical pillar, the upper part tapering to a clearly defined point, within a few degrees of the zenith. It remained in this shape and position for two minutes, and then faded away. At 10.15 there appeared, at about 10 degrees above the horizon, a peculiar lightness, like the edge of a dark horizontal cloud illuminated by the hidden moon, but I could distinctly discern some stars below the illuminated *stratum*, which proves that the *cloud* was transparent; the stars could not, however, be seen through the lightness. At 10.40 there were three distinct streamers shooting up from this light, emanating from separate parts, but all in the north and north-west. They then assumed an easterly movement, the right hand streamer before disappearing being in the north-east. The centre one of these was of a very light colour, approaching a faint or whitish green; the others were crimson. At 11 o'clock I saw an exceedingly brilliant *patch* undefined in the north-east; by this time some clouds, stratified horizontally, rose from the northern horizon and passed into the light part of the heavens, which seemed to influence the display by intensifying the streamers, which were shooting up, at this time, to the zenith. At 11.30 I saw six beams start across east and west, of a whitish colour with dark spaces between, and the southern one in the zenith. The northern streamer now disappeared, but the *auroral twilight* was still visible, although gradually fading, and by 12 o'clock all was darkness. I did not continue my observation beyond this hour, the temperature not being conducive to personal comfort.

I may remark that with the exception of the few clouds which rose last night, both nights were perfectly cloudless, and the milky way shone with uncommon splendour. A portion of this band of stars at one time looked grand, as one mighty streamer ran along its course, some of the largest stars being visible through the intercepting redness.

I hope that some of your correspondents will give particulars of any magnetic disturbances which may have occurred on the nights of the above displays.

JOHN JEREMIAH

43, Red Lion Street, Nov. 11

P.S.—I have been informed that the white horizontal light mentioned in my communication of the 11th inst. was visible at 7.30 on the night of the 9th, but no streamers were seen until the time stated by me.

On Saturday night, at 7.45, I saw in the north-western sky a slight auroral redness, but it did not last more than two minutes.

Nov. 13 J. J.

NOV. 10th, 11 P.M.—I have just witnessed a most magnificent display of Aurora. I first saw it at 9.30. Here is an account of it. The bearings given are magnetic.

9.30 P.M.—On the W. was a deep crimson glow of the richest possible colour, about 50° broad and 60° high. From W.N.W. to N. the sky was filled by a mass of white light, pulsating in long horizontal masses moving upwards. At 9.36 they were moving, not very uniformly, at the rate of 33 waves per minute. From the N. to the E. extended a bright horizontal bend of steady white light, marked with vertical lines and having jagged edges. Suddenly from the centre of it shot up a vertical white streamer 3° or 4° wide; this remained stationary for a few minutes and then gradually faded away.

At 9.38 a fan-shaped mass of white light appeared at N.N.E. At 9.45 a band of white light extending from the horizon to a height of about 20°. From the centre of this streamed upwards a kind of waving flag of intense red light, about 20° broad and reaching to the zenith. At N.N.E. the fan was gone and a bright horizontal band of white light marked with vertical lines had taken its place. It was almost 40° long and 30° high. At 9.50 there appeared an arch of white light about 10° thick. The centre was about 60° high, white, the ends were on the horizon at E.N.E. and N.W. This vanished and was replaced by a horizontal white band, about 60° long and 10° high, the lower edge being about 20° above the horizon. Out of this presently rose four beautiful white streamers. At 9.52 an intensely bright red light was observed at W. At the N.E. were a few patches of white light. At the N.N.E. appeared about ten vertical white streaks for a minute or so. They were 15° high and filled a horizontal space of about 20°. At 9.53 a rather fine meteorite fell. At the N.W. was a red stream about 30° broad and 80° high, while at W.S.W. was a mass of red light. At 9.55 the mass of white light at E.N.E. threw out a number of jets of light in shape like the streams of water from the rose of a watering can. At 10 P.M. the arch which had vanished reappeared, reaching from W.N.W. to E.N.E. It glowed with a deep white light, which was motionless, except that at 10.2 I observed two downward waves. At 10.3 a long streamer grew out of it. At 10.5 the right-hand end was tossed up into the form of a haycock. At 10.8 a glow spread upwards from the centre of the arch, and filled the upper part of the sky. At the same time a slight patch of red light reappeared in the W. The sky to the S. was lighted up with the reflection of the white light in the N. The reflected light seemed to have a faint reddish tinge.

By 10.10 nothing was left except the arch, and between 10.10 and 11 that also vanished.

The stars could be seen distinctly through the aurora. When the light was at the brightest I could see the figures and hands of a large watch, but could not distinguish the figures one from another. Thermometer 30·5 F.; Barometer 29·69 inches.

Pixholme, Dorking, Surrey J. E. H. GORDON

Structure of Lepidodendron

PROFESSOR DYER has already discovered one of the many new facts with which he has yet to become familiar, and hastens, in a straightforward manner, to acknowledge the circumstance; but I must again remind him that this, along with many other facts, was described in No. 129 of the Proceedings of the Royal Society. Professor Dyer further says: "Suppose the transverse septa separating these cells absorbed, *as probably eventually they would have been*, and the rows of cells become scalariform vessels." But I can assure him, as a question of fact, that these cells do *not* become so changed; consequently his conclusion that the central cells and the investing vessels are but parts of "one central structure" becomes negatived. The separation of these two structures *increases* with age instead of *diminishing*.

W. C. WILLIAMSON

Encke's Comet

IT may interest those who possess small telescopes to know that this comet is now within the range of instruments of moderate apertures. On November 10 I had a very satisfactory view of it, with a 4" equatorial by Cooke; no signs of a nucleus were observed, but there appeared to be a slight condensation of light on the *following* side of the comet. THOS. G. E. ELGER

Bedford, Nov. 11

The Science and Art Department

IN your last number there appears a letter signed "Henry Uhlgren," which, among other interesting statements, contains the following: Referring to Mr. Forster's statement in the House of Commons that there was no foundation for the report that "the Examiners after having made their reports had the papers returned to them, with an instruction to reduce the number of successful candidates, as an intimation had been given by a right hon. gentleman that the amount of the Grant due upon those papers must be reduced 20,000*l.*," Mr. Uhlgren states: "But previous to that a provincial local secretary, hearing the rumour, wrote to ask the Department if it were true, and received a reply saying it was true, and that instead of the amount being 20,000*l.* it was 40,000*l.* (the Department's letter can be produced.)" Premising that the amount of the whole vote for payments to teachers on results in science (which was to be reduced by 40,000*l.*) was 26,000*l.*, may I ask for the production or publication of this extraordinary official letter? X

ECONOMICAL ALIMENTATION

IN glancing over the recent issues of the *Comptes Rendus*, one cannot but fail to be struck with the practical importance of many of the communications contained therein, a large proportion of which bear special reference to the Siege of Paris. In nearly every branch of science there is some endeavour made to supplement and improve our knowledge in matters such as were then of the greatest importance, and the members of the *Académie* have come forward eagerly to aid, by advice and precept, in overcoming the misery of a prolonged siege. Unfortunately, but little could be done, even by such men as Fremy, Dumas, Chevreul, and others, against the insuperable difficulties which presented themselves; but nevertheless Paris owes much to her men of science who contributed many services of value, at a time when these were most needed. The manufacture and employment of nitro-glycerine in mines and shells, were successfully accomplished at a crisis when the stock of gunpowder was running terribly short, and the supply of some other reliable explosive was rendered imperative. Hitherto nitro-glycerine had been regarded as a most dangerous combustible, liable to explode at the slightest concussion, and yet we hear of its employment in shells against the Prussians, thundered forth from guns of the heaviest calibre, without one single instance of its premature explosion being recorded. Again the question of ballooning, although not perhaps very far advanced by the deliberations of the *Académie*, has, at any rate, been more satisfactorily solved than at any previous period, and Paris has been certainly the first to employ these frail and romantic contrivances in a practical every-day manner, and thus to render the words, "*par ballon monté*" familiar to the ear as a household phrase. In matters of surgery, as in those of a sanitary nature, sound advice was not wanting, and even the abstract calling of the soldier,—the philosophy of his manner of fighting—formed the theme of much scientific discussion.

But the most interesting, perhaps, of all the subjects with which the *Académie des Sciences* busied itself, was that of seeking an economical means of alimeniation for the inhabitants of Paris during the siege. Given certain limited sources of supply, a fixed amount of suitable organic matter, and the problem was how to use the same to the fullest and most profitable degree. Of sheep and oxen there was but an exceedingly limited provision in proportion to the very populous state of the city, and although corn and wine were said to be in abundance, there is no doubt the authorities were from the first sorely troubled by the vague estimates that were published of these comestibles.

As a suitable manner of economising corn, M. Gauldrée called attention to the method in vogue among the Romans of parching and bruising the grains, which in this state may be made to yield an excellent and highly nutritious soup or porridge. The corn is carefully sifted by hand, browned without charring, until it breaks when taken between the teeth, and then ground in any available mill; it is mixed with cold water, boiled for thirty minutes, and seasoned as desired. So economical was this preparation, that at the public kitchens, established in certain quarters of Paris, it was possible to dispense one portion of *bouillie romaine* together with a small modicum of wine for the amount of five centimes.

A proposition to manufacture artificial milk, brought forward by M. Gaudin, seems worthy of some notice. That gentleman estimated that 500,000 litres per day of milk could be prepared in Paris at an exceedingly trifling cost, which should have all the nutritious qualities of good milk, and which should, besides, be neither unpleasant of taste or smell. An emulsion at a very high temperature is made of *bouillon de viande* prepared from bones, fat, and gelatine, and when cold, a product is obtained resembling in taste stale milk of a cheesy flavour; the components of ordinary milk are all present, the gelatine representing the casein; fat, the butter; and sugar, the sugar of milk. For admixture with coffee, chocolate, soup, &c., the milk is said to be by no means disagreeable.

Many propositions were brought forward to economise the blood from the abattoir, the plan suggested by M. Gaultier of mixing it with flour in the manufacture of bread being perhaps the best and simplest, as the fibrine and albumen, so rich in nitrogen—of which the alimentary properties are well known—are in this way utilised to the highest degree. Less inviting is the proposal of M. Fud to consume he carcases of animals that died of typhus, rhinderpest, and other diseases, the flesh in these instances being, so asserts M. Fud, capable of use as food, if only cooked in a suitable manner.

More important, however, than all, is M. Fremy's attempt to bring forward osseine as an article of food. Osseine is essentially different from gelatine, which has recently been asserted by chemists—erroneously, so M. Fremy thinks—to be not only unnutritious, but positively injurious to the human system. Leaving, however, the question of gelatine on one side, M. Fremy proceeds to advance the qualifications of osseine as an alimentary substance. Although gelatine and osseine are isomeric, in the same way as starch and dextrine are isomeric, they have not the same properties. Gelatine, unlike osseine, does not exist in organism, but is produced by chemical transformation resulting from the action of water and heat upon the bony tissue; gelatine, moreover, is completely soluble in water, while osseine is not so. For these reasons the two substances would doubtless be different in their alimentary capacities, and deductions drawn from the influence of one upon the human system ought not in any way to prejudice the other. Of course, says M. Fremy, osseine cannot be expected to fulfil the same duty as a complete aliment; such, for instance, as bread, or meat, but must be employed in conjunction with some other suitable material. In the same way gluten, which is simply flour freed from starch, oil, and soluble substances, would alone be powerless to support life and health. If regarded in the same light as fibrine, casein, and albumen, and associated with other bodies, osseine would be found a valuable aliment. White meat, calf's head, neatsfoot, &c., contain much bony tissue, and their nutritious qualities are incontestable.

Of this osseine, then, bones are said to contain 35 per cent., the mode of separation being simply to slice the bone very thinly, and to treat the same with dilute hydrochloric acid; hard white bones, free from fat, are most suitable, and some care and attention in manipulation is of course necessary, so that the product may be perfectly sweet and free from any taint or unpleasant odour. For if disgust is once aroused against this kind of food, as indeed against any other for that matter, no amount of pushing or puffing can force it into the public market. Should, therefore, any trace of acid be perceptible after preparation of the osseine, it is recommended that the product be treated with an alkali of some kind, for example, lime or carbonate of soda, but this must obviously be done with due care and discretion. The cost of this aliment is about one franc per pound, whereas gelatine of good quality costs from four to five francs.

As regards the best method of cooking or curing, M. Fremy recommends the swelling of the mass with hot water, and then boiling for about an hour, when the tissue becomes soft and pliable; it may be seasoned in the cooking, or may be allowed to cool and then kept for thirty-six hours in brine. If eaten warm with admixture of some fat and vegetables the osseine is decidedly palatable. Owing to its large constituent of nitrogen it is extremely nutritious, and, furthermore, forms a comestible not liable to become putrid.

It is right to mention that on some of the points enumerated by M. Fremy, exception is taken by M. Dumas and others, who are not so confident of the real value of osseine as an alimentary substance, those gentlemen maintaining the injurious nature of gelatine; M. Chevreul, however, confirms to some extent M. Fremy, and states that osseine is decidedly more nutritious than gelatine.

Other measures for improving the alimentation of Paris were taken during the siege, but these for the most part present little novelty. Mr. Wilson's plan for salting the carcases intact, and thus preserving the meat in an almost fresh condition, was resorted to, that gentleman bringing his personal staff from Ireland to afford assistance just at the instant of closing the gates of the metropolis. The assistance of M. Georges, whose plan of preserving meat is both original and peculiar, was likewise obtained; this invention, which has been practised it is said with much success in America, is adapted more particularly for the curing of mutton rather than beef, and consists in treating the meat in a bath acidified with hydrochloric acid, and afterwards in a solution of sulphite of soda. In this condition, after further sprinkling with sulphite of soda, the flesh is packed in tins and soldered down: the sulphite of soda acting upon the hydrochloric acid gives rise to sea salt and sulphurous acid, thus ensuring the perfect preservation of the meat.

H. B. P.

THE TEMPERATURE PRODUCED BY SOLAR RADIATION

SIR ISAAC NEWTON determined the intensity of solar radiation by observing the increment of temperature of dry earth on being exposed to the sun. In the latitude of London at midsummer, dry earth acquires a temperature of 150° in the sun at noon and 85° in the shade, difference about 65° Fah. This difference Sir Isaac Newton regarded as a true index of the intensity of solar radiation; hence his celebrated demonstration proving that the comet of 1680 was subjected to a temperature 7,000 times higher than that of boiling water ($212° \times 7,000 = 1,484,000°$ Fah.).* The comet when in its perihelion being within one-third part of the radius of the sun from his surface, we have to add the diminution of temperature, 0·44, attending the dispersion of the rays in passing through the solar atmosphere and the remainder of the stated distance from the sun. Accordingly, the demonstration showing that the comet of 1680 was subjected to a temperature 7,000 times higher than that of boiling water, establishes a solar temperature exceeding 2,640,000°; and if we add 0·21 for the retardation of the rays in traversing the terrestrial atmosphere, it will be found that the temperature deduced from the experiments with incandescent radiators, and our actinometer observations, differs scarcely $\frac{1}{5}$ from that roughly estimated by the author of the "Principia." In order to comprehend fully the merits of the method of determining solar intensity conceived by his master mind, let us imagine an extended surface of dry earth, one half of which is shaded, the other half being exposed to the sun. Dry earth being a powerful absorbent and radiator, and at the same time a bad conductor, the central portion of the supposed surface evidently cannot suffer any loss of heat by lateral radiation; while the non-conducting property of the material prevents loss by conduction laterally or downwards. Consequently, no reduction of temperature can take place excepting by radiation in the direction of the source of the heat. Removing the shade, during an investigation, it will be found that, notwithstanding the uninterrupted radiation of the exposed substance upwards, the intensity will gradually increase until an additional temperature of about 65° Fah. has been acquired. Indisputably, this increase of temperature is due to unaided solar radiation. Evidently the accidental interference of currents of air need not be considered. Besides, if the dry earth is confined within a vacuum, such interference may be entirely obviated. It is scarcely necessary to point out that the generally-adopted mode of measuring the sun's radiant heat by thermometers, is in direct opposition to the principle involved in the method under consideration. The meteorologist, in place of preventing the bulb from radiating in all directions and guarding against loss of heat by convection, puts his thermometer on the grass, or suspends it on a post, one half of the convex area of the bulb receiving the sun's radiant heat, while the other half is permitted to radiate freely, the whole being exposed to the radiation from surrounding objects and to the refrigerating influence of accidental currents of air, in addition to the permanent current produced by the ascending heated column above the bulb. This explains the cause of the perplexing discrepancies in meteorological records. The extent of the diminution of intensity of solar radiation occasioned by cold air acting on the bulb, and by the latter radiating freely in all directions, is demonstrated in the most conclusive manner by the result of observations made with the instrument described by Père Secchi in

* Sir Isaac Newton has been criticised for comparing the temperature to that of red-hot iron, "a term of comparison indeed of a very vague description," it is said in "Outlines of Astronomy." This criticism is far from being correct, since the demonstration clearly shows what is meant by the term red-hot, viz. a temperature 3·5 times that of boiling water. The reference to red-heat, exceeded "two thousand times," was evidently intended to furnish some adequate notion of the inconceivably high degree of temperature involved in the computation.

Table A.—*Showing the Temperature produced by Solar Radiation at Noon, for each degree of Latitude, when the Earth is in Aphelion. Northern Hemisphere :—*

	Latitude.	Solar intensity at Noon.	Latitude.	Solar intensity at Noon.	Latitude.	Solar intensity at Noon.	Latitude.	Solar intensity at Noon.	
	Deg.	Fah.	Deg.	Fah.	Deg.	Fah.	Deg.	Fah.	
Equator	0	65·30	24	67·20	49	64·95			
	1	65·45	25	67·19	50	64·77			
	2	65·60	26	67·18	51	64·58			
	3	65·75	27	67·17	51·28	64·48	... ...	... ...	Greenwich.
	4	65·89	28	67·14	52	64·38	72	58·69	
	5	66·02	29	67·10	53	64·17	73	58·31	
	6	66·15	30	67·05	54	63·96	74	57·92	
	7	66·27	31	66·99	55	63·74	75	57·52	
	8	66·39	32	66·93	56	63·51	76	57·10	
	9	66·49	33	66·87	57	63·28	77	56·67	
	10	66·58	34	66·80	58	63·04	78	56·24	
	11	66·66	35	66·73	59	62·79	79	55·79	
	12	66·73	36	66·66	60	62·53	80	55·32	
	13	66·80	37	66·58	61	62·25	81	54·84	
	14	66·87	38	66·49	62	61·96	82	54·35	
	15	·66·93	39	66·39	63	61·65	83	53·84	
	16	66·99	40	66·27	64	61·34	84	53·32	
	17	67·05	41	66·15	65	61·03	85	52·78	
	18	67·10	42	66·02	66	60·72	86	52·23	
	19	67·14	43	65·89	66·30	60·57	... ...	... ...	Arctic Circle.
	20	67·17	44	65·75	67	60·41	87	51·68	
	21	67·18	45	65·60	68	60·09	88	51·11	
	22	67·19	46	65·45	69	59·76	89	50·52	
	23	67·20	47	65·30	70	59·42	90	49·91	North Pole.
Tropic of Cancer	23·30	67·20	48	65·13	71	59·06			

Table B.—*Showing the Temperature produced by Solar Radiation of the Earth's orbit; also the gradual* diminution *of Temperature during the first half, and the gradual* increment *of Temperature during the second half-year :—*

Dates.	1st.		5th.		10th.		15th.		20th.		25th.	
Month.	Max.	Diff.	Max.	Diff.	Max.	Diff.	Max.	Diff.	Max.	Diff.	Max.	Diff.
	Fah.	Fah.	Fah.	Fah.	Fah.	Fah.	Fah.	.	Fah.	Fah.	Fah.	Fah.
January	90·72	5·88	90·70	5·86	90·67	5·83	90·62	5·78	90·54	5·70	90·44	5·60
February...	90·28	5·44	90·16	5·32	90·01	5·17	89·83	4·99	89·64	4·80	89·43	4·59
March	89·27	4·43	89·09	4·25	88·86	4·02	88·62	3·78	88·37	3·53	88·12	3·28
April	87·77	2·93	87·57	2·73	87·32	2·48	87·07	2·23	86·83	1·99	86·59	1·75
May	86·32	1·48	86·15	1·31	85·95	1·11	85·76	0·92	85·58	0·74	85·43	0·59
June	85·22	0·38	85·13	0·29	85·03	0·19	84·96	0·12	84·90	0·06	84·86	0·02
July	84·84	0·00	84·85	0·01	84·87	0·03	84·92	0·08	84·99	0·15	85·07	0·23
August	85·22	0·38	85·34	0·50	85·49	0·65	85·65	0·81	85·83	0·99	86·03	1·19
September	86·32	1·48	86·50	1·66	86·73	1·89	86·97	2·13	87·22	2·38	87·47	2·63
October	87·77	2·93	87·97	3·13	88·22	3·38	88·47	3·63	88·71	3·87	88·95	4·11
November	89·27	4·43	89·43	4·59	89·64	4·80	89·83	4·99	90·01	5·17	90·16	5·32
December	90·33	5·49	90·42	5·58	90·52	5·68	90·61	5·77	90·66	5·82	90·70	5·86

Table C.—*Temperatures produced by Solar Radiation, June* 26, 1871, *compared with the Temperatures entered in the Table constructed* 1870, *for corresponding Zenith distances. Mean discrepancy* = 0·26° *Fah. :—*

	Zenith Distances—Degrees.								
	57	58	59	60	61	62	63	64	65
	Fah.	Fah.	Fah.	Fah.	Fah.	Fah.	Fah.	Fah.	Fah.
Observations June 26, 1871 ...	55·64	55·00	53·83	53·51	53·41	52·76	52·23	51·70	51·27
Table of 1870	55·09	54·60	54·10	53·58	53·05	52·50	51·90	51·40	50·81

his recent work "Le Soleil" (p. 267). "During a great number of observations made at Rome," says the author, "the difference between the two temperatures (that indicated by the thermometer exposed to the sun and that of the surrounding casing), was 12·06° (21·70° Fah.); during days when the sky was clearer, it rose to 14°." Consequently, the highest temperature indicated by the instrument referred to, was 25·2° Fah., against 66·04°, which is the true maximum solar intensity in the latitude of Rome. It will be seen then, that, by exposing the bulb of the thermometer in the manner pointed out, it is possible to reduce the temperature produced by solar radiation to 0·38 of the actual temperature.

It will be proper to observe with reference to the accompanying tables—constructed in accordance with the result of investigations continued winter and summer during three years—that the opinion expressed by the Director of the Roman Observatory, respecting solar intensity at different seasons, is wholly at variance with the facts established by my numerous observations. The question was raised last summer whether the high temperature during the "heated term" would not charge the atmosphere with an additional amount of vapour capable of retarding the passage of the heat rays, thus rendering the figures entered in my tables to some extent unreliable. Accordingly, during the solstice June 26, 1871, the sky being very clear, the actinometer was put in operation for the purpose of ascertaining with critical nicety whether the atmosphere which had been loaded with vapour for several weeks previously possessed any unusual property tending to check the heating power of the sun's rays. The observations were made late in the afternoon under great zenith distance and increased atmospheric depth, in order to subject the heat rays to an additional retardation from the supposed vapours. The result is recorded in Table C, by which it will be seen that the reduction of temperature was only 0·26° Fah., a difference too small to call for any explanation. The result of the observations made during midwinter are equally conclusive with reference to the permanency of solar energy at all seasons. Among others may be mentioned that of January 17, 1871, the zenith distance being 61° 30′, the actinometer remained perfectly stationary at 58·73° Fah., from 12h. 10m. to 12h. 20m. P.M. The table just referred to shows that on June 26, 1871, the actinometer indicated 53·08° when the sun's zenith distance was 61° 30′. Hence during midwinter the temperature proved to be 58·73° – 53·08° = 5·65° higher, for corresponding zenith distance, than during the summer solstice. By reference to Table B it will be seen that owing to the diminished distance between the sun and the earth, the increment of temperature on January 17, ought to have been 5·75°, discrepancy = 0·1° Fah. In the face of such facts it is idle to contend that the temperature produced by solar radiation under corresponding zenith distance and a *clear sky*, varies from any other cause than the varying distance between the sun and the earth. Of course there are many regions in which the sun, in consequence of local peculiarities, but seldom acts with maximum energy. Alaska, for instance, is hardly ever favoured with a full amount of solar heat; nor does Rome, we are now informed by the Italian physicist, receive maximum solar heat excepting during winter, owing, it may be imagined, to the absorptive power of the atmosphere of the Campagna during summer.

Without entering the field of speculation, let us consider that the established diminution of solar heat on the ecliptic, nearly 18° Fah., proves the existence of a powerful retarding medium, and points to the presence of a permanent mass of aqueous matter in the higher regions of the atmosphere; necessary, it may be urged, to regulate terrestrial temperature and render vegetable life possible under the destructive vicissitudes of heat and cold, inevitable in the absence of a permanent regulator. The assumption that the supposed mass of aqueous matter is nearly invariable, and at all times present, can alone account satisfactorily for the remarkable fact that, whenever a clear sun is presented, either by the opening of the clouds or by their disappearance, the actinometer indicates the same temperature, subject only to the variations depending on the sun's zenith distance, and the varying position of the earth in its orbit. The variation of temperature produced by the latter cause is entered in Table B, for every fifth day in each month. This table, an extract from a more elaborate one showing the temperature for every day in the year, the meteorologist will find indispensable to harmonise observations made at different seasons. It may be mentioned that the attempt to construct a curve, the ordinates of which would determine the temperature for different zenith distances, at first met with apparently insuperable difficulty. The result of observations made at different seasons under the most favourable circumstances, failed to produce a regular curve until the change of temperature corresponding with the varying distance between the sun and the earth was determined and introduced in the calculation. This at once harmonised the previously conflicting observations, and rendered the task easy of perfecting the curve, and obtaining ordinates consistent with the observed temperature produced by solar radiation at different seasons and different zenith distance.

Regarding Table A, it will suffice to state that it is based upon our acquired knowledge of the temperature produced by solar radiation at given zenith distances when the earth is in aphelion. Evidently if we know that, for instance, when the sun's zenith distance is 43° the temperature is 60·57° Fah., we know also that this is the temperature at noon on the Arctic Circle, the latter being 43° from the ecliptic at the summer solstice. Again, the North Pole being 66° 30′ from the ecliptic at the same time, we find by referring to the figures entered in the table of zenith distances and temperatures (previously published) that the depth of atmosphere to be penetrated by the rays when the sun is 66°, 30′ from the zenith, is 2·444 times greater than on the ecliptic; and that, therefore, the radiant intensity, as shown in the table, is reduced from 67·20° at the tropic of Cancer to 49·91° Fah. at the pole. Possibly it may be found necessary to introduce a correction for the difference of atmospheric density in the higher latitudes; but at present I deem it inexpedient to complicate the matter by applying a correction which obviously cannot affect the general result.

J. ERICSSON

NOTES

THE Anniversary Meeting of the Royal Society will be held on the 30th inst., when Sir Edward Sabine will deliver his eleventh and last anniversary address. Prof. G. B. Airy, the Astronomer Royal, will be brought forward as his successor.

AT the opening meeting for the session of the Royal Geographical Society, on Monday evening last, the president, Sir H. Rawlinson, announced that, in consideration of Dr. Livingstone's services, Her Majesty's Government had been pleased to grant to his children the sum of 300*l*.

THE following are the lecture arrangements for 1871-72 at the Royal Institution, Albemarle Street:—Prof. Tyndall, F.R.S.: six lectures on "Ice, Water, and Air," on December 28, 30, 1871; January 2, 4, 6, 9, 1872. Dr. W. Rutherford, F.R.S.E.: ten lectures on "The Nervous and Circulatory System," on Tuesdays, January 16 to March 19. Prof. Odling, F.R.S.: ten lectures on "The Chemistry of Alkalies and Alkali Manufacture," on Thursdays, January 11 to March 21. Mr. W. G. Clark, late Public Orator: six lectures on "The History of Dramatic Literature, Ancient and Modern," on Saturdays, January 20 to Feb. 24. Mr. Moncure D. Conway: four lectures on "Demonology," on Saturdays, March 2 to 23. The Friday evening meetings will commence on January 13. The Friday evening discourses before Easter will probably be given by Mr. W. R. Grove, the Archbishop of Westminster, Professors Odling and Humphrey, Dr. Gladstone, Messrs. C. W. Siemens, R. Liebreich, and John Evans, and Prof. Tyndall. Dr. Wm. A Guy, F.R.S.: three lectures on "Statistics, Social Science, and Political Economy," on Tuesdays, April 9, 16, and 23. Mr. Edward B. Tylor, F.R.S.: six lectures on "The Development of Belief and Custom amongst the Lower Races of Mankind," on Tuesdays, April 30 to June 4. Prof. Tyndall, F.R.S.: nine lectures, on Thursdays, April 11 to June 6. Mr. R. A. Proctor, F.R.A.S.: five lectures on "Star Depths," on Saturdays, April 13 to May 11. Prof. Roscoe, F.R.S.: four lectures on "The Chemical Action of Light," on Saturdays, May 18 to June 8.

THE following Lectures to Women, on the Elements of Physical Science, will be delivered during the ensuing term, in the Lecture Theatre of the South Kensington Museum, by Professors Huxley, Guthrie, and Duncan. Professor Duncan: ten lectures on "Elementary Physiography," commencing on Saturday the 18th November, and ending on the 20th December; Saturdays and Wednesdays at 2.30. Professor Guthrie: fifteen lectures on "Elementary Physics and Chemistry," commencing on Wednesday the 10th January, and ending on Wednesday the 28th February; Wednesdays and Saturdays, at 2.30. Professor Huxley. ten lectures on "Elementary Biology," commencing on Saturday the 2nd March, and continued on Saturdays only at 2.30 P.M., on the 9th, 16th, 23rd March; 13th, 20th, 27th April; 4th, 11th, 18th May.

A CLASS for the teaching of Natural Science has been formed at the College for Women, at Hitchin. Until very recently, classics and mathematics were almost exclusively the subjects brought under the consideration of the students; but a demand for the teaching of Natural Science has arisen, and under the advice of Prof. Liveing, of Cambridge, the subject of Chemistry has been taken to begin with. Prof. Liveing is on the list of lecturers at the College for Women, but in consequence of the weak state of his health—the result of overwork—he is unable to undertake the teaching himself. The actual professor at Hitchin is Mr. Hicks, Natural Science Lecturer at Sidney Sussex College, Cambridge. The lecturer gives one lecture a week, illustrated by experiments; and Mrs. Whelpdale, a lady who has had experience in teaching the subject, also gives supplementary teaching once a week. This lady works under the direction of Mr. Hicks, and acts as a tutor preparing for the lectures. So far as this has been worked, the plan seems to answer exceedingly well. The apparatus considered by Prof. Liveing and Mr. Hicks to be indispensable, has been provided by the college, but the authorities would be glad to make it more complete. Prof. Liveing has kindly promised to lend from Cambridge some of the more expensive things which are not in constant use. It is quite evident, however, that until there is a completely furnished laboratory, with all the appliances requisite for the study of Physical Science, the efforts made for the teaching of such science must be, to a certain extent, partial. It is to be hoped that funds will be forthcoming from some of the friends of the higher education of women to furnish the means for all that is needed in the new college building near Cambridge, to which the College for Women will, in time, be removed.

A BARONETCY has been conferred on Prof. Christison of Edinburgh in recognition of his well-earned position at the head of the profession in Scotland. Prof. Christison already holds the appointment of Honorary Physician to the Queen in Scotland, and is President of the Royal Society of Edinburgh. He has received the honorary doctorate of Oxford, and has been twice President of the Royal College of Physicians of Edinburgh. He has been a professor of the University of Edinburgh since 1822, and is the author of a work on Poisons, which, although written many years since, is still a standard authority; and of a highly esteemed treatise on Materia Medica. Sir Robert Christison is a Crown Member of the General Medical Council, and took a leading part in framing the authorised edition of the *British Pharmacopœia* issued by the Council. Recently, as a mark of especial esteem and respect from his colleagues in the University of Edinburgh and other friends, his bust was sculptured by subscription, and placed in the library of the University—an honour which, according to the *British Medical Journal*, had not previously been conferred on any professor during life.

IN the year 1872 there will be open for competition, at St. John's College, Cambridge, four minor scholarships, two of the value of 70*l*. per annum, and two of 50*l*. per annum, together with three exhibitions of 50*l*. per annum, tenable on the same terms as the minor scholarships, and two of 40*l*. per annum, tenable for four years. The examination of candidates for the above-mentioned scholarships and exhibitions will commence on Tuesday, the 9th of April, 1872. The examination will consist of three mathematical papers and four classical papers. Besides the nine minor scholarships or exhibitions above mentioned, there will be for competition an exhibition of 50*l*. per annum for proficiency in natural science, the exhibition to be tenable for three years in case the exhibitioner has passed within two years the previous examination as required for candidates for honours, otherwise the exhibition to cease at the end of two years. The candidates for the Natural Science Exhibition will have a special examination on Friday and Saturday, the 12th and 13th of April, 1872, in (1) chemistry, including practical work in the laboratory; (2) physics, viz., electricity, heat, and light; (3) physiology. They will also have the opportunity of being examined in one or more of the following subjects—(4) geology, (5) anatomy, (6) botany, provided that they give notice of the subjects in which they wish to be examined four weeks prior to the examination. No candidate will be examined in more than three of these six subjects, whereof one at least must be chosen from the former group. It is the wish of the master and seniors that excellence in some single department should be specially regarded by the candidates. Candidates must send their names to one of the tutors (Rev. S. Parkinson, Rev. T. G. Bonney, and Mr. J. E. Sandys), fourteen days before the commencement of the examination. The minor scholarships are open to all persons under twenty years of age, whether students in the university or not, who have not yet commenced residence in the university or who are in the first term of their residence.

TRINITY COLLEGE, Cambridge, offers one or more of its foundation scholarships, of the value of 80*l*. per annum each, for proficiency in the Natural Sciences. The examination will commence on April 5, and will be open to all undergraduates of Cambridge or Oxford, as well as to persons, under twenty-one, who are not members of the Universities. Further information may be obtained from the Rev. E. Blore, tutor of the college.

THE first course of Cantor Lectures of the Society of Arts for the ensuing session will be "On the Manufacture and Refining of Sugar," by C. Haughton Gill, and will consist of four lectures to be delivered Monday evenings November 27, and December 4, 11, and 18.

AT the late examination for the Natural Science Moderatorship in Trinity College, Dublin, the first senior moderatorship was awarded to Phineas Simon Abraham, the second to Charles B Ball; the junior moderatorships were given to R. D. Purefoy and W. J. Smyly. The subjects for examination were—Comparative and Physiological Anatomy, Zoology, Botany, Physical Geography, and Palæontology.

THE annual general meeting of the Royal Horticultural Society of Ireland was held in Dublin on the 9th of November. The report of the Council was most satisfactory, and the treasurer's account showed a balance on the year to the credit of the society of upwards of 1,060*l*. Of this sum 1,000*l*. was added to the reserve fund. In addition to the usual early Spring, Summer, and Autumn shows it was resolved to hold in October next a grand international fruit show, which we hope will be attended with success.

MR. JOHN RUSKIN has lately presented a valuable collection of minerals and fossils to the High School, Nottingham. Among the former are two hundred metalliferous ores, including some rare specimens from Hungary, a hundred choice silicates, the principal varieties of fluor spar, calcite, and barytes, some agates, and a series of fine gems. The fossils are mainly from the Cretaceous Rocks of Kent and Sussex.

ON Saturday last Sir William Stirling Maxwell was elected Rector of the University of Edinburgh.

THE great Aquarium at the Crystal Palace, of which we recently gave a full description and drawing, was formally opened to the public on Friday evening last by a *soirée*.

THE Session of the Institution of Civil Engineers commenced on the 14th inst., and the annual general meeting "to receive and deliberate upon the report of the Council on the state of the Institution, and to elect the officers for the ensuing year," will be held on Tuesday, the 19th of December. At the same time the members have been reminded of the obligation entered into on election to promote the public and scientific obligations contemplated in the Royal Charter of Incorporation granted to the institution by preparing, or aiding in the preparation of, original communications for reading at the meetings, by frequent attendance at the meetings and occasionally taking part in the discussion, and by presenting to the library copies of reports and scientific treatises not already in the collection. It has also been notified that the qualifications of candidates seeking admission into the institution must in all cases be set forth with the utmost precision and in considerable detail, in order to enable the Council, upon whom the classification involves, and the members, with whom the subsequent election rests, to form a correct opinion as to the nature of the practice, the extent of the experience, and the degree of responsibility of every candidate. The casualties which have occurred among the members of this body during the last three months include the death of Field-Marshal Sir John Burgoyne, G.C.B., &c., honorary member; of Messrs. Joseph Hamilton Beattie, John George Blackburne, Robert Benson Dockray, Albinus Martin, and Josiah Parkes, members; and of Messrs. Arthur Field, Edward Mosely Perkins, and Henry Beadon Rotton, associates. This has reduced the total number of members of all classes from 2,009, at which it stood on the 1st of August last, to 2,000, comprising 14 honorary members, 725 members, 1,056 associates, and 205 students. During the period referred to the ordinary general meetings have been suspended, so that there has been no ballot for new members.

MR. BROTHERS has made a photograph eight inches in diameter of one of Mr. Proctor's star maps, containing nearly fifty thousand stars. The more marked constellations are just distinguishable upon a background, which appears to be shaded with innumerable minute points representing smaller stars. The increase of intensity in the shading is very evident upon certain parts of the picture. The whole represents the heavens as we should see them if the pupils of our eyes were a little more than two inches in diameter.

DR. J. B. PETTIGREW, F.R.S., will deliver a course of twelve lectures on physiological and pathological subjects at the Royal College of Surgeons, Edinburgh.

THE GEOGNOSY OF THE APPALACHIANS AND THE ORIGIN OF CRYSTALLINE ROCKS *

III.

THE direct formation of the crystalline schists from an aqueous magma is a notion which belongs to an early period in geological theory. De la Beche, in 1834,† conceived that they were thrown down as chemical deposits from the waters of the heated ocean, after its reaction on the crust of the cooling globe, and before the appearance of organic life. This view was revived by Daubrée in 1860. Having sought to explain the alteration of palæozoic strata of mechanical origin, by the action of heated waters, he proceeds to discuss the origin of the still more ancient crystalline schists. The first precipitated waters, according to him, acting on the anhydrous silicates of the earth's crust, at a very elevated temperature, and at a great pressure, which he estimated at two hundred and fifty atmospheres, formed a magma, from which, as it cooled, were successively deposited the various strata of the crystalline schists.‡ This hypothesis, violating, as it does, all the notions which sound theory teaches with regard to the chemistry of a cooling globe, has, moreover, to encounter grave geognostical difficulties. The pre-Silurian crystalline rocks belong to two or more distinct systems of different ages, succeeding each other in discordant stratification. The whole history of these rocks, moreover, shows that their various alternating strata were deposited, not as precipitates from a seething solution, but under conditions of sedimentation very like those of more recent times. In the oldest known of them, the Laurentian system, great limestone formations are interstratified with gneisses, quartzites, and even with conglomerates. All analogy, moreover, leads us to conclude that even at this early period life existed at the surface of the planet. Great accumulations of iron-oxyd, beds of metallic sulphids, and of graphite, exist in these oldest strata, and we know of no other agency than that of organic matter capable of generating these products.

Bischof had already arrived at the conclusion, which in the present state of our knowledge seems inevitable, that "all the carbon yet known to occur in a free state can only be regarded as a product of the decomposition of carbonic acid, and as derived from the vegetable kingdom." He further adds, "living plants decompose carbonic acid; dead organic matters decompose sulphates, so that, like carbon, sulphur appears to owe its existence in a free state to the organic kingdom."§ As a decomposition (deoxidation) of sulphates is necessary to the production of metallic sulphids, the presence of the latter, not less than that of free sulphur and free carbon, depends on organic bodies; the part which these play in reducing and rendering soluble the peroxyd of iron, and in the production of iron ores, is, moreover, well known. It was, therefore, that, after a careful study of these ancient rocks, I declared in May, 1858, that a great mass of evidence "points to the existence of organic life, even during the Laurentian or so-called azoic period."‖

This prediction was soon verified in the discovery of the Eozoon Canadense of Dawson, the organic character of which is now admitted by all zoologists and geologists of authority. But with this discovery appeared another fact, which afforded a signal verification of my theory as to the origin and mode of deposition of serpentine and pyroxene. The microscopic and chemical researches of Dawson and myself showed that the calcareous skeleton of this foraminiferal organism was filled with the one or the other of these silicates in such a manner as to make it evident that they had replaced the sarcode of the animal, precisely as glauconite and similar silicates have, from the Silurian times to the present, filled and injected more recent foraminiferal skeletons. I recalled, in connection with this discovery, the observations of Ehrenberg, Mantell, and Bailey, and the more recent ones of Pourtalès, to the effect that glauconite or some similar substance occasionally fills the spines of Echini, the cavities of corals and millepores, the canals in the shells of Balanus, and even forms casts of the holes made by burrowing sponges (Clionia) and

* Address of Prof. T. Sterry Hunt on retiring from the office of President of the American Association for the Advancement of Science; abridged from the "American Naturalist," concluded from p. 34.
† Researches in Theoretical Geology, pp. 297-300.
‡ Etudes et expériences synthétiques sur le Metamorphisme, pp. 119-121.
§ Bischof, Lehrbuch, 1st ed. II. 95. English ed. I. 252, 344.
‖ Amer. Jour. Science, II. xxv. 436.

worms. The significance of these facts was further illustrated by showing that the so-called glauconites differ considerably in composition, some of them containing more or less alumina or magnesia, and one from the tertiary limestones near Paris being, according to Berthier, a true serpentine.*

These facts in the history of Eozoön were first made known by me in May 1864, in the *American Journal of Science*, and subsequently more in detail, February 1865, in a communication to the Geological Society of London.† They were speedily verified by Dr. Gümbel, who was then engaged in the study of the ancient crystalline schists of Bavaria, and who soon recognised the existence, in the limestones of the old Hercynian gneiss, of the characteristic Eozoön Canadense, injected with silicates in a manner precisely similar to that observed by Dawson and myself.‡ Later, in 1869, Robert Hoffmann described the results of a minute chemical examination of the Eozoön from Raspenau, in Bohemia, confirming the previous observations in Canada and Bavaria. He showed that the calcareous shell of the Eozoön examined by him, had been injected by a peculiar silicate, which may be described as related in composition both to glanconite and to chlorite. The masses of Eozoön he found to be enclosed and wrapped around by thin alternating layers of a green magnesian silicate allied to picrosmine, and a brown non-magnesian mineral, which proved to be a hydrous silicate of alumina, ferrous oxyd, and alkalies, related in composition to fahlunite, or more nearly to jollyte.§

Still more recently, in the course of the present year, Dr. Dawson detected a mineral insoluble in acids, injecting the pores of crinoidal stems and plates in a palæozoic limestone from New Brunswick, which is made up of organic remains. This silicate which, in decalcified specimens, shows in a beautiful manner the intimate structure of these ancient crinoids, I have found by analysis to be a hydrous silicate of alumina and ferrous oxyd, with magnesia and alkalies, closely related to fahlunite and to jollyte.‖ The microscopic examinations of Dr. Dawson show that this silicate injected the pores of the crinoidal remains and some of the interstices of the associated shell-fragments, before the introduction of the calcite which cements the mass. I have since found a silicate almost identical with this, occurring under similar conditions in an Upper Silurian limestone said to be from Llangedoc in Wales.

Gümbel, meanwhile, in the essay on the Laurentian rocks of Bavaria, in 1866, already referred to, fully recognised the truth of the views which I had put forward, both with regard to the mineralogy of Eozoön and to the origin of the crystalline schists. His results are still farther detailed in his *Geognost. Beschreibung des östbayerischen Grenzegebirges*, 1868, p. 833. Credner, moreover, as he tells us,¶ had already from his mineralogical and lithological studies, been led to admit my views as to the original formation of serpentine, pyroxene, and similar silicates (which he cites from my paper of 1865, above referred to**), when he found that Gümbel had arrived at similar conclusions. The views of the latter, as cited by Credner from the work just referred to, are in substance as follows :—The crystalline schists, with their interstratified layers, have all the characters of altered sedimentary deposits, and from their mode of occurrence cannot be of igneous origin, nor the result of epigenic action. The originally formed sediments are conceived to have been amorphous, and under moderate heat and pressure to have arranged themselves, and crystallised, generating various mineral species in their midst by a change, which, to distinguish it from metamorphism by an epigenic process, Gümbel happily designates diagenesis.

It is unnecessary to remark that these views, the conclusions from the recent studies of Gümbel in Germany and Credner in North America, are identical with those put forth by me in 1860.

At the early periods in which the materials of the ancient crystalline schists were accumulated, it cannot be doubted that the chemical processes which generated silicates were much more active than in more recent times. The heat of the earth's crust was probably then far greater than at present, while a high temperature prevailed at comparatively small depths, and thermal waters abounded. A denser atmosphere, charged with carbonic acid gas, must also have contributed to maintain, at the earth's surface, a greater degree of heat, though one not incompatible with the existence of organic life.* These conditions must have favoured many chemical processes, which, in later times, have nearly ceased to operate. Hence we find that subsequently to the eozoic times, silicated rocks of clearly marked chemical origin are comparatively rare. In the mechanical sediments of later periods certain crystalline minerals may be developed by a process of molecular re-arrangement—diagenesis. These are, in the feldspathic and aluminous sediments, orthoclase, muscovite, garnet, staurolite, cyanite, and chiastolite, and in the more basic sediments, hornblendic minerals. It is possible that these latter and similar silicates may sometimes be generated by reactions between silica on the one hand and carbonates and oxyds on the other, as already pointed out in some cases of local alteration. Such a case may apply to more or less hornblendic gneisses, for example; but no sediments, not of direct chemical origin, are pure enough to have given rise to the great beds of serpentine, pyroxene, steatite, labradorite, &c., which abound in the ancient crystalline schists. Thus, while the materials for producing, by diagenesis, the aluminous silicates just mentioned, are to be met with in the mud and clay-rocks of all ages, the chemically formed silicates capable of crystallising into pyroxene, talc, serpentine, &c., have only been formed under special conditions.

The same reasoning which led me to maintain the theory of an original formation of the mineral silicates of the crystalline schists, induced me to question the received notion of the epigenic origin of gypsums and magnesian limestones or dolomites. The interstratification of dolomites and pure limestones, and the enclosure of pebbles of the latter in a paste of crystalline dolomite, are of themselves sufficient to show that in these cases, at least, dolomites have not been formed by the alteration of pure limestones. The first results of a very long series of experiments and inquiries into the history of gypsum were published by me in 1859, and further researches, reiterating and confirming my previous conclusions, appeared in 1866.† In these two papers it will, I think, be found that the following facts in the history of dolomite are established, viz.: first, its origin in nature by direct sedimentation, and not by the alteration of non-magnesian limestones; second, its artificial production by the direct union of carbonate of lime and hydrous carbonate of magnesia, at a gentle heat, in the presence of water. As to the sources of the hydrous magnesian carbonate, I have endeavoured to show that it is formed from the magnesian chlorid or sulphate of the sea or other saline waters in two ways :—first, by the action of the bicarbonate of soda found in many natural waters; this, after converting all soluble lime-salts into insoluble carbonate, forms a comparatively soluble bicarbonate of magnesia, from which a hydrous carbonate slowly separates; secondly, by the action of bicarbonate of lime in solution, which, with sulphate of magnesia, gives rise to gypsum; this first crystallises out, leaving behind a much more soluble bicarbonate of magnesia, which deposits the hydrous carbonate in its turn. In this way for the first time, in 1859, the origin of gypsums and their intimate relation with magnesian limestones were explained.

It was, moreover, shown that to the perfect operation of this reaction, an excess of carbonic acid in the solution, during the evaporation, was necessary to prevent the decomposing action of the hydrous mono-carbonate of magnesia upon the already formed gypsum. Having found that a prolonged exposure to the air, by permitting the loss of carbonic acid, partially interfered with the process, I was led to repeat the experiment in a confined atmosphere, charged with carbonic acid, but rendered drying by the presence of a layer of dessicated chlorid of calcium. As had been foreseen, the process under these conditions proceeded uninterruptedly, pure gypsum first crystallising out from the liquid, and subsequently the hydrous magnesian carbonate.‡ This experiment is instructive as showing the results which must have attended this process in past ages, when the quantity of carbonic acid in the atmosphere greatly exceeded its present amount.

As regards the hypotheses put forward to explain the supposed dolomitisation of previously-formed limestones by an epigenic process, I may remark that I repeated very many times, under varying conditions, the often-cited experiment of Von Morlot, who claimed to have generated dolomite by the action of sulphate of magnesia on carbonate of lime, in the presence of water at a

* Amer. Jour. Sci. II. xl. 360, Report Geol. Survey Canada, 1866, p. 231, and Quar. Geol. Jour. XXI. 71.
† Amer. Jour. Sci. II. xxxvii. 431. Quar. Geol. Jour. XXI. 67.
‡ Proc. R. Bavar. Acad. for 1866, and Canadian Naturalist, N. S., III. 81.
§ Jour. für Prakt. Chem. May, 1869, and Amer. Jour. Sci. III. i. 378.
‖ Amer. Jour. Sci. III. i. 379.
¶ Hermann Credner: die Gleiderung der Eozoischen Formationsgruppe Nord Amerikas. Halle, 1869.
** That in the Quar. Geol. Jour. XXI. 67.

* Amer. Jour. Sci. II. xxxvi. 396.
† Amer. Jour. Sci. II xxxviii. 170, 365; xlii. 49.
‡ Proceedings Royal Institution, May 30, 1867, and Canadian Naturalist, New Series, III. 231.

somewhat elevated temperature under pressure. I showed that what he regarded as dolomite was not such, but an admixture of carbonate of lime with anhydrous and sparingly soluble carbonate of magnesia; the conditions in which the carbonate of magnesia is liberated in this reaction not being favourable to its union with the carbonate of lime to form the double salt which constitutes dolomite. The experiment of Marignac, who thought to form dolomite by substituting a solution of chlorid of magnesium for the sulphate, I found to yield similar results, the greater part of the magnesian carbonate produced passing at once into the insoluble condition, without combining with the excess of carbonate of lime present. The process for the production of the double carbonate described by Ch. Deville, namely, the action of vapours of anhydrous magnesian chlorid on heated carbonate of lime, in accordance with Von Buch's strange theory of dolomitisation, I have not thought necessary to submit to the test of experiment, since the conditions required are scarcely conceivable in nature. Multiplied geognostical observations show that the notion of the epigenic production of dolomite from limestone is untenable, although its resolution and deposition in veins, cavities, or pores in other rocks is a phenomenon of frequent occurrence.

The dolomites or magnesian limestones may be conveniently considered in two classes; first, those which are found with gypsums at various geological horizons; and secondly, the more abundant and widely distributed rocks of the same kind, which are not associated with deposits of gypsum. The production of the first class is dependent upon the decomposition of sulphate of magnesia by solutions of bicarbonate of lime, while those of the second class owe their origin to the decomposition of magnesian chlorid or sulphate by solutions of alkaline bicarbonates. In both cases, however, the bicarbonate of magnesia, which the carbonated waters generally contain, contributes a more or less important part to the generation of the magnesian sediments. The carbonated alkaline waters of deep-seated springs often contain, as is well known, besides the bicarbonates of soda, lime and magnesia, compounds of iron, manganese, and many of the rarer metals in solution, and thus the metalliferous character of many of the dolomites of the second class is explained. The simultaneous occurrence of alkaline silicates in such mineral waters, would give rise, as already pointed out, to the production of insoluble silicates of magnesia, and thus the frequent association of such silicates with dolomites and magnesian carbonates in the crystalline schists is explained, as marking portions of one continuous process. The formation of these mineral waters depends upon the decomposition of feldspathic rocks by subterranean or sub-aërial processes, which were doubtless more active in former ages than in our own. The subsequent action upon magnesian waters of these bicarbonated solutions, whether alkaline or not, is dependent upon climatic conditions, since, in a region where the rainfall is abundant, such waters would find their way down the river-courses to the open sea, where the excess of dissolved sulphate of lime would prevent the deposition of magnesian carbonate. It is in dry and desert regions, with limited lake-basins, that we must seek for the production of magnesian carbonates, and I have argued from these considerations that much of north-eastern America, including the present basins of the Upper Mississippi and St. Lawrence, must, during long intervals, in the palæozoic period, have had a climate of excessive dryness, and a surface marked by shallow enclosed basins, as is shown by the widely-spread magnesian limestones, and the existence of gypsum and rock-salt at more than one geological horizon within that area.* The occurrence of serpentine and diallage at Syracuse, New York, offers a curious example of the local development of crystalline magnesian silicates in Upper Silurian dolomitic strata under conditions which are imperfectly known, and which, in the present state of the locality, cannot be studied.†

Since the uncombined and hydrated magnesia mono-carbonate is at once decomposed by sulphate or chlorid of calcium, it follows that the whole of these lime-salts in a sea-basin must be converted into carbonates before the production of carbonated magnesian sediments can begin. The carbonate of lime formed by the action of carbonates of magnesia and soda, remains at first dissolved as bicarbonate, and is only separated in a solid form, when, in excess, or when required for the needs of living plants or animals, which are dependent for their supply of calcareous matter, on the bicarbonate of lime produced, in part by the process just described, and in part by the action of carbonic acid on insoluble lime-compounds of the earth's solid crust So many limestones are made up of calcareous organic remains, that a notion exists among many writers on geology that all limestones are, in some way, of organic origin. At the bottom of this lies the idea of an analogy between the chemical relations of vegetable and animal life. As plants give rise to beds of coal, so animals are supposed to produce limestones. In fact, however, the synthetic process by which the growing plant, from the elements of water, carbonic acid and ammonia, generates hydrocarbonaceous and azotised matters, has no analogy with the assimilative process by which the growing animal appropriates alike these organic matters and the carbonate and phosphate of lime. Without the plant, the synthesis of the hydrocarbons would not take place, while independently of the existence of coral or mollusk, the carbonate of lime would still be generated by chemical reactions, and would accumulate in the waters until, these being saturated, its excess would be deposited as gypsum or rock-salt are deposited. Hence, in such waters, where, from any causes, life is excluded, accumulations of pure carbonate of lime may be formed. In 1861 I called attention to the white marbles of Vermont, which occur intercalated among impure and fossiliferous beds, as apparently examples of such a process.*

It is by a fallacy similar to that which prevails as to the organic origin of limestones, that Daubeny and Murchison were led to appeal to the absence of phosphates from certain old strata as evidence of the absence of organic life at the time of their accumulation.† Phosphates, like silica and iron-oxyd, were doubtless constituents of the primitive earth's crust, and the production of apatite crystals in granitic veins, or in crystalline schists, is a process as independent of life as the formation of crystals of quartz or of hematite. Growing plants, it is true, take up from the soil or the waters dissolved phosphates, which passed into the skeletons of animals, a process which has been active from very remote periods. I showed in 1854 that the shells of Lingula and Orbicula, both those from the base of the palæozoic rocks and those of the present time have (like Conularia and Serpulites) a chemical composition similar to the skeletons of vertebrate animals.‡ The relations of both carbonate and phosphate of lime to organised beings are similar to those of silica, which, like them, is held in watery solution, and by processes independent of life is deposited both in amorphous and crystalline forms, but in certain cases is appropriated by diatoms and sponges, and made to assume organised shapes. In a word, the assimilation of silica, like that of phosphate and carbonate of lime, is a purely secondary and accidental process, and where life is absent, all of these substances are deposited in mineral and inorganic forms.

I have thus endeavoured to sketch, in a concise and rapid manner, the history of the earlier rock-formations of eastern North America, and of our progress in the knowledge of them; while I have, at the same time, dwelt upon some of the geognostical and chemical questions which their study suggests. With the record of the last thirty years before them, American geologists have cause for congratulation that their investigations have been so fruitful in great results. They see, however, at the same time, how much yet remains to be done in the study of the Appalachians and of our north-eastern coast, before the history of these ancient rock-formations can be satisfactorily written. Meanwhile our adventurous students are directing their labours to the vast regions of western America, where the results which have already been obtained are of profound interest. The progress of these investigations will doubtless lead us to modify many of the views now accepted in science, and cannot fail greatly to enlarge the bound of geological knowledge.

* Geology of South-western Ontario, Amer. Jour. Sci. II. xlvi. 355.
† Geology of the 3rd district of New York, 108 110, and Hunt on Ophiolites, Amer. Jour. Sci. II. xxvi. 236.

* Amer. Jour. Sci. II. xxxi. 402.
† Siluria, 4th ed. pp. 28 and 537.
‡ Amer. Jour. Sci. II. xvii. 236.

THE SCOTTISH SCHOOL OF GEOLOGY §

II.

WHILE Hutton fortified his convictions by constant appeals to the rocks themselves, his disciple Hall tested their truth in the laboratory. It is the boast of Scotland to have led the way in the application of chemical and physical experiment to the elucidation of geological history. It was objected to Hutton's theory, that if basalt and similar rocks had ever been in a

§ A Lecture delivered at the opening of the class of Geology and Mineralogy in the University of Edinburgh, by Archibald Geikie, F.R.S., Nov. 6, 1871, concluded from p. 39.

melted state, they would now have been seen in the condition of glass or slag, and not with the granular or crystalline texture which they actually possess. Hall demolished this objection by melting basalt into a glass, and then by slow cooling reconverting it into a granular substance like the original rock. Hutton had maintained that under enormous pressure, such as he conceived must exist under the ocean, or deep within the crust of the earth, even limestone itself might be melted without losing its carbonic acid. This was ridiculed by his opponents, on whom he retorted that they "judged of the great operations of the mineral kingdom from having kindled a fire and looked into the bottom of a little crucible." Hall, however, to whom fire and crucible were congenial implements, resolved to put the question to the test of experiment, and though, out of deference to his master, he delayed his task until after the death of the latter, he did at last succeed in converting limestone, under various great pressures, into a kind of marble, and even in reducing it to complete fusion, in which state it acted powerfully on other rocks. He concluded his elaborate essay on this subject with these words, "This single result affords, I conceive, a strong presumption in favour of the solution which Dr. Hutton has advanced of all the geological phenomena; for the truth of the most doubtful principle which he has assumed has thus been established by direct experiment."

Though they saw clearly the proofs which the rocks afford us of former revolutions, neither Hutton nor his friends had any conception of the existence of the great series of fossiliferous formations which has since been unfolded by the labours of later observers—that voluminous record in which the history of life upon this planet has been preserved. They spoke of "Alpine schistus," "primary" or "secondary" strata, as if the geological past had consisted but of two great ages—the second replete with traces of the destruction of the first. "The ruins of an older world," said Hutton, "are visible in the present structure of our planet." He knew nothing of the long, but then undiscovered, succession of such "ruins," each marking a wide interval of time. Nevertheless, for the establishment of the great truths which Hutton laboured to confirm, such knowledge was not necessary. On the other hand, it was most needful that the significance of that discordance between the older and newer strata which Hutton recognised should be persistently proclaimed. And the Huttonians, in spite of their limited range of knowledge and opportunity, saw its value and held by it.

2. But it was not merely, or even chiefly, for their exposition of the structure and history of the rocks under our feet that the geologists of the Scottish School deserve to be held in lasting remembrance. They could not, indeed, have advanced as far as they did in expounding former and ancient conditions of the planet, had they not, with singular clearness, perceived the order and system of change which is in progress over the surface of the globe at the present day. It was their teaching which first led men to see the harmony and co-operation of the forces of nature which work within the earth, with those which are seen and felt upon its surface. Hutton first caught the meaning of that constant circulation of water which, by means of evaporation, winds, clouds, rain, snow, brooks, and rivers, is kept up between land and sea. He saw that the surface of the dry land is everywhere being wasted and worn away. The scarped cliff, the rugged glen, the lowland valley, are each undergoing this process of destruction; wherever land rises above ocean, there, from mountain-top to sea-shore, degradation is continually going on. Here and there, indeed, the *débris* of the hills may be spread out upon the plains; here and there, too, dark angular peaks and crags rise as they rose centuries ago, and seem to defy the elements. But these are only apparent and not real exceptions to the universal law, that so long as a surface of land is exposed to the atmosphere it must suffer degradation and removal.

But Hutton saw, further, that this waste is not equally distributed over the whole face of the dry land, that while, owing to the greater or less resistance offered by different kinds of rocks, the rate of decay must vary indefinitely, the amount of material must necessarily be greatest where the surplus water flows off towards the sea, that is, along the channels of the streams. Water-courses, he argued, are precisely in the lines which water would naturally follow in running down the slope of the land from its water-shed to the sea, and which, when once selected by the surplus drainage, would necessarily be continually widened and deepened by the excavating power of the rivers. Hence he regarded the streams and rivers of a country as following the lines which they had chiselled for themselves out of the solid land, and thus he arrived at the deduction that valleys have been, inch by inch and foot by foot, dug out of the solid framework of the land by the same natural agents—rain, frost, springs, rivers—by which they are still made wider and deeper. "The mountains," he said, "have been formed by the hollowing out of the valleys, and the valleys have been hollowed out by the attrition of hard materials coming from the mountains." This is a doctrine which is only now beginning to be adequately realised. Yet to Hutton it was so obvious as to convince him, to use his own memorable words, "that the great system upon the surface of this earth is that of valleys and rivers, and that however this system shall be interrupted and occasionally destroyed, it would necessarily be again formed in time while the earth continued above the level of the sea."

Although these views were again and again proclaimed by Hutton in the pages of his treatise, and though Playfair, catching up the spirit of his master, preached them with a force and eloquence which might almost have insured the triumph of any cause, they met with but scant acceptance. The men were before their time; and thus, while the world gradually acknowledged the teaching of the Scottish school as to the past history of the rocks, it lent an incredulous ear to that teaching when dealing with the present surface of the earth. Even some of the Huttonians themselves refused to follow their master when he sought to explain the existing inequalities of the land by the working of the same quiet unobtrusive forces which are still plying their daily tasks around us. But no incredulity or neglect can destroy the innate vitality of truth. And so now, after the lapse of fully two generations, the views of Hutton have in recent years been revived, and have become the war-cry of a yearly increasing crowd of earnest hard-working geologists.

While they insisted upon the manifest proofs of constant and universal decay over the surface of the globe, the Scottish geologists no less strongly contended that the decay was a necessary part of the present economy of Nature, that it had been in progress from the earliest periods in the history of the earth, and that it was essential for the presence of organised beings upon the planet. They pointed to the vegetable soil, derived from the decomposition of the rocks which it covers, and necessary for the support of vegetable life. They appealed to the vast quantity of sedimentary rocks forming the visible part of the crust of the earth, and bearing witness in every bed and layer to the degradation and removal of former continents. They showed that the accumulated *débris* of the land, carried to sea, was there spread out on the sea-floor to form new strata, which, in due time hardened into solid rock, would hereafter be upheaved to form the framework of new lands.

Such was the geology of the Scottish School. It was based not on mere speculation, but on facts drawn from mountain and valley, hill and plain, and tested as far as was then possible by the scrutiny of actual experiment. It strove, for the first time in the history of science, to evolve a system out of the manifold complications of nature, to harmonise what had seemed but the wild random working of subterranean forces with the quiet operations in progress upon the surface of the earth, to understand what is the present system of the world, and through that to peer into the history of the earlier conditions of the planet. It taught that the earthquake and volcano were parts of the orderly arrangement by which new continents were from time to time raised up to supply the place of others which had been worn away; that the surface of the land required to decay to furnish life to plants and animals; that in the removal of the *débris* thus produced mountains and valleys were carved out; and that in the depths of the ocean there were at the same time laid down the materials for the formation of other lands, which in after ages would be upheaved by underground forces, to be anew worn away as before. The Scottish School proclaimed that in the inorganic world there is ceaseless change, that this change is the central idea of the system, and that in its constant progress lie the conditions necessary for the continuance of our earth as a habitable globe.

That Hutton and his followers should have seen only a part of the truth, that they did not perceive the full scope which their views would ultimately acquire, that they fell into errors, and attached to some secondary parts of their system an importance which we now see to have been misplaced, is only what may be said of any body of men who, at any time, have led the way in a new development of human inquiry. But, after all allowance is made for such shortcomings, we see that their errors were for the most part on mere matters of detail, and that the fundamental principles which they laboured to establish have become the very life and soul of modern geology

I have spoken of this Scottish School as marking a period of activity which rose into brightness and then waned. It is only too true, that so far as the originality and influence of its cultivators go, Geology has never since held in Scotland the place which it held here at the beginning of the century. Its decay is perhaps to be ascribed chiefly, if not entirely, to the introduction of the doctrines of Werner from Germany. The Huttonians had dealt rather with general principles than with minute details; they were weak in accurate mineralogical knowledge—not that they were ignorant of or in any degree despised such knowledge; but it was not necessary for their object. When, however, the system of Werner came to be taught within these walls by his enthusiastic pupil Jameson, its precision and simplicity, and its supposed capability of ready application in every country, joined to the skill and zeal of its teacher, gave it an impulse which lasted for years. I shall have occasion in a subsequent lecture to speak of this system. It is enough for the present to describe it as a crude and artificial attempt to explain the geological history of the globe from the rocks of a district in Saxony. It required mineralogical determination of rocks, and in so far it did good service, but its theoretical teaching in matters of geology cannot now be regarded without a smile. It maintained that the globe was covered with certain universal formations, and that these had been precipitated successively from solution in a primeval ocean. Of upheaval and subsidence, earthquakes and volcanos, and all the mechanism of internal heat, it could make nothing, and ignored as much as it dared. Werner, the founder of this system, had the faculty of attaching his students to him, and of infusing into them no small share of his own zeal and faith in his doctrines. His pupil Jameson had a similar aptitude. Skilled in the mineralogy of his time, and full of desire to apply the teachings of Freyberg to the explication of Scottish geology or geognosy, as he preferred to call it, he gathered round him a band of active observers, who gleaned facts from all parts of Scotland, and to whom the first accurate descriptions of the mineralogy of the country are due. It is but fitting that a tribute of gratitude should on the present occasion be offered to the memory of Jameson for the life-long devotion with which he taught Natural History, and especially Mineralogy, in this University. His influence is to be judged not by what he wrote, but by the effect of his example, and by the number of ardent naturalists who came from his teaching. He founded a scientific Society here, and called it Wernerian, after his chief—a Society which under his guidance did excellent service to the cause of science in Scotland. And yet in the course of my scientific reading I have never met a sadder contrast than to turn from the earlier volumes of the Royal Society of Edinburgh, containing the classic essays of Hutton, Hall, and Playfair—essays which made an epoch in the history of Geology—to the pages of the Wernerian memoirs, and find grave discussions about the universal formations, the aqueous origin of basalt, and the chemical deposition of such rocks as slate and conglomerate!

Between the followers of Hutton and Werner there necessarily arose a keen warfare. The one battalion of combatants was styled by its opponents "Vulcanists" or "Plutonists," as if they recognised only the power of internal fire, while the other was in turn nicknamed "Neptunists," in token of their adherence to water. The warfare lasted in a desultory way for many years, and though the Wernerian school, having essentially no vitality, eventually died out, and its leader Jameson publicly and frankly recanted his errors, the early Huttonian magnates had one by one departed and left no successors. The Huttonian school triumphed indeed, but its triumph was seen rather in other countries than in Scotland. Here the Wernerian school attracted in great measure the younger men who gathered round Jameson, and when its influence waned there were no great names on the other side to rally the thinned and weakened ranks of Huttonianism. Hence came a period of comparative quiescence, which has lasted almost down to our own day. From time to time, indeed, a geologist has arisen among us to show that the science was not dead, and that the doctrines of Hutton had borne good fruit. But Geology has never since held such a prominent place amongst us, nor have the writings of geologists in Scotland taken the same position in the literature of the science. The great name of Lyell, and others of lesser note, have earned elsewhere their title to fame.

But there is one name which must be in our hearts and on our lips to-day, that of Roderick Impey Murchison. To his munificence, and the liberality of the Crown, we owe the foundation of this Chair of Geology, and to his warm friendship I am indebted for the position in which I stand before you. Of his achievements in science, and of the influence of his work all over the world, it is not necessary now to speak. But on Scottish Geology no man has left his name more deeply engraven. It was he who, along with Prof. Sedgwick, first made known the order of succession of the Old Red sandstone of the north of Scotland; it was he who sketched for us the relations of the great Silurian masses of the Southern uplands; and it was he who, by a series of admirable researches, brought order out of the chaos of the so-called Primary rocks of the Highlands, and placed these rocks in a parallel with the Silurian strata of other conntries. These labours will come again before us in detail, and you will then better understand their value, and the debt we owe to the man who accomplished them.

Sir Roderick Murchison looked forward with interest to the occasion which has called us together to-day. Only a few weeks ago I talked with him regarding it, and his eye brightened as I told him of the subject on which I proposed to speak to you. I had hoped that he would have lived to see this day, and to hear at least of the beginning of the work which he has inaugurated for us in this University. But this was not to be. He has been taken from us ripe in years, in work, and in honours, and he leaves us the example of his unwearied industry, his admirable powers of observation, and his rare goodness of heart.

In the course of study now before us, we are to be engaged in examining together the structure and history of the earth. We shall trace the working of the various natural agents which are now carrying on geological change, and by which the past changes of the globe have been effected. In so doing we shall be brought continually face to face with the history of life as recorded in the rocks—for it is by that history mainly that the sequences of geological time can be established. We shall thus have to trespass a little on what is the proper domain of the professors of botany and of natural history. But you will find that no hard line can be drawn between the sciences. Each must needs overlap upon the other; and indeed it is in this mutual interlacing that one great element of the strength and interest of science lies. From Profs. Balfour and Wyville Thomson you will learn the structure and the relation borne to living plants and animals by the fossils with which we shall have to deal as our geological alphabet. By Prof. Crum Brown you are taught the full meaning and application of the chemical laws under which the minerals and rocks, which we in this class must study, have been formed, and of the processes concerned in those subsequent changes, both of rocks and minerals, which are of such paramount importance in Geology.

And now, in conclusion, permit me to give expression to the feelings which must strongly possess the mind of one who is called upon to fill the first Chair dedicated in Scotland to the cultivation of Geology. When I look back to the times of that illustrious group of men—Hutton, Hall, Playfair—who made Edinburgh the special home of Geology; of Boué and Macculloch, who gave to Scottish rocks and mountains an European celebrity; of Jameson and Edward Forbes, who did so much to stimulate the study of Geology and Mineralogy in this University; and to the memory of Hugh Miller and Charles Maclaren, who fostered the love of the sciences throughout the community here, and to whose kindly friendship and guidance, given to me in my boyhood, I would fain express my hearty gratitude—when I cast my thoughts back upon these recollections, it would be affectation to conceal the anxiety with which the prospect fills me. The memory of these great names arises continually before me, bearing with it a consciousness of the responsibility under which I lie to labour earnestly not to be unworthy of the traditions of the past. And, gentlemen, I feel deeply my responsibility to you who are to enter with me upon a yet untrodden path of the Academic curriculum. It is only experience that will show us how we shall best travel over the wide field before us. In the meantime I must bespeak your kindly forbearance. While I shall cheerfully teach you all I know, and confess what I do not know, I would fain have you in the end to regard me rather in the light of a fellow-student, searching with you after truth, than of a teacher putting before you what is already known. We have now an opportunity of combined and sedulous work which has not hitherto been obtainable in Scotland. We may not rival a Hutton or a Hall; but we may at least try to raise again the standard of geological inquiry here. On every side of us are incentives to study. Crag and hill rise around us, each eloquent of ancient revolutions, and each a silent witness of the revolution in progress now. At our very gates tower on one side the picturesque memorials of long silent volcanoes, with their crumbling lavas and ashes. On the other lie the buried vegetation of an ancient land, and the corals and shells of a former ocean. Everywhere the scarred and wasted rocks tell of the degradation of the solid land, and show us how the waste goes on. Let us then carry into our task some share of the enthusiasm which these daily exemplars called forth in earlier days. Let us turn from the lessons of the lecture room to the lessons of the crags and ravines, appealing constantly to Nature for the explanation and verification of what is taught. And thus, whatsoever may be your career in future, you will in the meantime cultivate habits of observation and communion with the free fresh world around you—habits which will give a zest to every journey, which will enable you to add to the sum of human knowledge, and which will assuredly make you wiser and better men.

SOCIETIES AND ACADEMIES

London

Zoological Society, November 7.—Prof. Newton, F.R.S., vice-president, in the chair. The secretary read a report on the additions that had been made to the society's menagerie during the months of June, July, August, and September 1871, amongst which were specimens of the Tamandua Ant-eater (*Tamandua tetradactyla*), Baird's Tapir (*Tapirus bairdi*), and several other animals of special interest.—A communication was read from Mr. Gerard Krefft, Curator of the Australian Museum, Sydney, N.S.W., containing notes on a rare Ziphioid Whale, which had been stranded near Sydney, and which appeared to be referable to *Ziphius layardi*.—Mr. Gould exhibited and characterised a small but lovely Fruit Pigeon from the Fiji Islands, which he proposed to name *Chrysœna victor*.—Mr. Sclater called attention to the supposed existence of an undescribed animal, of about the size of a Dingo, in the Rockingham Bay district of Queensland, and read a letter addressed to him by Mr. Brinsley G. Sheridan, containing particulars on this subject.—Dr. John Anderson, of Calcutta, communicated a description of a short tailed Macaque from Upper Burmah, supposed to be new, which he proposed to call *Macacus brunneus*.—A communication was read from the Viscount Walden on a new and interesting Falconine Bird, of the genus *Polihierax*, recently obtained by Major Lloyd, in the vicinity of Tongoo, Upper Burmah, and proposed to be called *Polihierax insignis*.—Mr. W. H. Flower, F.R.S., read a memoir on the recent Ziphioid Whales, among which he proposed to recognise the following generic types:—*Hyperoodon*, *Ziphius*, *Mesoplodon*, and *Berardius*. This was followed by a description of the skeleton of *Berardius arnouxii*, founded on a specimen lately transmitted to the Museum of the Royal College of Surgeons from New Zealand by Dr. J. Haast, F.R.S.—Mr. Herbert Taylor Usher read some notes on the habits of the Horned Viper (*Vipera nasicornis*), as observed by him in the vicinity of Cape Coast Castle, Western Africa.—Prof. Newton read a notice of a remarkable peculiarity which he had recently discovered in an Australian duck, *Anas punctata*, viz., that in this species the osseous development of the lower trachea was common to both sexes.—A paper by Dr. J. C. Cox, of Sydney, was read, describing a new Volute and twelve new species of Land Shells from Australia and the Solomon Islands.—A communication was read from Surgeon Francis Day, Inspector-General of Fisheries of British India, containing some remarks on the identification of certain species of Indian Fishes.—Mr. P. L. Sclater, F.R.S., read some notes on Pelicans, being supplementary to a previous paper on the same subject read at a meeting of the society in May, 1868.—A communication was read from Mr. J. Brazier, of Sydney, containing descriptions of eight new Australian Land Shells.—Prof. Newton communicated a paper by Dr. J. Murie, containing supplementary notes concerning the powder-down patches of *Rhinochetus jubatus*.

Anthropological Institute, November 6.—Sir John Lubbock, Bart., F.R.S., president, in the chair. Mr. M. Allport, F.R.S., was elected a corresponding member for Tasmania.—Mr. J. W. Flower, F.G.S., treasurer, read a paper "On the relative ages of the Flint and Stone Implement Periods in England." In this paper, which was illustrated by the exhibition of a series of various kinds of flint implements, the author proposed to show, that having regard to recent discoveries, the arrangement hitherto adopted regarding the Prehistoric Stone period in England as divisible into the Palæolithic and Neolithic was altogether inadequate, and that as well on Geological as on Palæontological grounds the drift period was separable by a vast interval from that of the bone Caves, as the cave period was separable from the Tumulus or Barrow period. The author adduced various reasons for believing that the implements were made and the drift gravel was thrown down long before this island was severed from the Continent, and that thus before that event both countries were inhabited. He also contended, on this and other grounds founded upon recent discoveries, that the implements could not have been transported (if transported at all by fluviatile action) to the places in which they are found by any rivers flowing in the same channels and draining the same areas as now; and he also expressed doubts whether the gravels were transported by river action, and also whether the makers of the implements were contemporary with the Mammalia with whose remains they were associated; the gravel and the fossils having been evidently carried from considerable distances, whereas the implements were made on the spot from stones taken from the gravel. Mr. Flower then pointed out that the works of art found in the caves, as well as the animal remains, differed in many important particulars from those found in the drift, and that those of the Tumulus period differed entirely from those in the caves; that in truth the cave fauna had then quite disappeared, and had been succeeded by one entirely different, including most of our domestic animals, and that for effecting such a change an interval of long duration must be allowed. He also pointed out that the use of bronze was common to both what were known as the Palæolithic and Neolithic periods, and could not be regarded therefore as it usually has been, as distinct from and posterior to both; and, in conclusion, he suggested that the drift period might properly be termed Palæolithic, that of the caves as Archaic, that of the Tumuli as Prehistoric, whilst that of the polished stones might still be known as Neolithic.

Geologists' Association, November 3.—The Rev. Thomas Wiltshire, M.A., F.G.S., president, in the chair. "On the old Land Surfaces of the Globe," by Prof. Morris, F.G.S. The indications of land surfaces to be found in Palæozoic, Mesozoic, and Cainozoic strata were recapitulated. Conglomerates and ripple marks, as well as the great thickness of the oldest sedimentary rocks, the result of denudation, clearly show the existence of land during Cambrian and Silurian times. Though there are indications of vegetable life in Cambrian rocks, the earliest remains of vegetable organisms allied to our present land plants occur in the uppermost Silurian Strata, or passage beds. The Old Red sandstone of Scotland affords evidence of fresh-water origin, and consequently of lakes and land. But in carboniferous rocks we have in the vast accumulations of vegetable remains forming the great coal beds of the world, perhaps the most striking and conclusive proof of land and terrestrial conditions to be found in the geologic record. After noticing the indications of land in the Permian rocks, the Mesozoic reptilia and mammalia, as well as the many other evidences of land surfaces to be met with in the Secondary rocks, were dwelt upon; and a similar review of Cainozoic, or Tertiary, terrestrial indications was followed by an exposition of the upward and onward progress of life, culminating in the present conditions of the globe with a flora and a fauna admirably adapted to the wants of the latest addition to the marvels of the universe, man, whose duty it is, and whose pleasure it ought to be, to study those successive changes, the grand result of which he now enjoys.—A note "On recent exposure of the Glacial Drift at Finchley" was read by Mr. H. Walker. This was a brief notice, and intended as an introduction of the subject, which will be more fully elucidated in a paper by the same author to be read at the next meeting of the association.

Society of Biblical Archæology, November 7.—Dr. S. Birch, president, in the chair. Dr. Richard Cull, F.S.A. read a paper contributed by Mr. Henry Fox Talbot, F.R.S., "On the Religious Beliefs of the Assyrians."—Mr. R. Hamilton Lang, H.B.M. Consul at Cyprus, read a paper "On the Discovery of some Cypriote Inscriptions." After stating that the credit was due to Duc de Luynes of having proved the existence of a Cypriote alphabet, he enumerated the various inscriptions which he had himself discovered, and drew especial attention to one, a bi-lingual inscription in Phœnician and Cypriote, which he first discovered during the excavation of a temple at Idalion. The alphabet, which had been compiled by the Duc de Luynes, consisted of 80 letters, but Mr. Lang felt justified in reducing that number to 51, and exhibited an alphabet which he believed to contain all the Cypriote characters of which we are at present certain. In proceeding he dwelt at some length upon an apparent resemblance between the Cypriote and Lycian alphabets, and stated that they were both derived from the same source, the Lycians having however engrafted upon the ancient forms a great many Grecian Letters, while in Cyprus the character was preserved in its original fulness and power. Mr. Daniel Sharpe had endeavoured to prove that the Lycian alphabet was of Indo-Germanic origin, and so also might be the Cyprian. Mr. Lang alluded to the attempt which had been made both by De Luynes and von Röth to read the Cypriote writing, especially as regarded a word which both gentlemen agreed in rendering "Salamis," and which they considered to be the key to the Cypriote characters. Mr. Lang, on the contrary, gave his reasons for dissenting from this reading upon the testimony of coins, and showed why he thought that the word should be read as "King." The evidence of the bi-lingual inscription before referred to was dwelt upon in confirmation of this reading. A resemblance was further pointed out between the word translated "king" by Mr. Sharpe in Lycian, and that proposed to be read in the same way in Cypriote, and a reading was suggested for the whole of the first line in the Cypriote part of the bi-lingual inscription. Many other points of interest connected with this alphabet were also detailed, and Mr. Lang concluded by observing that in it "we have a child long lost both to the sight and knowledge of the world, and he felt convinced that more extended research would prove that the pedigree of the foundling was of more than usual philological interest and importance."—Mr. G. Smith then read a paper "On the Decipherment of the Cypriote Inscriptions," in which, after alluding to the antiquities discovered by General Cesnola and Mr. Lang, particularly the bi-lingual inscription already mentioned, he went on to detail the discovery of the values of eighteen Cypriote signs from that inscription alone. He further related the discovery of the sounds of twenty other signs by comparison of various texts, together with the reading of the names "Idalium Citium Evagoras," and many others. His conclusions were that the Cypriote language belonged to the Aryan group, and was written with about fifty-four syllabic signs. Diagrams showing case endings of nouns, proper names, and part of the bi-lingual inscription, illustrated the paper. A collection of electrotypes of the Cypriate coins referred to in the foregoing papers was exhibited by Mr. Ready of the British Museum.

Paris

Academy of Sciences, November 6.—A memoir was read by M. A. Mannheim on the properties relating to the infinitely small displacements of a body when these displacements are only defined by four conditions, and one by M. Maurice Lévy on the integration of equations with partial differences relating to the internal movements of ductile solid bodies, when these movements take place in parallel planes.—M. Phillips also communicated a memoir containing a summary of observations made during the last seven years at the Observatory of Neuchatel upon chronometers furnished with spirals with theoretical final curves.—M. P. A. Favre presented a continuation of his thermic investigations upon electrolysis. This paper contains chiefly the results of experiments upon various acids.—General Morin communicated a paper by M. H. Tresca on the effects of torsion prolonged beyond the limits of elasticity.—M. Le Verrier communicated a note on the observation of the flight of meteors of the 12th, 13th, and 14th of this month at the stations of the French Scientific Association.—M. E. Peligot presented a further memoir on the distribution of potass and soda in plants, upon which MM. Dumas and Chevreul made some remarks.—M. I. Pierre presented some observations on the solubility of chloride of silver, with reference to the note on this subject recently communicated by M. Stas.—M. Peligot communicated a note by M. J. Bouis on the determination of hydrochloric acid in cases of poisoning, in which he recommends the heating of the filtered liquids in contact with a plate of gold after the addition of a few fragments of chlorate of potass. The dissolution of the gold indicates the presence of hydrochloric acid, and it is determined by means of protochloride of tin.—M. Berthelot presented a note on the formation of precipitates, in which he commenced the discussion of the phenomena connected therewith, and noticed especially the heat evolved or absorbed during the formation of a solid compound, and the dehydratation of precipitated compounds.—A note by M. F. Cayrol on the Lower Cretaceous formation of Corbières was presented by M. Milne-Edwards. The author compared this formation with that of the Clape, formerly described by him, and stated that it consisted in ascending order of a marly clay containing *Orbitolinæ*, a thick limestone with *Requienia Lonsdalii*, and a second *Orbitolina*-zone, the latter overlain by the Gault.—A note by M. Guido Susain was also read on an improved method of managing the egg-laying of the silkworm moth.—The tables of meteorological observations made at Paris in October was communicated to the Academy.

BOOKS RECEIVED

English.—The Student's Manual of Geology: Jukes and Geikie: 3rd edition (Edinburgh: A. and C. Black).—A Treatise on the Origin, Nature, and Varieties of Wine: Thudichum and Dupré (Macmillan and Co.).—Lights and Shadows of a Canine Life, by Ugly's Mistress (Chapman and Hall).—The Ornithology of Shakespeare: J. E. Harting (Van Voorst).—The Royal Institution; its Founder and its Professors: Dr. Bence Jones (Longmans and Co.).

American.—Illustrated Catalogue of the Museum of Comparative Zoology at Harvard College: No. 4.—Deep-Sea Corals: Count Pourtalès.

Foreign.—Mémoires de la Société de Physique et d'Histoire Naturelle de Genève; Tome xxi.—Nachtrag zum 6 u. 7 Jahresbericht des Vereins für Erdkunde zu Dresden.—Bulletin de la Société Impériale des Naturalistes de Moscou, 1870; Parts 3 and 4.

DIARY

THURSDAY, November 16.

Royal Society, at 8.30.—Considerations on the Abrupt Change at Boiling or Condensing in Reference to the Continuity of the Fluid State of Matter: Prof. J. Thomson.—Magnetic Survey of the East of France in 1869: Rev. S. J. Perry and Rev. W. Sidgreaves.—Action of Hydriodic Acid on Codeia in presence of Phosphorus: Dr. C. R. A. Wright.—Corrections and Additions to the Memoir on the Theory of Reciprocal Surfaces: Prof. Cayley, F.R.S.—On the Dependence of the Earth's Magnetism on the Rotation of the Sun: Prof. Miller.

Linnean Society, at 8.—On the Floral Structure of Impatiens fulva, &c.: A. W. Bennett, F.L.S.—Remarks on Dolichos uniflorus: N. A. Dalzell.—Floræ Hongkongensis Supplementum: H. F. Hance, Ph. D.

Chemical Society, at 8.

London Institution, at 7.30.—The Influence of Geological Phenomena on the Social Life of the People: Harry G. Seeley, F.G.S.

SUNDAY, November 19.

Sunday Lecture Society, at 4.—The Gulf Stream, what it does and what it does not: W. B. Carpenter, M.D., F.R.S.

MONDAY, November 20.

London Institution, at 4.—Consciousness: Prof. Huxley, F.R.S. (Course on Elementary Physiology).

Anthropological Institute, at 8.—Anthropological Collections from the Holy Land: Captain Richard F. Burton, F.R.G.S.

Entomological Society, at 7.

TUESDAY, November 21.

Zoological Society, at 9.—On the Osteology of the Marsupialia. (Part III.) Modifications of the Skeleton in the species of Phascolomys: Prof. Owen, F.R.S.—Report on Several Collections of Fishes recently obtained for the British Museum: Dr. A. Günther, F.R.S.

Statistical Society, at 7.45.—The President's Opening Address.—Suggestions for the Collection of Local Statistics: J. T. Hammick.

WEDNESDAY, November 22.

Geological Society, at 8.—On some Devonian Fossils from the Witzenberg, S. Africa: Prof. T. Rupert Jones, F.G.S.—On the Geology of Fernando Noronha: Dr. Alex. Rattray.—Note on some Ichthyosaurian Remains from Kimmeridge Bay, Dorset: J. W. Hulke, F.R.S.—Appendix to a Note on a Wealden Vertebra: J. W. Hulke, F.R.S.

Society of Arts, at 8.—On the Present State of the Through Railway Communication to India: Hyde Clarke.

Royal Society of Literature, at 8.30.

THURSDAY, November 25.

Royal Society, at 8.30.

Society of Antiquaries, at 8.30.

London Institution, at 7.30.—Science and Commerce, illustrated by the Raw Materials of our Manufactures. (I.) P. L. Simmonds.

CONTENTS

THURSDAY, NOVEMBER 23, 1871

SCIENCE FOR WOMEN

IN the present condition of the two questions of Science Teaching and of the Higher Education of Women, it may be worth while to regard them for a moment from that point of view in which they coalesce, to inquire, in other words, what is being done for the scientific instruction of women. We do not propose now to argue the question whether it is desirable that women should learn science—that we take to be already decided; but rather to speak of the extent to which, at the present time, provision is being made for carrying out this object. The attention of the public was called to the subject a fortnight ago by the publication of the report of the Syndicate appointed by the University of Cambridge for the examination of women above eighteen years of age in July last. The following are the portions of this report which refer to the various subjects coming within our scope:—

"The answers in the present year in Mathematics show a marked improvement upon those in 1870. The Euclid was decidedly well done, one candidate answering every question except one rider. The conic sections were tried by only two, and without any great success, nothing being attempted in analytical geometry. The algebra was creditably done, but I observe, as I did last year, that while the candidates are fairly skilled in the management of symbols, they seem to have little idea of a logical proof. I should recommend, in this subject, a much more careful study of proofs of rules. The trigonometry, making allowance for the greater intrinsic difficulty of the subject, was better done than the algebra. Statics, astronomy, and dynamics were taken by very few candidates, one of whom, however, showed a knowledge of these subjects small in amount, but thoroughly sound as far as it went. It may be worth while to remark that one candidate, who took in Euclid and algebra only, was the best in each of these subjects.

"In Botany and Zoology the examiner states that the number of candidates was so small as to give little scope for a report. The examination was satisfactory, as far as was possible under the circumstances. One of the candidates passed with distinction. In Geology and Physical Geography the examiner reports as follows:—'No one has done well. The answers are in most cases shallow and full of bad blunders. The examinees seem not to have sufficient acquaintance with the simple laws of physics to make much progress; for instance, it was plain that some did not understand the ordinary laws of evaporation and condensation of vapour, and it seems to me impossible to understand the causes of clouds and rainfall without such preliminary knowledge. There seemed no better foundation laid in geology. More than one confounded Plutonic with Laurentian rocks. No one showed a tolerable acquaintance with the outlines of systematic geology, or any knowledge at all of Palæontology.'"

The report, though in some respects not unsatisfactory, shows how very much still remains to be done before even a fair start can be said to be made in a general training of our women in the elements of Natural and Physical Science. It is therefore with great pleasure that we welcome the attempts, unconnected and imperfect though some of them may be, which are now being made to remedy this defect.

To place the matter on its right footing, it is essential that the work should be undertaken by the very best teachers we have at our command; and in London at least this is being done in a manner that must in time bring forth good fruit. The classes for women conducted last season at South Kensington by Professors Huxley, Guthrie, and Oliver were attended by large and highly appreciative audiences; and the programme for the present season, already announced by Professors Duncan, Guthrie, and Huxley, is no less attractive. The Ladies' Educational Association of London has wisely confined its teaching to that of the professors of University College, thus affording a guarantee that the instruction shall be of a first-class kind; and now that the whole scientific staff of the College has placed its services at the disposal of the Association, and the Council has given permission for the lectures to be delivered within its walls, with full use of its philosophical apparatus, a scientific training is for the first time offered to ladies on a par with that obtained by its male students. We learn that the classes named in the programme have all been started, and with a fair number of entries. That there is great room for instruction of this kind is shown also by the eagerness with which women take advantage of the opportunity of attending mixed classes wherever they are conducted by men of high repute. We need only refer to the success which has attended Prof. Huxley's lectures at the London Institution in Finsbury Circus, especially as regards the position taken by girls at the examinations in previous years, and to the crowded audiences, consisting at least half of ladies, who are now attending his course on Elementary Physiology.

In the provinces the same work is going on, though hardly with the same degree of organisation. The professors of the University of Cambridge in particular have shown a praiseworthy zeal in the cause, and have offered their time and their services for a more general system of instruction than could be comprised within the lectures which have been given during the last two years at Cambridge itself. We referred last week to the attempt now being made at the College for Women at Hitchin—to be removed, whenever sufficient funds can be obtained, to Cambridge—to inaugurate systematic instruction in Chemistry as an introduction to the other sciences, an attempt to which we heartily wish the success it deserves. When the College for Physical Science was founded at Newcastle, the Council took into consideration a request from a number of ladies of the neighbourhood that women should be admitted to its classes, and decided to make no restriction as to sex in the admission of students or in the rules to which they should be subject. Greatly, however, to the disappointment of the Professors themselves, after all this preparation, when the time came not a single lady presented herself as a pupil. We cannot but think that the ladies of Newcastle were ill-advised in urging the subject upon the Council when there was no actual demand among them for the instruction itself, and thereby giving occasion for unjust reflections on the genuineness of the desire among women for instruction in science.

We wish we could refer with the same satisfaction to the present position of the question in Scotland. The ladies of Edinburgh have shown their high appreciation of the opportunity that has been offered them by several of the Professors of the University for the highest intellectual training, and the Ladies' Educational Association of the Scottish capital has been among the most successful in the kingdom. Emboldened probably by the favour with which the cause of female education was received in Edinburgh, several ladies applied to the University for instruction in a purely medical course of studies; and, the required permission having been obtained, pursued with credit and success the earlier portion of their studies. When they had advanced thus far, however, an unexpected obstacle arose, and the highest governing body of the University, the Senate, stepped in and barred all further progress. The mode, indeed, in which the authorities of the University have played fast and loose with the question of the medical education of women redounds little to their credit. It remains to be seen whether the Council will consent, at the bidding of the Senate, to rescind the regulations which they themselves freely passed in 1869, with the sanction of the Senate, viz.:—

"Women shall be admitted to the study of medicine in the University. The instruction of women for the profession of medicine shall be conducted in separate classes, confined entirely to women. The professors of the Faculty of Medicine shall, for this purpose, be permitted to have separate classes for women. All women attending such classes shall be subject to all the regulations now or at any future time in force in the University as to the matriculation of students, their attendance on classes, examination, or otherwise."

Any proposal for mixed classes of both sexes in purely medical subjects excites so great a repugnance both among the teachers and students of medicine that it would be extremely unwise to press it; but it will be observed that no such question has been raised here, and no such request has ever been made by the lady medical students The best of the medical as well as the general press of London has been almost unanimous in pointing out the undignified position in which the Senate now stands; and it is earnestly to be hoped that wiser counsels will prevail, and that the University will in future pursue a course which will give greater satisfaction to all its best friends.

We noticed with pleasure the large and comprehensive views expressed by Lord Lyttelton when presiding last week over a meeting of the National Union for Improving the Education of Women of all Classes. Lord Lyttelton's position as Chairman of the Endowed Schools' Commission rendered peculiarly important the opinion he expressed as to the misappropriation of the enormous educational endowments of the country to the benefit of male students only.

The extreme importance to all women, as great if not greater than to men, of an acquaintance with the elements of human physiology and of the laws which govern the body in health and sickness, was admirably set forth in an introductory lecture by Prof. Bennett to his ladies' class at Edinburgh, a portion of which will be found in our present number. The advantage which the community, no less than individuals, will gain when some knowledge of Natural and Physical Science is spread throughout our female population, is so obvious that we have no fear but that the movement now happily inaugurated will spread and prosper in spite of temporary checks and disappointments.

ALLEN'S MAMMALS OF FLORIDA

On the Mammals and Winter Birds of East Florida: with an Examination of Certain Assumed Specific Characters in Bird Faunæ of Eastern North America. By J. A. Allen, Cambridge, U.S.A. 1871.

THIS essay forms a portion of the second volume of the "Bulletin of the Museum of Comparative Zoology at Harvard College, Cambridge, Mass.," in which work Prof. Agassiz and his disciples are giving to the world the results arrived at from the study of the rich collections accumulated during the past few years under their charge. Its author is almost new to the particular branch of zoology which he now enters upon, and puts forward his views in a very decided and uncompromising manner. Yet he has obviously taken great pains in the investigations which have conduced to his results, and has, it must be allowed, to a certain extent, proved his point, although, as is usual with most reformers, he has in some cases pushed his theories too far.

Mr. Allen's paper embraces, as he tells us in his Introduction, "five more or less distinct parts." The first contains remarks on the topography, climate, and fauna of Florida, based principally upon observations made during a three months' expedition to that country in the winter of 1868-9. The second portion contains an annotated list of the Mammals of Eastern Florida. In this list some unusual identifications are made—*e.g.*, the Common American Fox (*Canis fulvus*, auct.) is identified with *Canis vulpes* of Europe, and the American Black Bear (*Ursus americanus*) is considered inseparable from *Ursus arctos.* In Part III. we have the reasons which have led the author to adopt these and similar views as to certain species in the class of birds hitherto considered to be distinct put forward at considerable length. The examination of the extensive series of the common North American Birds in the Museum of Comparative Zoology "has disclosed a hitherto unsuspected range of purely individual differentiation in every species thus far studied. . . . Local or geographical variations have been likewise carefully considered, with results that were a short time since unsuspected. . . . These several lines of investigation have shown that in many instances what have been regarded as reliable characteristics of species have in not a few cases little or no value, that the importance of many diagnostic features has been too highly estimated, and that consequently a careful revision of our published faunæ will be necessary for the elimination of the merely nominal species." To all this every true naturalist will give his cordial assent. We are all for reform and revision, when founded on sufficient evidence. But on turning to Part IV. of our author's work, it would appear that some of his identifications have been based on mere conjecture without any evidence at all. For example: *Quiscalus brachypterus* of Porto Rica and *Q. crassirostris* of Jamaica are placed as synonyms of *Q. purpureus.* Yet it does not appear, or at all events is not stated, that the author has ever examined authentic specimens of the two former species. Again, *Chordeiles texercis* is united to *C. popetue* without any further remark than that "this widely distributed species presents the usual variations in size and colour." Such and similar errors will, we fear, tend to discredit the identifications which Mr. Allen has discreetly made between certain supposed species, of which he has examined a large series of specimens in a most exhaustive and painstaking manner.

In Part V. of his memoir Mr. Allen treats of the geographical distribution of the birds of North America, "with special reference to the number and circumscription of the ornithological faunæ." In this essay, which well merits perusal, although it is evident that the author has never made himself acquainted with some of the most certainly ascertained facts of the general distribution of bird-life,* a new and arbitrary division of the world's surface into eight "realms" is proposed.

The division of North America, however, into its constituent sub-faunæ is fully discussed and well worked out. An appendix to the volume contains a list of authorities to be consulted on the geographical distribution of North American birds, which will be useful, although by no means well arranged. Mr. Allen's knowledge of the geography of Central America seems, moreover, to be somewhat imperfect, as Mr. Salvin's articles on the birds of Veragua are placed under "Guatemala," and papers relating to British Honduras (*i.e.*, Belize), the Republic of Honduras, and Nicaragua, are all confounded under one head. P. L. S.

* *E.g.* The "Neotropical Region" of Sclater, *i.e.*, South and Central America, is divided between two "realms," an "American Tropical" and a "South American Temperate," than which nothing can be more unnatural, and North America is parcelled out into "three realms!"

OUR BOOK SHELF

Sir Isaac Newton's Principia. Reprinted for Sir W. Thomson, LL.D., and Hugh Blackburn, M.A. (Glasgow: Maclehose.)

FINDING that all editions of the Principia are out of print, the Glasgow Professors of Natural Philosophy and of Mathematics have issued a careful reprint of the last (third) edition as finally revised by Newton himself; attending, of course, to the *Corrigenda*, but wisely abstaining from the insertion of either note or comment. We have had far too much of such things. Think only of the painfully elaborate notes of poor Bishop Horsley, which deface an otherwise splendid edition, and of the truly amazing comments made by Lord Brougham in his "Analytical Views!" True, these are coarse attempts at painting, or rather at "whitewashing," while the Glasgow professors are quite able to "gild." But even gilding would have had a smack of profanation about it, and we are delighted to have Newton left to speak for himself in the old, imperishable, words whose full meaning is only now gradually dawning on the world. So far as we have compared it with other copies, this edition seems to be better than any of its predecessors; the printing and paper are excellent, and the cuts especially are greatly improved. There is, however, one remark which is forcibly thrust upon us by this performance. How eccentric and inscrutable are mathematicians! Comets are nothing to them; and the greater they are, the less do they seem subject to any law of what would be called common sense by mere average humanity. One man of exceptional genius is found wasting day after day in neatly rounding off a sonnet; anon he calculates, to fifty places more than can ever be required, the root of some transcendental equation. Others occasionally burst from their seclusion and rush wildly into gymnastic feats, high-jinks, and what not; but in cold blood to determine to verify, letter by letter, a reprint of a somewhat bulky Latin book seems a species of self-torture, of which nothing we ever before heard concerning our northern friends, could have led us to imagine them capable. They have gone through it, however; and, having done it well, deserve our hearty thanks.

Description of an Electrical Telegraph. By Sir Francis Ronalds, F.R.S. (London: Williams and Norgate.)

SIR FRANCIS RONALDS has done well in republishing this portion of his work, which was first printed in 1823. The hope which he expresses in the preface to this reprint that his name "may remain connected with an invention which has conferred incalculable benefits on mankind," is quite justified by the experiments which he made and published many years before the final success of telegraphy. Sir Francis, before 1823, sent intelligible messages through more than eight miles of wire insulated and suspended in the air. His elementary signal was the divergence of the pith balls of a Canton's electrometer produced by the communication of a statical charge to the wire. He used synchronous rotation of lettered dials at each end of the line, and charged the wire at the sending-end whenever the letter to be indicated passed an opening provided in a cover; the electrometer at the far end then diverged, and thus informed the receiver of the message which letter was designated by the sender. The dials never stopped, and any slight want of synchronism was corrected by moving the cover. Hughes' printing instrument is the fully developed form of this rudimentary instrument. A gas pistol was used to draw attention, just as now a bell is rung. The primary idea of reverse currents is to be found where Sir Francis suggests that the wire when charged with positive electricity should discharge not to earth but into a battery negatively charged. Equally interesting is the discussion on what we now call lateral induction, then known as compensation. The author clearly saw that in the underground wires which he suggests as substitutes for aerial lines, this induction would be or might be a cause of retardation. His own words must here be quoted:—"That objection which has seemed to most of those with whom I have conversed on the subject the least obvious, appears to me the most important, therefore I begin with *it*, viz., the probability that the electrical compensation, which would take place in a wire enclosed in glass tubes of many miles in length (the wire acting, as it were, like the interior coating of a battery) *might* amount to the *retention* of a charge, or, at least, might destroy the *suddenness* of a discharge, or, in other words, it might arrive at such a degree as to retain the charge with more or less force, even although the wire were brought into contact with the earth." This passage, written in 1823, is very remarkable, and would alone entitle the author to be mentioned in any history of underground or submarine telegraphs. Testing-boxes were invented by Sir Francis, and a code is suggested by him. If these things had been mere suggestions they would have been remarkable, but accompanied by practical experiments proving that the scheme could be carried out, they ought to connect his name permanently with the history of the Electric Telegraph. F. 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.*]

Oceanic Circulation

ON returning from my second Mediterranean cruise, I find that Mr. Croll has published in the *Philosophical Magazine* his promised demonstration of the theoretical impossibility of the production of under-currents by gravitation, according to the doctrine which I have advocated with reference to—

1. The Gibraltar Current.
2. The Baltic and Black Sea Currents.
3. The General Oceanic Circulation.

At the same time I find awaiting me a very important treatise on the Physics of the Baltic ("Untersuchungen über Physikalische Verhältnisse des Westlichen Theiles der Ostsee") by Dr. H. A. Meyer, of Kiel, containing the results of a continuous series of most careful and systematic observations on the temperature, specific gravity, and movement of the different strata of its water, dating back to the spring of 1868. With this work I received a letter from its author, of which the following extracts will, I think, be interesting to your readers:—

"I have followed with special attention the splendid results of your different voyages, and hope that the experience which I have gathered on a more confined area may yet offer something which you may deem worth your attention. The favourable opportunity which I enjoyed for continuing regular observations at a spot where the waters of the North Sea mingle with those of the Baltic, enabled me to collect matters which cannot be brought together on sea-voyages only; and I should be much pleased to see similar work undertaken at Gibraltar and Constantinople. If among your large circle of acquaintance you might know of gentlemen who may be interested in this cause, I should be happy to send them my book.

"I regularly read NATURE, and am much surprised to find that your views on Ocean-currents should not be universally accepted. How one can suppose that such a vast force which constantly acts in one direction should remain without any influence whatever, is perfectly incomprehensible to me!

"Most probably the cold under-current coming from the pole will be—wherever it is not very confined—very slow; but I doubt not that, should you consider it of sufficient importance, you will succeed in proving that the current, when confined, is pretty fast, that is to say, fast enough to be measured by the instrument which you used in the Straits of Gibraltar.

"With a similar appliance, which I have used for years, and which you will find figured in my work, I have lately been able to trace the heavier under-current in the Baltic to a much greater distance. On board one of the despatch boats of the German Navy, accompanied by some friends, I have this summer made several trips through the Cattegat and Skager Rack, and into the eastern parts of the Baltic; and my views have been everywhere confirmed."

I have further to state that my prediction that a similar under-current of dense water must pass through the Dardanelles and the Bosphorus from the Ægean into the Black Sea, which, it has been alleged by Captain Spratt, is disproved by experiments made by him several years ago, is regarded by three of the ablest of our Hydrographers to be conclusively proved by those very experiments when rightly interpreted. This I shall shortly demonstrate in an appendix to the forthcoming Report of my recent cruise.

The case between Mr. Croll and myself, therefore, stands thus:—

1. I have experimentally proved the existence of an *outward* under-current in the Straits of Gibraltar, and have adopted the gravitation theory of Captain Maury as affording an adequate account of it.

2. I have shown that this gravitation theory is applicable, *mutatis mutandis*, to the converse cases of the Baltic and Black Sea *inward* under-currents, the existence of which has been experimentally demonstrated.

I have further shown that it is applicable to that general Oceanic Circulation, the evidence of which appears to me to be afforded by the aggregate of observations that indicate the prevalence of a temperature not far above 32° on the deep ocean-bottom, even under the equator, and by the intermediate soundings which indicate the existence of two distinct strata, separated by a "stratum of intermixture," in parts of the deep ocean which the Gulf Stream assuredly does not reach.

These views have been accepted by Physicists of the highest eminence; but, as Mr. Croll affirms, without due consideration of their theoretical difficulties. I venture to suggest, however, that it is not beyond the range of possibility that Mr. Croll's data may be erroneous; and I do so with the more confidence, because I have been assured by first-rate Mathematicians that the science of Hydro-dynamics has not yet attained a development which would justify the assertion, that (to use Dr. Meyer's words) "a vast force constantly acting in one direction remains without any influence whatever."

It happens that I very early became impressed with the power of very small differences in Temperature to produce currents in liquids, by the following remarkable fact, which has never (so far as I am aware) been published. More than thirty years ago Mr. West of Bristol (where I then resided) built an observatory on Clifton Down, the principal instrument of which was intended to be a refracting telescope of large aperture, the object-glass of which was to be made on the plan of Mr. Peter Barlow; the double concave of flint being replaced by sulphuret of carbon, or some other liquid of great dispersive power. The object-glass was constructed with the greatest care, Mr. Barlow kindly assisting in the computation of the requisite curves; but when tried it was found to be practically useless, in consequence of the movement produced in the liquid by the very minute differences of temperature occasioned by air-currents striking the surface of the outer lens.

I would also direct the attention of your readers to the very interesting paper by Prof. Karl Möbius, the coadjutor of Dr. Meyer, "On the Source of the Nourishment of the Animals of the Deep Seas," of which a translation will be found in the "Annals of Natural History" for September. Careful and prolonged observation of the movements of organic particles in aquaria satisfied him that very slight changes of temperature have a very important effect in producing changes in the stratification, so to speak, of the water; in one instance, he says, "a downward current, which readily carried organic bodies along with it, was produced when the difference between the superficial and bottom temperatures had scarcely attained half a degree of Reaumur (1°·1 Fahr.)."

Such being the facts of the case, and Mr. Croll having offered no explanation of them, whilst demonstrating to his own satisfaction that the explanation I advocate is untenable, I do not feel called upon to discuss the subject further. There can be no reasonable doubt that, within the next few years, a great mass of additional data will be collected, which will afford adequate materials for the construction of a definite Physical Theory, by Mathematicians fully competent to the task. At present I do not pretend to have done more than offer a hypothesis which accords with the facts at present known, and with what Sir John Herschel called the "common sense of the matter."

Nov. 14 WILLIAM B. CARPENTER

The Solar Parallax

IF Mr. Proctor had printed in full my memoranda on the errors and imperfections of his history of the solar parallax, or if he had said nothing about it, I should have said nothing more in defence of my review. But, in NATURE of September 28, he gives so inadequate an account of my notes, hiding the point of the most remarkable of his inaccuracies, and ignoring the imperfections entirely, that I am compelled in self-defence to explain. In describing the various discussions of the Transit of Venus which preceded that of Mr. Stone, he says (p. 61): "Newcomb, of America, was more successful. He deduced the value 8″·87 by a method altogether more satisfactory than Powalky's. But still the agreement between the different observations was not so satisfactory as could be wished, nor had Newcomb adopted any fixed rule for interpreting the observations of internal contact, which, as I have said, are affected by the peculiar distortion of Venus's disc at that moment."

To express my appreciation of this compliment it is only necessary to say that I have no recollection of having discussed the past transits of Venus at all, beyond correcting what I supposed to be an oversight in Mr. Stone's paper, and I am still utterly at a loss to know on what ground the compliment is based. In his letter he tries to throw the responsibility upon an anonymous correspondent of the Astronomical Register, which I regret to say does not circulate here, but he does not quote anything to justify a single statement in the preceding paragraph. The correspondent says nothing about 8″·87, which, it will be noted, is Mr. Petrie's pyramid value, nor about my treatment of contacts, so far as quoted by Mr. Proctor, so that I am as much in the dark as ever.

We have all heard suspicions that critics sometimes review books without reading them, but this is the first time I remember to have seen so circumstantial a description of a work which never existed, save in the writer's imagination. I really cannot help viewing it as something "remarkable" when coming from a writer of Mr. Proctor's accuracy and erudition, and must beg pardon if I measure his writings by too high a standard.

The imperfections consist briefly in the regularity with which the more recent and complete researches on the solar parallax are ignored, incorrectly given, or placed in the back-ground of older and less complete ones. If any one wants to satisfy himself of this, he has only to look at the papers and discussions which have appeared in the *Comptes Rendus*, the Monthly Notices, and the German "Vierteljahrsschrift des Astronomischen Gesellschaft" within the past four or five years, and see that only a single one of them all is expressly mentioned, and to note the values of the parallax adopted in the astronomical ephemerides of France, Spain, Portugal, and Germany, and see that not one of them can be traced in Mr. Proctor's history. If, as he once said, he had not room to describe the recent researches, I should have supposed he would have condensed or omitted the older ones, which these recent ones have superseded, instead of doing the contrary. The importance of this matter arises from the fact that these discussions and researches put a different face on a number of questions connected with the determination of the solar parallax from that given by Mr. Proctor, and I do not think the latter can successfully argue that the astronomical world of to-day is nearly all wrong in the views to which it has been led by five years of discussion, experiment, and research.

On Nos. 3, 4, and 7, of Mr. Proctor's defence, it is only needful to remark (1) that I did not write No. 3 till I had verified Foucault's result by a careful calculation not made on my thumbnail; (2) that Mr. Proctor leaves it to be logically inferred that the discussion alluded to in No. 4 was an unpublished one; (3) that, having disclaimed my interpretation of No. 7, his book gives no explanation of the reason why Mr. Stone's parallax was so much greater than those of Encke and Ferrer. It is only necessary to refer to the paper of the latter in vol. v. of the Memoirs of the Royal Astronomical Society at pages 254 and 264 to find a very full discussion of the apparent, and the so-called true contacts.

No. 6 involves one of the most important questions connected with the determination of the solar parallax from transits of Venus, and I am sorry to see that Mr. Proctor simply evades the issue, as the misinterpretation to which he refers consists in supposing him less erroneous than he really is. Let one make a drawing representing the limbs of Venus and the sun in mathematical contact. On each side of the point of tangency there will be an exceedingly thin thread of light, vanishing at that point. How much of this thread will be visible by an ordinary telescope? We must remember that the sun is viewed through a dark glass, which reduces its light to that of an ordinarily illuminated object. The narrowest visible line so illuminated subtends an angle of about 20″. With a power of 120 this would correspond to a breadth of one-sixth of a second. But it is well known that atmospheric tremors, and, with most old instruments, imperfect corrections of the object-glass, prevent our seeing an object at all approaching the minimum visible, and that the same cause prevents the increase of magnifying power from giving a corresponding increase of seeing power. It is probable that the thickness of the least visible thread may have varied with the telescope, the observer, the dark glass, and the atmosphere, from one or two tenths of a second to one or even two seconds. Let us take the more favourable cases in which a thread of 0″·2 is visible. A simple calculation will show that there is a space of 3″·4 on each side of the point of tangency, in which the thread will be thinner than this, and therefore invisible, and that the visible cusps will be about 7″ apart. How different this 7″ from Mr. Proctor's invisibly thin ligament! This explains the observations of Wolf and André, who found that the black drop when seen at all continued after internal contact at ingress and preceded it at egress.

In answer to Mr. Proctor's letter of October 5, I beg leave to reply, if the "fringe" is something actually produced by the telescope or the atmosphere, it is simply bad definition. If it is not so produced, it is an optical illusion, of which the laws are obscure, and the very existence problematical under the circumstances in question. See, for instance, the celebrated paper of Prof. Baden Powell on Irradiation. Mr. Proctor's intimation that the great mass of astronomers who have observed transits of Mercury within the past forty years, among whom are included Bessel, Airy, and the Struves, were careless and inferior observers, because they did not see an optical illusion according to his view of it, is as good a *reductio ad absurdum* of his theory as I could ask for. It is comforting to know that one of his careless observers can be turned into a careful and attentive one by giving him a telescope with plenty of irradiating power.

To prevent misapprehension, allow me to say that the theory set forth in my letter of September 28 is in no way my own, but was promulgated by Bessel nearly forty years ago, and has, I believe, been since universally received on the continent of Europe.

SIMON NEWCOMB

Washington, Oct. 23

The Aurora of Nov. 9th and 10th

I WITNESSED on the night of Nov. 9, at about 7.30 P.M., an aurora which, for symmetry of form and other features, was very remarkable; and unless, as is very likely, some more able observer has already sent you a description of it, you may like to put my account on record.

In the magnetic north horizon was the usual segment of auroral light, very brilliant, and stretching considerably to the east and west, its altitude being 20° or more. High above this, and extending in a complete arch from the east to the west horizon, was a remarkable and well-defined band of still brighter light, about 7° in breadth, and passing about 30° from the zenith.

Filling the space between these two arcs of light was what I can call nothing else than a dark shadow, which had somewhat of a mysterious appearance; for, though decidedly darker to the eye than other parts of the heavens, it did not in the least obscure even small stars, nor do I think this darkness was the effect of contrast. In this dark space faint auroral streamers occasionally shot up to the upper arch, but did not pass it. This shadow was what the French observers speak of as the *nuée*.

The light of the upper arched band was silvery, and increased much in intensity towards the horizon both east and west; the points of greatest intensity being about 5° above the horizon, as would be expected in the direction in which the arch appeared most foreshortened.

While watching this phenomenon I was impressed by the conviction that, to an observer in space, the north magnetic pole of our planet would have presented the appearance of being surmounted by a symmetrical cap of light, streaked by one or more bands, and terminated at its greatest distance from the pole by a well-defined brilliant margin.

In the hope that an observer in some other locality might have made similar observations, I was preparing to measure the distance of the upper arch of light from the zenith, as well as the positions in azimuth of the points where it touched the horizon, when the whole phenomenon was obscured by dense clouds.

Stretton Rectory, Hereford, Nov. 15 H. C. KEY

THE following brief extract from our observatory note book may be interesting:—

"Nov. 10.—For about 20° on each side of north, at 9.30—9.40 P.M., brilliant *waves* of light followed one another rapidly, from two to four in a second, moving upwards, following the direction of the streamers, fading away at about 40° from the horizon. Three or four waves could be seen at once, measuring about 5° to 8° by estimation, from crest to crest. . . ."

I heard some of the boys remark "How close it must be; it looks like puffs of steam from behind those houses."

Rugby J. M. W.

As none of your correspondents who described the brilliant auroræ of Nov. 9th and 10th last week, speak of their being seen earlier than from 7 to 10 P.M., it may be interesting to note that in the Midland Counties the latter was visible at a considerably earlier period of the evening. On the evening of the 10th I was walking from Reading in Berkshire to Caversham in Oxfordshire, from 5.45 to 6.5 P.M. During the whole of that time I had before me the steady *white* light of the coming aurora, extending perhaps 25° to 30° in width, and 20° in height, its centre being immediately beneath Polaris. The appearance was exactly that of the departing twilight in a clear winter sky, for which, indeed, but for its position and the time of the evening, it would have been mistaken. As I noticed the light immediately on leaving the railway station above the lights of the town, I have little doubt that it had been visible since sunset. I had no opportunity of watching its progress after 6.5 P.M.; up to that time there were no coloured streamers, nothing but the white light I have described.

ALFRED W. BENNETT

The Ghost of Flamstead

I OUGHT earlier to have thanked this venerated shade for a communication which will enable me to correct (at some future time) an omission in my treatise on the Sun. Let me hasten to assure him (or it), however, that the omission has been in no way connected with those "queer notions of honour, and justice, and fairness," which he conceives to be rife in our times. Why should I seek to wrong the honoured dead? And who would gain in this case by the injustice? The present Astronomer Royal? Surely no. To add this small matter to his real claims to our esteem would be

To gild refined gold, to paint the lily,
And throw a perfume on the violet.

Neither, I am sure, has any other writer who has overlooked Flamstead's claims, desired to do him injustice. On this point I would merely remark, "Rest, rest, perturbed spirit."

But now "we'll shift our ground," by the Ghost's good leave.

Our visitor from Valhalla remarks that "a stir was lately 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 our grandfathers)." Here the spirit of Flamstead refers obviously to the Astronomer Royal's method. I am sure that Prof. Airy would desire greatly that if his method be indeed so ancient, the fact should be made widely known. I myself am particularly anxious to be set right on this point, about which I am at this very time writing. For though I care more about explaining this and the other methods than about their history, yet it is desirable to be accurate even in historical details.

If I may say so without offence, I would remark that a ghost was not needed—certainly not the ghost of the first Astronomer Royal—to teach astronomers that the opposition of Mars in 1877 will be exceptionally important. At page 25 of my "Sun" I have already pointed this out, and I dare say others have done likewise.

I hope the "great injustice" to which our ghostly correspondent refers as endured by him in life, does not relate to his difficulties with Newton, for at the present time the opinion of Brewster on this point is in vogue—not Baily's; and the warmest admirers of Flamstead are those who least desire to moot the subject. R. A. PROCTOR

Brighton, Nov. 4

Creators of Science

PERMIT me to do my little towards clearing up a most unfortunate confusion of thought respecting the intellectual ranks of mathematicians and metaphysicians, which is, in my experience, widely prevalent. We may safely divide the mathematicians into three orders:—(1) Inventors, (2) Experts, (3) Readers or Students, so as to discriminate from one another those who create systems, those who manipulate with them, as "ministers and interpreters of nature"—just as easily and familiarly as Professor Tait (*e.g.*) employs and applies the theory of Quaternions—and those who have merely studied into an understanding of an author or subject. It was an expedient of the late Sir William Stirling Hamilton to confound all these orders, and from the heterogeneous lump to extract—if not extort—testimonies to the worthlessness of mathematics as a mental discipline, without the least discrimination of their sources.

On the other hand, the metaphysicians cannot be trichotomised; for, even in the present advanced state of metaphysics, there is no class of philosophers corresponding to the mathematical experts, the reason of which explains why examiners in mental science do not set problems. There are, in fact, only two classes of metaphysicians: I., Creators; II., Students, more or less thoroughly versed in the systems of the leaders, and more or less accepting or rejecting, with more or less reason, those creations. Accordingly, when on May 17, 1869 (I *think* that was the date), Professor Tait, at a meeting of the Royal Society of Edinburgh, challenged the metaphysical world to produce a metaphysician who was also a mathematician, he not being able at the moment to call to mind a single instance, he was to be understood as asking for a person of the order 1, who was also in the class I. Professor Calderwood's reply, then, was not wholly unexceptionable, for of the three names he adduced, viz., Descartes, Leibnitz, and Hegel, the last was that of a reader of mathematics, and not of a mathematical inventor. The challenger might have spared the respondent the trouble of reply, had he known what De Morgan wrote in Notes and Queries, 2nd S. vi. 293–4, where are distinguished five mathematical inventors, as *facile principes*: viz., Archimedes, Galileo, Descartes, Leibnitz, and Newton; and in which Aristotle, Plato, and D'Alembert are allowed a very high rank in mathematics. Had the inventor of Quaternions been then dead, I have little doubt that De Morgan would have added to the five the name of Sir William Rowan Hamilton, who, besides being a mathematical inventor of the very first rank, was also a diligent and accomplished student of Plato, Kant, Reid, and the other Hamilton, and a writer on Logic; *i.e.*, as good as D'Alembert as a philosopher, and perhaps better than he as a mathematician. Now, it is not a little curious and very instructive to observe that, *pace Platonis*, the two who were creators of strictly defined metaphysical systems, viz., Descartes and Leibnitz, are the only two among the five metaphysicians adduced by De Morgan who belong to the highest rank as mathematical inventors.

It is quite incredible that a man of Professor Tait's learning (I say here nothing of his judgment) should not have been aware of the identity of Descartes (the poor dreamer!) and Cartes, the founder of the Cartesian Geometry; still more so that he should not have known that the immortal analyst, the co-inventor of the Differential Calculus, was the most eminent metaphysician native to Germany before Kant. It was, then, not "ignorance," but "ignoration," on the part of the Scotch mathematician, that was involved in his challenge; and that challenge was doubtless intended as mere *badinage*, at the expense of a science which he had taken no pains to understand.

Be that as it may, I trust I am not singular in adjudging (as De Morgan did) these two grand intellectual pursuits to be worthy of being cultivated together, and to be able to give material aid to each other. For myself, I cannot but look upon any man as the enemy of intellectual progress, who delights in setting the one class of investigators against the other, and endeavours to prolong the controversy which has raged between them since the "Principia" was promulgated.

Highgate, Nov. 8 C. M. INGLEBY

Descartes' "Animated Machines"

As you open your valuable columns to philosophical discussions, may I request you to publish the following remarks on a passage in Mr. Lewes's popular "History of Philosophy" (Vol. ii. p. 148 of the new edition), where he confesses himself puzzled, along with other critics, to account for Descartes' theory that animals were only *animated machines*. "I am not prepared," he says, "with a satisfactory explanation." I cannot but think that a careful perusal of the "Discourse on Method" (Part 5, *sub. fin.*) and of the treatise on *les Passions de l'âme*, makes Descartes' reasons perfectly clear. In the first place, the use of the word *machine* has misled most of his critics, and if the story of Malebranche and his dog be true, even this great disciple had grievously mistaken the principles of his master. For in the last-named treatise Descartes endeavours to show that such feelings as joy, grief, fear, &c., though in us accompanied by really mental acts (*pensées*), are produced by physical causes, and produce physical effects apart from the mind. Descartes would therefore never have denied to brutes any of the bodily sensibilities which we possess; and says expressly that he calls them machines in a special sense—machines made by the Deity, and therefore infinitely more subtle and perfect than any which we can construct. He says that we could not ourselves be ranked higher in the scale of beings did we not possess the gift of *language*, the phenomena of which can only be accounted for by an internal principle different in kind from those which appear to guide the lower animals, though there are also those passions in us which we have in common with them.

But to come to the psychological reasons for the theory. Historians of philosophy before the 18th century should be particularly alive to theological *idola*, even in sceptical writers; much more so in good Catholics like Descartes. Just as Berkeley put forward prominently the theological advantages of his Idealism, so Descartes indicates plainly in his "Discourse on Method" (*loc. cit.*) that these were the chief reasons of his theory. "Next to the error of those who deny the Deity, which I have already refuted, there is none more apt to seduce feeble minds *from the path of virtue* than to imagine that the soul of beasts is the same as ours." But the *locus classicus* has, I think, escaped Mr Lewis, and will be found in a letter to a Lord (supposed to be the Duke of Newcastle), the 54th of the 1st volume in the original quarto edition. Descartes there specially answers objections made to him on this point, and in the way above indicated; adding however the following passage: "Yet it may be said that although the beasts perform no action which convinces us that they think, nevertheless, as the organs of their bodies do not differ much from ours, it may be conjectured that some sort of thought is joined to these organs, such as we experience in ourselves, but much less perfect;

to which I have no reply to make, except that if they thought as we do, they must have an immortal soul as we have, which is not likely, as we should apply the argument to *all* animals, such as sponges, oysters," &c. I am sure these ideas are not unfrequently repeated in his correspondence, as for example, in one of his replies to Morus (vol. i. No. 67 of the 4to edition, in Cousin's Edition, x. p. 204 *et seq.*). He there even talks of two souls, an *âme corporelle* which is the cause of passions and affections, and an incorporeal principle of thought, which he elsewhere says was infused by the Deity into man at the first moment of his existence. He also observes, I think logically enough, that as no boundary line can be drawn elsewhere, we have no choice between conceding a soul to oysters or refusing it to all animals save man. I am not however concerned to defend the validity of his reasons, but rather to contribute this information as an historical point of interest.

Trin. Coll., Dublin, Nov. 11 J. P. MAHAFFY

Plane-Direction

I THINK "plane-direction" is the best of the competing names. The planes of cleavage in a crystal are the "plane-directions" in which it is most easily split. They cannot be called either "aspects" or "positions." The opposite faces of a cube certainly cannot be said to have the same "aspect."

If a rigid body receives a movement of translation, it retains something unchanged. What is this something to be called? It might be called "lie" or "set," but both names are equivocal. Two equal and similar figures possessing this something in common might be very well described as "similarly laid," "similarly set," or "similarly placed." We may say that they have "similar positions," but we can scarcely say that they have "the same position;" for change of position is commonly held to include movements of translation as well as of rotation, and a point is usually defined as having position but not magnitude. I think it is worth while to consider whether "position" cannot be restricted to the more limited sense, "place" being employed in the wider sense.

I wonder that no one has yet raised a murmur against the proposition itself, which your correspondents are so anxious to render literally into English. It appears to me that the plain English form in which Mr. Wilson first stated it is clearer and more precise than the German abridgement. In the strictest sense of "determine," one "Richtung" determines one "Stellung" and one "Stellung" determines one "Richtung," inasmuch as to one plane-direction there corresponds one normal direction.

In a special sense it is true that two "Richtungs" determine a third (perpendicular to them both), and that two "Stellungs" determine a third (also perpendicular to both); just as two points may be said to determine one plane (bisecting their joining line at right angles). In all these instances the fact is that not one only but many are "determined," but all except one come out in pairs or multiples of two. It is this one, which has no fellow, that is in a special sense "determined."

I think it is paradoxical and misleading to state, without qualifying words, that two linean directions determine one plane-direction; inasmuch as two linean directions really serve to define as many different pairs or multiple pairs of plane-directions as we please, and if we are permitted to distinguish the two linean directions by dtfferent names, three plane directions can be separately defined by them without any ambiguity. Similar remarks, of course, apply to the other half of the proposition.

J. D. EVERETT

Rushmere, Malone Road, Belfast, Nov. 11

"Wormell's Mechanics"

WILL you do me the favour of inserting a brief reply to the few remarks made concerning the above text-book in last week's NATURE?

1. On page 8 of the book occurs an explanation of what is usually termed the transmissibility of force, and a statement of the axiomatic principle that we may imagine a force to be applied at any point in the line of its direction, *provided this point be rigidly connected with the first point of application*. On page 14 a deduction from this principle is made and employed to prove the rule for finding the directions of the resultant of two forces acting on a point. The reviewer says that this deduction, "if true, would assert that the attraction of the sun and the earth upon the moon might be transferred to any heavenly body in space which happened to be in the line of direction of the resultant of the forces." If the restriction laid down with emphasis in the book, and printed in italics as quoted above, be not ignored, this is a legitimate inference, and if the point to which the forces are transferred parallel to themselves be rigidly connected with the moon, any conclusion having reference to the magnitude or direction of the resultant action on the moon derived as a consequence of the imaginary transposition of the point of application of the forces will be correct.

2. In finding the direction of the resultant of two parallel forces, the same transposition of the point of application is employed, and, of course, it is understood with the same proviso. This proof your reviewer qualifies as "meaningless," whereas I feel sure that, taken in connection with the original axiom and the deduction above referred to, it would be accepted by any mathematician as both intelligible and correct.

3. The next statement is that the definition of a rigid body is given as a property of forces. This is not so, but the whole theory of statics, when developed independently of dynamics, rests on the properties of a force and the properties of a rigid body jointly.

4. The reviewer next dwells upon a curious error which unfortunately escaped my notice until it was pointed out but a short time ago by a schoolboy, and which forms one of three corrections on a slip of errata. Any student would, however, have been able to make the correction for himself by the help of the preceding pages and the applications to the following exercises, a circumstance which I think an unprejudiced critic should not have overlooked.

5. Your reviewer next remarks that a student who tries an experiment with a block and tackle would naturally be surprised at finding that the result of experiment does not agree with that of the theory, and adds, "nor can we find a single word in the book which would enlighten his difficulty." The reviewer cannot have read section 71.

6. The subjects included in the book are such as comprise the course described in the curriculum and examination papers of the University of London, and if occasionally the discussion of unpractical arrangements of mechanical powers is required, I am not answerable. Indeed, I hope to see the day when a reform of this part of the curriculum will necessitate my rewriting the work on an entirely different plan, namely, one according to which kinematics forms the first part, kinetics the second, and statics the third, the propositions of the third part being special cases of those of the second. But that at present it answers the purpose for which it is intended, is proved by the fact that all the questions set this year can be answered from it.

So far as most of the facts and illustrations are concerned, "I am but a gatherer and disposer of other men's stuff," and a writer of an elementary text-book to suit the requirements of a particular examination could not easily be more.

The tone of depreciation with which the writer of the article has been pleased to refer to the work, so directly opposed to a previous notice of the same book in the same journal, seemed to me to call for some reply, and I should wish to describe more fully the objects I have aimed at in compiling the work, but that I know I have already taken up enough of your valuable space.

RICHARD WORMELL

ONE OF THE GREATEST DIFFICULTIES OF THE DARWINIAN THEORY

SIR JOHN LUBBOCK has done good service to science in directing attention to the metamorphoses of insects, by admitting freely the great difficulty in conceiving "by what natural process an insect with a suctorial mouth, like that of a gnat or butterfly, could be developed from a powerful mandibulate type like the Orthoptera, or even from that of the Neuroptera" (NATURE for Nov. 9, page 28). Such "difficulties" have struck many from the first, and it is in no small degree encouraging to those who love the liberty of science, to find that the time is approaching when difficulties may be brought under consideration and discussion.

"There are," Sir John Lubbock remarks, "peculiar difficulties in those cases in which, as among the Lepidoptera, the same species is mandibulate as a larva, and suctorial as an imago." The power of mastication during the first period of life being an advantage, on account of a certain kind of food being abundant, and that of suction during the second, when another kind of food prevailed, or *vice versâ*, is suggested as a possible explanation of the origin of species which are mandibulate during one period of life and not during another. In such cases it is said we have "two forces acting successively on each individual, and tending to modify the organisation of the mouth in different directions." It is suggested that the change from one condition to the other would take place "contemporaneously" with a change of skin. Then it is urged that even when there is no change of form, the softness of the organs precludes the insect from feeding for a time, and when any considerable change was involved, "this period of fasting, it is remarked, would be prolonged, and would lead to the existence of a third condition, that of pupa, intermediate between the other two."

There is much that is assumed in this reasoning; but I shall now venture to call the attention of naturalists to one point only, namely, the analogy between the period of fasting caused by the temporary softness of the organs while the caterpillar changes its skin, and the more prolonged fasting period when the organs undergo that more considerable (!) modification involved in the change from the mandibulate to the suctorial type of mouth. The change from a small mandibular apparatus to a larger one seems to be compared with the change from a mandibular to a suctorial apparatus—the change of skin of the caterpillar with the change of skin when the caterpillar becomes the pupa, and the latter the imago—the temporary softness which prevails when the little mandibles grow into bigger mandibles, with the temporary softness which prevails while the mandibles become converted (!) into the suctorial mouth. But these changes are surely of different orders, and the operations of a different nature. The mandibles do not change. The one type of mouth does not pass through gradations of any kind into the other kind of mouth. But one abruptly ceases, its work having been discharged, while the other is developed anew. As compared with the change of skin of the caterpillar, the change of skin from chrysalis to butterfly is indeed a "*considerable change*." It would require an amazing intelligence to premise from the study of a caterpillar that from it, after certain changes of skin and periods of rest, would emanate a butterfly.

It is very well to suggest that "in reality the necessity for rest is much more intimately connected with the change in the constitution of the mouth"; but what, I would ask, is the evidence of the connection implied? Between the *change* from the small mandibles to the large, and the *change* from the latter to the suctorial apparatus, there can be no comparison—no analogy, for the suctorial mouth is developed anew during the pupa state, and its formation is not commenced until all traces of the mandibles are gone. Nay, every tissue of the caterpillar disappears before the development of the new tissues of the imago is commenced. The muscular and nervous systems of the latter are as different from those of the former as are the digestive apparatus, the oral mechanism, and the external covering. These organs do not change from one into the other; but one, having performed its work, dies, and is removed entirely. Not a vestige of it remains. Its place is occupied by formless living matter, like that of which the embryo in its early stages of development is composed; and from this *formless matter* are developed all the new organs so marvellously unlike those that preceded them; and others unrepresented at all in the larval stage, make their appearance. To explain, according to Mr. Darwin's theory, the "period of change and quiescence" intermediate between the caterpillar and imago states of existence, is likely to remain for some time a very difficult task. If the difficulty cannot be resolved until the period of quiescence during which the imago is formed, is proved to be analogous to the periods of quiescence during the change of skin of the larva, the life history of a butterfly will remain for a long time a puzzle to Mr. Darwin and those who believe in the universal application of his views.

LIONEL S. BEALE

ON THE RECURRENCE OF GLACIAL PHENOMENA DURING GREAT CONTINENTAL EPOCHS

IN the August number of the Geological Society of London I published two papers "On the Physical Relations of the New Red Marl, Rhætic Beds, and Lower Lias," and "On the Red Rocks of England of older date than the Trias." In the latter I attempted to prove that for the north of Europe and some other parts of the world, a great Continental epoch prevailed between the close of the upper Silurian times and the end of the Trias or commencement of the deposition of the Rhætic beds; in other words, that the Old Red sandstone, Carboniferous strata, Permian beds, and New Red series were chiefly formed under terrestrial conditions, all, with the exception of the Carboniferous series, in great lakes and inland seas, salt or fresh.

The Permian strata, in particular, appear to have been deposited under conditions to which the salt lakes in the great area of inland drainage of Central Asia afford the nearest modern parallel.

While brooding over the whole of this subject for several years past, I have often been led to consider its bearing on those recurrent phenomena of glacial epochs which now begin to be received by many geologists.

The phenomena of moraine-matter, scratched stones, and erratic boulders, whether deposited on land by the agency of glaciers, or in the sea and lakes by help of floating ice, are evidently intimately connected with the contemporary occurrence of large areas of land, much of which may, or probably must, have been mountainous.

The late Mr. Cumming, in his History of the Isle of Man, "hints at the glacial origin of certain Old Red conglomerates in that island, conceiving that the bony external skeletons of some of the fish of the period may have been provided to enable them to battle with floating ice. In lectures and in print I have frequently stated my belief that the brecciated subangular conglomerates and boulder beds of the Old Red sandstone of Scotland and the north of England are of glacial origin, so distinct, indeed, that when these masses and our recent boulder clay come together, there is often actual difficulty in drawing a line of demarcation between them. I frequently felt this difficulty years ago, when, commencing the Geological Survey of Scotland, I mapped the strata in the country south of Dunbar, and the same difficulty was occasionally felt by others in the valley of the Lune, near Kirkby Lonsdale.

If, as I believe, the Old Red sandstone was deposited in inland Continental waters, the Grampians, as a mountain tract, bordered these waters, and they must have been much higher then than now; not only because of the probably greater elevation of the whole continent, but also because the Grampians formed land during the whole of the Upper Silurian epoch, and suffered great waste by denudation, then and ever since. The glaciers of these mountains marked an episode in Old Red sandstone times, and yielded much of the material of the boulder beds of the Old Red sandstone.

In these regions and in North America, the Carboniferous strata were evidently formed under the influence of "a warm, equable and moist climate," and I know of no glacial phenomena in connection with this epoch

But respecting Permian times I attempted in 1855 to prove the existence of ice-borne boulder beds during part of that epoch, and by degrees this opinion has been more or less adopted. These boulder beds were derived by glacial transport from the mountains of Wales, which then, also, were necessarily much higher than now. As the Old Red boulder beds were formed during a glacial episode or episodes of parts of that epoch, so the Permian boulders mark another glacial episode occupying part of Permian time, just as our last great glacial epoch formed an episode in those late Tertiary times of which the present time forms a part. At the time of the publication of this paper, I conceived the Permian boulders to have been deposited in the sea by the agency of icebergs, but I now consider them to have been deposited in inland lakes.

This, if true, formed a second glacial epoch, of unknown intensity, during the long continental period that lay between the close of Upper Silurian and the beginning of Liassic times.

During the Triassic period there is no certain sign of glacial phenomena in the British area.

I have elsewhere attempted to show that at the present day there is an intimate connection between past glacial phenomena and the occurrence of lakes, large and small, many of which are true rock-bound basins.

I further believe that this cause would be found to characterise ancient Continental recurrent glacial epochs through all past time, if perfect data were accessible, or had been preserved from destruction by denudation and disturbance of strata. In the Palæozoic cases mentioned above, there is, in my opinion, an evident connection of some kind between inland lakes and glacial action, and in stating this it must be borne in mind that I do not consider the Old Red and Permian strata of Britain to have been deposited solely in two lake basins during two epochs, but in various basins during each of two special eras of geological time. For example, the Magnesian Limestone beds of Yorkshire and Northumberland were formed in a hollow quite distinct from the great conglomerates (locally called "brockram") and sandstones of the Vale of Eden. Prof. Harkness in 1856* showed that in the South of Scotland Permian beds, partly formed of brecciated conglomerates, lie in rocky hollows entirely surrounded by lips of Silurian and Carboniferous strata, in fact, in rock basins; and he attributed this singular circumstance to a sinking in of the Silurian strata in each case underneath the Permian rocks.

Ever since the publication of my paper, in 1862, on the Glacial origin of certain lakes in rock basins, I have suspected that these Permian rock basins may also have been scooped out by the agency of glacier ice. I connect this view with my paper on Permian glaciers, published in the *Geological Journal* in 1855, but as I have not yet seen the country where these hollows lie, I have not been able either to verify or disprove this supposition. I expect, however, that some day this view will be proved, not for these areas alone, but for others of larger area and very different date, which as yet I have only partially examined, in other European countries.

The unravelling of nearly all stratigraphical phenomena of every geological age resolves itself simply into attempts to realise ancient physical geographies, and we may rest assured that those forces that are now in action have played their part in the world sometimes with greater, sometimes with less intensity, through all known geological time, as far as it can be studied by an examination of the rocks that form the crust of the earth. If glacier ice scooped out many lake rock-basins in the latest great glacial epoch, it did the same during glacial epochs of earlier date.

A. C. RAMSAY

* Geol. Jour., vol. xii. p. 254.

WOOD'S "INSECTS AT HOME"*

THIS bulky volume of 670 pages appears to us to be altogether a mistake. It is far too voluminous and too uninteresting for a beginner, while for the more advanced student it is almost valueless, being a very incomplete compilation from the works of well-known writers. It consists of brief and imperfect descriptions of a selection of, perhaps, one-twentieth of the insects inhabiting Great Britain, with occasional notices of their habits and economy, and extracts from a few entomological works. These descriptions are generally introduced by such words as "Our next example," "We next come to," "We now come to," "Next in order comes," "Next on our list is," &c. &c.; and for the most part are mere amplifications of short technical characters, conveying a minimum of useful information, with a maximum expenditure of words. Let us take two examples at random. At p. 76 we have two-thirds of a page devoted to a beetle:—

"Our first example of the Staphylinidæ is one of the finest, in my opinion the very finest, of that family. It is called scientifically *Creophilus maxillosus*, but has, unfortunately, no popular name, probably because it is confounded in the popular mind with the common black species, which will be presently described. Its name is more appropriate and expressive than is generally the case with insect names. The word *Creophilus* is of Greek origin, and signifies 'flesh-lover,' while the specific title, *maxillosus*, signifies 'large-jawed.' Both names show that those who affixed them to the insect were thoroughly acquainted with its character and form, for the Beetle is a most voracious carrion eater, and has jaws of enormous size in proportion to its body. The colour of this beetle is shining black, but it is mottled with short grey down.

"In some places this Beetle is tolerably plentiful, but in others it is seldom if ever seen. It can generally be captured in the bodies of moles that have been suspended by the professional mole-catchers, and, indeed, these unfortunate moles are absolute treasure-houses for the coleopterist, as we shall see when we come to the next group of Beetles. A figure of this insect is given on woodcut No. viii. Fig. 3. It is the only British insect of its genus which is distinguished by having short and thickened antennæ, smooth head and thorax, and the latter rounded."

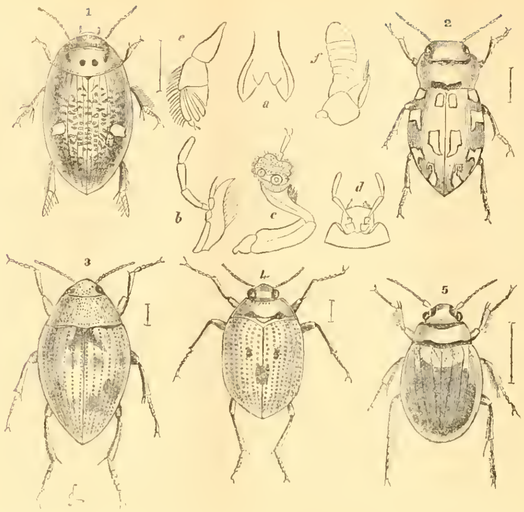
The descriptive portion of this characteristic passage could be easily compressed into two or three lines. In the other twenty we are told that the original describers of the insect were well acquainted with it, that the public are not, and that moles caught by professional mole-catchers are unfortunate!

Turning to page 447, we have a moth described as follows:—

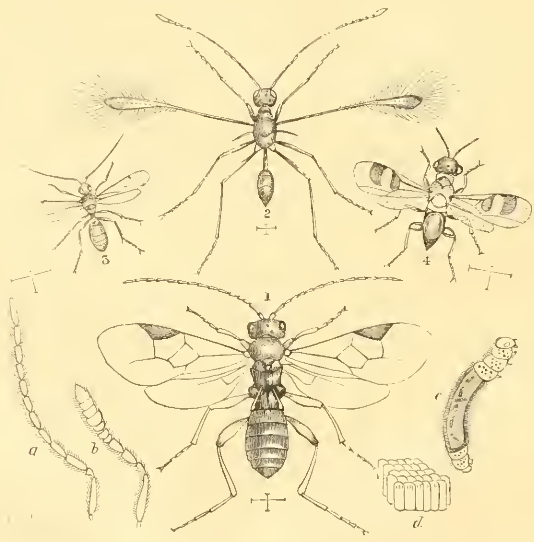
"The first family of the Geometræ is called Urapterydæ, or Wing-tailed Moths, because in them the hinder wings are drawn out into long projections, popularly called 'tails.' In England we have but one insect belonging to this family, the beautiful, though pale-coloured, swallow-tailed moth (*Urapteryx sambucata*). The generic name is spelt in various ways, some writers wishing exactly to represent the Greek letters of which it is composed, and others following the conventional form which is generally in use. If the precisians are to be followed, the word ought to be spelt Ourapteryx.

"There is no difficulty in recognising the moth, the colour and shape being so decided. Both pairs of wings are delicate yellow, and the upper pair are crossed by two narrow brown stripes, which run from the upper to the lower margin. These stripes are very clear and well defined, but besides these are a vast number of very tiny

* "Insects at Home: Being a Popular Account of British Insects, their Structures, Habits, and Transformations." By the Rev. J. G. Wood, M.A., F.L.S., &c. With upwards of 700 Figures by E. A. Smith and J. B. Zwecker. Engraved by G. Pearson. (Longmans, Green, and Co. 1872.)



1. Agabus biguttatus. 2 Hydrophilus duodecim-pustulatus. 3. Haliplus variegatus. 4. Cnemidotus cæsus. 5. Pelobius Hermanni. *a*. Dyticus, process of metasternum. *b*. Dyticus, maxillary palpus. *c*. Dyticus, anterior leg of male. *d*. Dyticus, labial palpi. *e*. Gyrinus, posterior. leg *f* Gyrinus, antenna.



1. Microgaster glomeratus. 2. Mymar pulchellus. 3. Teleas elatior. 4 Cleonymus maculipennis. *a*. Teleas, antenna, female. *b*. Do., antenna, male. *c*. Microgaster, larva in caterpillar of cabbage-butterfly. *d*. Microgaster alvearius, cocoons.

DRAGON-FLIES, MAY-FLIES, AND CADDIS

1. Ephemera vulgata. 2. Ephemera, larva. 3. Libellula depressa. 3a. Libellula emerging from pupa-case. 4. Libellula, larva. 5. Calopteryx virgo. 6. Agrion minium. 7. Phryganea grandis. 8. Phryganea, larva cases, or Caddis.
PLANTS:—Flowering Rush (*Butomus umbellatus*). In Centre. Mare's-tail (*Hippuris vulgaris*). On Right. Water Bistort (*Polygonum amphibium*). On Left.

streaks of a similar colour, which look as if they had been drawn in water-colours with the very finest of brushes, and then damped so as to blur their edges. The hind wings have only one streak, which runs obliquely towards the anal angles, and, when the wings are spread, looks as if it were a continuation of the first stripe on the upper wings. The shape of the moth almost exactly resembles that of the Brimstone Butterfly, described on page 393.

"The larva affords an admirable example of the twig-resembling caterpillars. It is exceedingly variable in colour, but is always some shade of brown. It has seven bud-like humps, and a few pale stripes along the sides. I is a very general feeder, and may be found on a considerable number of trees and plants. It is quite common, and but for its curious form, would certainly be found much more frequently than is the case. The perfect insect appears about July, and can be beaten out of bushes and hedges. Though the wings are large, they are thin and not very powerful, so that there is no difficulty in capturing the insect."

Of course much of the book consists of more interesting matter than this, but hundreds of pages are filled with such verbose and meagre passages as those quoted, which are far more repulsive to the learner than the most condensed and technical description. Those given in Stainton's Manual, for instance, contain more than double the actual information in about one fourth of the space.

The book is illustrated by copious woodcuts in the letterpress and by several whole-page pictures. The former are most admirable, and do great credit to the artist, Mr. E. A. Smith. We select a group of Water Beetles (Cut vi.), and one of the minute and curious parasitic Hymenoptera (Cut xxxii.) as examples of these excellent figures, which would do credit to a far more scientific work. The whole-page illustrations are by another hand, and are in every respect inferior. Some of them contain fair representations of insects in their haunts, but the vegetation is generally badly drawn, and the plants said to be figured are often quite unrecognisable. The best and most artistic picture is Plate VIII., representing a group of Neuroptera with aquatic vegetation. The worst is Plate XIX., representing aquatic Heteroptera. The insects are pretty well drawn, but the plants are dreadful. One of them is said to be the Starwort (*Aster tripolium*). What is meant for this stands prominently out in the view; but the artist has evidently never seen the plant, and, trusting to his imagination to invent something suited to the name, has perched three thick six-rayed starfish on bending stalks. We venture to assert that no plant having the faintest resemblance to this monstrosity forms part of the British flora, and its introduction into a modern work on natural history is most discreditable. It is painful to have to speak in these terms of the work of an author who has done so much to popularise natural history as Mr. Wood, but we must protest against mere book-making; and in this case there could be no pretence of a want to be supplied, since the excellent series of "Introductions" published by Messrs. Reeve and the more general works of Prof. Duncan, Dr. Packard, and others, are far better guides to the student or to the general reader than such a hasty and imperfect compilation as the present volume. A. R. W.

NOTES

THE Council of the Royal Society have awarded the medals in their gift for the present year as follows:— The Copley Medal, to Julius Robert Mayer, of Heilbronn; the Royal Medals to Mr. George Busk, F.R.S., and Dr. John Stenhouse, F.R.S.

PROF. ARCHIBALD GEIKIE is desirous of addressing himself through our columns to those of our readers who were friends and correspondents of Sir Roderick Murchison. They would much oblige and assist him if they would let him have the use of such of his letters as they can allow to be employed in the preparation of the biography which, at Sir Roderick's request, he has undertaken to write. If the documents are sent to him at Ramsay Lodge, Edinburgh, they will be returned at the earliest possible date.

FROM the English Government Eclipse Expedition we learn that since leaving Malta, on the evening of Saturday the 4th, the weather has been all that could be wished, and that Mr. Lockyer and the other members of the expedition have not failed to take all possible advantage of the calm weather in testing their instruments and preparing themselves in every possible way for rapid yet correct observations during the few minutes over which the phenomena of the morning of the 12th December will extend. The *Mirzapore* commenced steering through the canal at 2.30 on the 8th, and anchored in Suez Roadstead at twelve on the 10th, all well. It was hoped that she might sail by daylight on the morning of Sunday, the 12th. In that case she might get to Galle by the 25th, and the Expedition would then have seventeen days at their disposal for arranging themselves and their instruments over the line of totality, from the north of Ceylon to the western shore of Southern India. The passage through the Canal has been a pleasant and interesting one.

THE Falconer Memorial Fellowship, at the University of Edinburgh, which is of the annual value of about 100*l*., tenable for two years, has been conferred on Mr. William Stirling, B.Sc. The Baxter Physical Science Scholarship, vacant by the appointment of Mr. William Stirling, to the Falconer Memorial Fellowship, has been conferred for one year on Mr. Alexander Hodgkinson.

MR. P. L. SIMMONDS is now delivering at the London Institution, Finsbury Circus, the Travers Course for 1871-2, on Science and Commerce, illustrated by the raw materials of our manufactures, in two lectures, the first of which will be this evening, and the second on November 30th.

PROF. PARTRIDGE commenced his annual course of lectures on Anatomy to the pupils and Royal Academicians in the new theatre at Burlington House on Monday last week, and will continue the same every Monday evening up to December 11 inclusive, at eight o'clock.

AMONG the disastrous results of the recent fire at Chicago, one not referred to in the public papers was, we regret to learn from *Harper's Weekly*, the entire destruction of the building and collections of the Academy of Sciences of that city. This institution, first started by the energy of the late Mr. Robert Kennicott, and carried to its late condition of prosperity under the charge of Dr. William Stimpson, had already taken a front rank among the learned establishments of the country. Its publications embraced material of the utmost value, while its museum ranked at least as high as the fifth in the United States. Although believed to be fire-proof, the building, like others of the same character in Chicago, presented but little resistance to the flames, and everything within the walls was destroyed. The loss included, besides the collections in natural history of the Academy, a large number of marine invertebrates belonging to the Smithsonian Institution, which had been forwarded to Dr. Stimpson for investigation. The private cabinet of this gentleman, and a large mass of valuable manuscript belonging to him, embracing extended memoirs upon the mollusca, radiata, and crustacea of North America, with numerous illustrations, were entirely destroyed.

AFTER a seven years' tour of exploration in South America, Dr. A. Habel, a former resident of Hastings-on-the-Hudson, has returned to New York, where he is assiduously engaged in preparing the results of his labours for the press. Among the regions traversed by this gentleman may be mentioned the greater part of Central America, the Cordilleras of the Andes in Colombia, Ecuador, and Peru, and finally the Chincha Islands and the Galapagos. During this whole period Dr. Habel was diligently occupied in gathering information in regard to the natural and physical history of the countries mentioned, especially in the departments of ethnology, meteorology, and zoology. He has already made some communications on the subject of his travels to the Academy of Sciences at Paris, and other learned bodies, and we look forward to his detailed report with anticipations of much interest. The Guano deposits of the Chinchas were thoroughly explored by the doctor, who found them to be of a much more complicated structure than has hitherto been supposed.

MR. MESTRE, the secretary of the Academy of Sciences of Havana, has lately offered on its behalf certain prizes for memoirs on subjects of medicine and natural history, indicating a gratifying condition of scientific activity in Cuba. Competition is open to persons of all nations, although the memoirs are to be written in the Spanish language. Among the prizes mentioned by Mr. Mestre is one of three hundred dollars, proposed by the president of the society, Dr. Gutierrez, for the best paper upon a certain beetle, which is very destructive to the sweet-potato. A full account of the animal and its habits is required, and the best method of protecting the plants against its ravages. The Zayas premium of one hundred dollars is offered for a paper upon the hygiene of children—to be written as an aid to mothers. Competition for these prizes is to close on the 1st of March, 1872.

THE Royal Geographical Society has again invited the following public schools to take part in the competition for its prize medals in 1872:—*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. Examinations will be held in both Physical and Political Geography, the special subjects for 1872 being as follows:—In Physical Geography; the Physical Geography of South America and the adjacent Islands, Trinidad, Galapagos, Falkland Islands, and Tierra del Fuego. In Political Geography; the Geography of the same districts.

THE Bussey Institution School of Agriculture and Horticulture, in connection with Harvard University, has been established in execution of the Trusts created by the will of Benjamin Bussey, to give thorough instruction in Agriculture, Useful and Ornamental Gardening, and Stock-Raising. In order to give the student a sound basis for a thorough knowledge of these Arts, the school supplies instruction in physical geography, meteorology, and the elements of geology, in chemistry and physics, in the elements of botany, zoology and entomology, in levelling and road-building, and in French and German. Connected with it are the names of such eminent professors as Asa Gray in botany, Whitney in geology, Shaler in zoology, and Trowbridge in physics.

A REPORT on the Physical Laboratory of the Massachusetts Institute of Technology, has been presented to Prof. J. D. Runkle, President of the Institute, by E. C. Pickering, Thayer Professor of Physics. The object designed by the establishment of the laboratory was to provide apparatus and other convenience for the performance of the more common lecture-room experiments, to supply a place where investigations of a high order can be carried on, and to train instructors in physics for the numerous colleges now springing up all over the Continent of America. Particulars are given of a number of experiments of high order successfully carried on in the laboratory during the past year.

ENGLAND is beginning to acknowledge her forgotten scientific worthies. We learn that a public meeting was held last week at Birmingham, for the purpose of taking steps to establish a memorial to Dr. Priestley. It was resolved that the memorial should embrace three objects, viz., the purchase of a site, a scholarship, and a statue, so as to pay honour to Dr. Priestley both as a pioneer of science and as a champion of civil and religious liberty. A committee was appointed to carry the resolution into effect. It was stated that a sum of 3,000*l.* would be required, and several handsome subscriptions have been promised. The proposed site is that of the house at Fairhill, where Dr. Priestley resided for eleven years. The building was burnt down by rioters in 1791, after which he went to America.

THE Hartley Institution at Southampton has issued its Report for the year ending June 30, 1871. Although the managers of the Institution appear to have especially cultivated the training of engineering students for the Cooper's Hill College and elsewhere, the report refers with satisfaction to the increased number of students who have entered for general educational training as compared with former years.

THE Proceedings of the Bristol Naturalists' Society, vol. vi., part 1, for January to May 1871, contains the following papers:—The Natural History of the German People, by Dr Beddoe; On the Origin of Species in Zymotic Diseases, by D. Davies; Personal Experiences in the Deep-sea Dredging Expedition in H.M.S. *Porcupine*, by W. L. Carpenter; On the Strata comprising the shores of Waterford Haven, with especial Reference to the Occurrence of Llandeilo Fossils in that Locality, by Major T. Austin; On the Development of the Carboniferous System in the neighbourhood of Edinburgh, by E. W. Claypole; and On some Gravels in the Valley of the Thames in Berkshire, by E. W. Claypole. Valuable as these papers may be in themselves, it will be seen that not one of them has any special reference to the natural features of the neighbourhood of Bristol.

HERE is a grand opportunity for our young teachers of science; we give the trustees the benefit of the advertisement gratis:—"Grammar School of King Edward VI., Morpeth.—Wanted for the above school during the year 1872, a competent Master, to instruct the boys in modern languages (French and German indispensable). He will be required to attend at least forty days in each half year, and to teach not less than three hours each day. Salary, 50*l.* per annum. Travelling expenses at the rate of 10*l.* per annum will be allowed if the master does not reside in Morpeth. Also, a Master to teach elementary science (*botany, chemistry, and geology indispensable*). He will be required to attend twenty days in each half year, and devote three hours each day to teaching. *Salary*, 25*l.* *per annum.* Travelling expenses at the rate of 5*l.* per annum will be allowed as above. Also, a Master to teach practical drawing (to include mapping, planning, mechanical and architectural drawing). He will be required to attend twenty days in each half year, and devote three hours each day to teaching. Salary, 25*l.* per annum. Travelling expenses at the rate of 5*l.* per annum will be allowed as above. The trustees will not object to one master holding the two latter appointments. Applications, accompanied with testimonials, &c., to be sent to me on or before Friday, the 1st day of December next.—By order, BENJ. WOODMAN, Clerk to Trustees. Morpeth, 1st November, 1871." Seriously, we had thought the days gone by when it was deemed possible to teach "botany, chemistry, and geology," to say nothing of the other branches of "elementary science," in sixty hours in each half year, and to remunerate the teacher who is competent to instruct in all these subjects, at the rate of 12*s.* 6*d.* per diem and 2*s.* 6*d.* extra for travelling expenses!

THE new edition of Gauss's "Motus Corporum Cœlestium," which has just been published by Perthes, in Gotha, and which is designated as the seventh volume of Gauss's works, and is accompanied by a copy of the original vignette, might easily seem to be a part of the edition of Gauss's works, prepared by the Royal Society of Sciences in Göttingen. We are informed by that Society that the designation of this book as "Gauss's Works, vol. vii.," was chosen without their consent, and that it forms no part of the complete edition of Gauss's works, edited by the Royal Society, and now in the press.

WE are requested by Mr. R. A. Proctor to correct a slight error in the description of Mr. Brothers' negative of his star-chart given last week. The 8-inch negatives, like the 11-inch pictures, are copies of a chart containing upwards of 324,000 stars (not 50,000 only). Prof. Airy, at the last meeting of the Astronomical Society, remarked that the constellations in this chart are not conspicuous. They could not be so, without spoiling the chart; but the lithographic key-map practically removes the objection. The chart is a contribution to physical astronomy—not intended to aid the search for individual stars, though useful in the Observatory, as showing where the richer star-fields are.

MR. THOS. J. BOYD has reprinted his paper, "Educational Hospital Reform: The Scheme of the Edinburgh Merchant Company," presented to the Statistical Section at the recent meeting of the British Association.

A SERIES of "penny lectures for working men" in connection with the Museum of the Folkestone Natural History Society was commenced last week. The series is intended to illustrate the specimens in the museum—the subject of the first by the hon. secretary, Mr. Ullyett, being "Our Chalk Hills and their Fossils." If the experiment succeeds the lectures will be continued fortnightly during the winter months. Classes in botany and geology, also under the direction of the energetic secretary, were commenced on Wednesday, the 8th inst.

WE are glad to learn that the interesting series of popular science lectures, to which the charge of admission is only one penny, have been recommenced this winter session in Manchester. The opening lecture was delivered by Prof. Huxley on "Yeast," before a large and attentive audience.

THE *Echo Agricole* complains of the neglect of instruction in science in France. "Why," says this journal, complaining of the importance attached in most schools to a semi-mythological teaching, "when an intelligence is just opening to the light, should it be led through the delusive labyrinths of the marvellous, instead of showing it the truth in all its splendour? Let the young intelligence be accustomed to the observation of natural phenomena, and it will be seen to develop itself normally, because to all the branches of activity which it is called upon to exercise it will bring the spirit of methodical order which it will have been obliged to employ in the study of nature. We therefore demand that the Minister of Public Instruction should introduce into our primary schools the elementary teaching of natural science applied to what children see daily in the country. M. Jules Simon has ordered that a geographical class should be held every fortnight in the colleges and lycées; now we would have the Minister complete this measure by requiring the students not only to mention the principal products of such and such a country, but, as regards France especially, to take account of the natural produce of the land, and to know what sort of soil these different products affect. This would be geography applied to agriculture. . . . Further, we would require that all sciences relating to agriculture taught in the lycées and colleges should be followed by practical application to the soil, such experiments to form the basis of special examinations."

A DISCOVERY has been made by several farmers on the Loddon River in South Australia, that kangaroo rats are good thistle eradicators. "It has been found," says the *Bendigo Independent*, "that these animals dig down under the thistles, and eat the roots of the plants, which thus necessarily die. One farmer has issued orders that no kangaroo rats are to be killed on his land, in consequence of their having been of much service to him in destroying the obnoxious thistles."

AT a meeting of the Philosophical Society of Christchurch, New Zealand, in August last, the President, Dr. Haast, made a few observations on some moa eggs recently received from the Colonial Museum, in comparison with those of living birds. The various models of eggs were displayed on the table. The President said that the first egg to which he would desire to draw their attention was one the pieces of which had been discovered by the Hon. Walter Mantell, and by him reunited after much labour. The original egg, from which a model had been taken by Dr. Hector, was in the British Museum. The second model was that of the largest egg found. It had been discovered in the Kaikoras Peninsula, between the legs of a human skeleton, which had been buried in a sitting position, and, from the fact of it having been so found, he argued that the moa was of great antiquity, as there was no mention in the very earliest Maori traditions of such a mode of burial being adopted. The egg was afterwards exhibited at the Otago Exhibition, and the model had been made by Dr. Hector from measurements taken by him. The third and last model was that of a small egg now in the Colonial Museum, which had been found in Otago, and which had in it the bones of a moa chick.

ON the 17th of September the installation of the Academy of Natural Science took place at Bogota, in Columbia or New Granada, with much eremony. As yet not much can be expected from it, but it is another sign of the progress taking place in the country The orator gratefully commemorated what had been done for Columbia by Humboldt, Boussingault, and Acosta in geology, and by Mutis and Caldes in botany.

THE first meeting of the Eastbourne Natural History Society for the present season was held on Friday, October 20. A paper "On the species of Hepaticæ found in the Eastbourne District" was read by Mr. F. C. S. Roper, F.L.S., containing notes on the structure and development of the group, with a list of the species (fifteen in number) occurring in the neighbourhood. It was followed by a paper "On the Bones of Red Deer, &c., found in Eastbourne," by Mr. S. Eveshed. We are glad to observe that active local work is a prominent feature of this young society.

THE Whitechapel Foundation School Literary and Scientific Society held its first annual public meeting last week in the School-room, Leman Street. The Chairman, Mr. Edmund Hay Currie, member of the London School Board, having briefly referred to the importance of the work, and to the dissemination of scientific knowledge by the society's agency, called upon the hon. secretary to read the report; from this we gleaned that the association had made good progress during its first year of existence, and that the interest in the undertaking was rapidly increasing. Twenty-six lectures had been delivered, amongs the principal subjects were "Oxygen and Hydrogen," by Mr. Joseph Loane, M.R.C.S,, L.S.A., &c., &c., "Blood and its Constituents," and "Respiration, with its Mechanism," by Mr. H. A. A. Nicholls, of St. Bartholomew Hospital; "The Solar System," "Heat," "Coal and its products," "Electricity," "Chemical affinity," "Water," &c., &c. The evening's proceedings were brought to a termination with a lecture on "Light," illustrated by experiments, by the President, Mr. Charles Judd. We are glad to find that this society has received considerable recognition from gentlemen interested in science and in education generally.

COLDING ON THE LAWS OF CURRENTS IN ORDINARY CONDUITS AND IN THE SEA

[I SEND to NATURE for translation the abstract (in French) appended, according to a most excellent custom, to Colding's great paper in the Copenhagen Transactions. The subject is one of especial interest at the present time, though, of course, everything written by such a man is deserving of careful attention. Those in particular who met the genial Dane at the British Association will be glad to have in a compact form his views on a question which has given rise to much discussion, and which is of very great practical importance.—P. G. TAIT]

I PRESENTED in 1863 to the Scientific Society, and some months later to the Congress of Scandinavian Naturalists at Stockholm, a short exposition of my researches on the motion of fluid bodies, on which I had been occupied for several years, and the results of which appeared to me worthy of being submitted to the Society.

The characteristic of this work is that it does not suppose, like previous works of the same kind, that all the parts of a current are endowed with the same rapidity; for it owes, in fact, its existence to my conviction that this mode of looking at the subject can only lead in exceptional cases to exact results. My researches are based on the different motions assumed by the liquid threads or elements of the currents, and are supported by the well-known fact that any body, and consequently any portion of a fluid, can only move with a constant rapidity when the accelerating force is equal to the resistance.

In the case of a fluid flowing by virtue of its own weight over a plane surface which opposes a resistance to the motion of the water, it was easy to determine how this motion varies with the depth, when the rapidity of the current is constant in all its parts; and, by pursuing this train of thought, I was led to the law of the variation of the rapidity with the depth, when the current moves in a cylindrical conduit with circular section, completely filled with the liquid. These researches are of greater interest from the circumstance that the laws at which I have in this manner arrived from theoretical considerations, are confirmed by the experiments which have recently been carried out in France by Capt. Boileau and Inspector General Darcy. These laws of the motion of water may be expressed by the formula

$$(V - v)^2 = K_0^2 \frac{h}{l} x^3;$$

where V is the rapidity of the first elements of the current, the motion of which is the most rapid, v the rapidity at the depth x, $\frac{h}{l}$ the fall per foot of the water, and K_0^2 a magnitude which depends entirely on the nature and dimensions of the conduit, on the depth of the current, &c. The theory shows besides that the laws of the motion of water on a level surface are included in the general law of the motion of water on a cylindrical surface, when the radius of the cylinder is supposed infinite

Darcy, who has experimentally established the formula given above for cylindrical conduits, endeavoured, at the same time, to determine K_0^2 by means of certain experiments performed with four different kinds of pipes, and found that K_0^2 was inversely proportional to the square of the radius of the conduit. It resulted, according to the theory, that, for level conduits, K_0^2 should be in the same manner in an inverse ratio to the square of the depth of the current. But two series of experiments performed by Boileau with level conduits led, on the contrary, to the supposition that K_0^2 was inversely proportional simply to the depth of the current. There was thus a want of agreement between the results of the two experiments, and the point was to discover which of these two hypotheses was correct. Several circumstances leading me to believe that Darcy's theory could not be exact, I took as my starting point the experiments of Boileau, and considered K_0^2 as inversely proportional to the depth of the current, which I did with the less scruple since this hypothesis agreed almost as well with Darcy's experiments as with his own. I pursued, therefore, my researches on this basis, and, after many difficulties, arrived at results which, on the whole, were so entirely in accordance with experiments that I could not suppose the possibility of Boileau's hypothesis being inexact. It was only afterwards, when I approached the study of marine currents, that new difficulties constantly arose, which I endeavoured at first to overcome, but which became day by day more insurmountable, until at last there was nothing left but to doubt the correctness of my calculations, since they led to results which were in obvious contradiction to facts.

The theory then was shown to be inexact; but since in so large a number of cases it was evidently in agreement with experiment, I attempted by a variety of means to discover the error which I must have committed; still all my attempts were unattended with result, and I was on the point of abandoning the resolution of the problem to which I had already devoted so much time, when the idea struck me of examining what would happen if I rejected Boileau's determination of K_0^2, and adopted Darcy's hypothesis, although it still appeared to me impossible; when I found, with as much delight as surprise, that it removed not only the great difficulties which I had up to that time encountered, but also all the contradictions which had occurred to me as an inevitable consequence of that hypothesis, and from that moment the results of the calculations showed themselves to be entirely in the most perfect accordance with what exists in nature.

The circumstance that the experiments of Darcy are almost as satisfactory whether $\frac{1}{K_0^2}$ is supposed to be proportional to the first or to the second power of the depth of the currents, made me think that the reality would be still more nearly approached by expressing this magnitude by a binomial of the first and second degree, and this was completely confirmed by facts. In determining the constants of the binomial according to the results of Darcy's experiments, I found the law of the motion of the water in cylindrical pipes with a radius R, with a coefficient of resistance m, and a rapidity v_0 at the surface of the conduit, may be represented by the formula

$$V - v = 6{\cdot}8 \sqrt{m} \times v_0 \times \left(\frac{x}{R}\right)^{\frac{3}{2}} \sqrt{\frac{117{\cdot}7\,R}{62{\cdot}5 + 117{\cdot}7\,R}}$$

V being the rapidity next the axis, to which corresponds $x = 0$. This formula may be applied equally to the movement of water in level conduits, if by R is designated the depth of the current; only the coefficient then becomes $\frac{6{\cdot}8}{\sqrt{2}} = 4{\cdot}8$, instead of $6{\cdot}8$.

This formula shows, among other things, that the ratio $\frac{v}{V}$ which corresponds to any point in a given conduit entirely filled by the current, is entirely independent of the rapidity of the current, a fact which Darcy's experiments confirm in a remarkable manner. This relation furnishes us besides with the means of determining the value of the coefficient of resistance m for different kinds of pipes which were employed by Darcy, and it is thus found that for

Old pipes . .	m = from 0·0120 to 0·0080
New pipes . .	m = from 0 0050 to 0·0033
New varnished pipes.	m = from 0.0033 to 0·0025

values which are altogether independent of the diameter of the conduit. For level wooden conduits, it is found, according to the experiments of Boileau, that m = 0·0160 to 0·0090, while the resistance of the air, according to the same author, corresponds to m = 0·0003 to 0·0002

Inspector-General Darcy has unfortunately died, but the researches on currents which he commenced were continued by the French engineer Bazin, who published in 1865 a great work on the results of a considerable number of experiments carried on with conduits of very different kinds.

However interesting otherwise these researches may be, they do not display either the powers of observation or the grandeur of conception which distinguish the works of Darcy. Among those experiments which are of the greatest interest, there are some begun by Darcy and finished by Bazin, such as researches into the motion of water in rectangular conduits, where the rapidity is determined in 45 points symmetrically distributed. The result for these, as for circular conduits, is that the ratio $\frac{v}{V}$ is independent of the absolute rapidity of the current, and if the results of experiment on the motion of water in level conduits are compared with those given by the theoretic formulæ, it will be found that these last agree so completely with experiment, that the difference between the calculated and observed rapidities, in each of the 45 points mentioned above, falls within the limit of errors of observation. This agreement is especially remarkable in the case of the conduit which Darcy employed in 1857 for the carrying out of his researches. In 1859 Bazin undertook similar experiments with a small rectangular conduit; but he did not make so great a number of experiments, and his errors of observation are larger than those of Darcy. In determining the coefficients of resistance of these conduits, it was found that for those of Darcy $m = \cdot 0104$, while for those of Bazin it rose to 0·0180.

Bazin performed a considerable number of experiments on the motion of water in open conduits, and thought himself compelled to admit that the laws of this motion are essentially different from those which relate to perfectly full conduits; but he is certainly in error.

The results of a considerable number of ancient measures of currents are in existence, which Bruning undertook, towards the close of last century, in different rivers—namely, the Rhine, Waal, &c. They were performed with much care; but, as might be foreseen, are nevertheless very defective. They deserve, however, to attract attention, partly because the rapidity was determined, for every section of the current, at distances of six inches from the surface to the bottom, in a series of perpendiculars, the imperfections which the measurement of the rapidity presents losing thus much of their importance; partly and especially because the currents examined by Bruning were of a depth which reached 23 feet. In applying this theory to these currents, and especially in determining the constants of the formulæ with the aid of Bruning's measurements, it was found that the observed and calculated rapidities are for all depths as accordant as could be desired; and this agreement furnishes a new proof of the exactness of the theory. The coefficient of resistance m, calculated according to Bruning's measurements, varies between 0·0250 and 0·0080, with a mean of 0·0160; and as the resistance at the depth of these currents must doubtless approach that which a marine current experiences in flowing over a mass of water placed beneath it, and which does not participate in the motion, I have a right to believe that the extreme value $m = 0{\cdot}0250$ corresponds nearly to the resistance which currents meet with when flowing freely in the sea.

After having in this manner assured myself that the preceding theory agrees with experiment wherever it has been tried, I endeavoured to determine the laws of the motion of water in currents of variable rapidity. In considering the simplest case of this kind, that, namely, in which the conduit is a level surface (I had already treated this case by the old theory), it was found that the laws of currents, according to the new theory, are entirely in agreement with the facts observed in nature; and consequently this theory may be regarded as giving the explanation of all permanent currents.

Having thus shown that this theory of the movement of fluid bodies accounts satisfactorily for all the phenomena, I shall now, from this as a stand-point, give a review of my recent researches upon ocean currents. The currents which more particularly demand our attention here are those of the North Atlantic, especially the Gulf Stream and the Polar Currents.

The Gulf Stream issues, as we know, from the Gulf of Mexico, but it is possible to follow its course across the Carribean Sea, where passing between the Antilles, it arrives from the Atlantic, and afterwards flows to the north-west at the rate of $\frac{1}{4}$ mile an hour until it enters the Gulf of Mexico. From this Gulf the Gulf Stream takes an easterly course towards the Bahamas along the north coast of Cuba; but, after rounding Florida, it bends northwards, and passes between the latter and the Bahamas, in the channel which separates Florida Cape from the Islets of Bemini; here the current has a speed of 1 mile per hour, a breadth of 8 miles, and a depth of 250 fathoms. From the channel of Bemini the Gulf Stream proceeds directly northwards at a rate which decreases gradually from $6\frac{1}{3}$ feet per second at Bemini to 4 feet at St Augustine; the distance between these two points being about 70 miles, during which the breadth of the current increases from 8 miles to $11\frac{3}{4}$. From St. Augustine to the Bay of New York the Gulf Stream takes a north-easterly course, parallel with the land, and conterminous with a cold current which flows from the north to the south between the stream and the American coast. In this part of its course it continues to increase in breadth from $11\frac{3}{4}$ miles at St. Augustine to $31\frac{3}{4}$ at New York; meanwhile its speed decreases from 4 feet to $2\frac{1}{2}$ per second. The depth of the sea along the course of the current is many hundred fathoms, and the distance between St. Augustine and New York is 180 miles. On quitting the Bay of New York, the Gulf Stream takes an E.N.E. direction to the south of Newfoundland, skirting the cold current, which goes down to south-west as far as New York, following the east coast of Newfoundland. By the time the Gulf Stream, after a course of 200 miles, reaches the south of Newfoundland, it has attained a breadth of about 80 miles, while its speed is only 2 feet per second; but the current continues to run in the same direction towards Europe for other 300 miles, with a speed which is from 2 feet to 0·6 feet, and a breadth increasing from 80 up to 200 miles. The Gulf Stream, when it has attained a distance of 750 miles from Bemini, separates into two branches, the one proceeding southwards towards the coast of Africa, at a speed of 0·6 feet per second, the other taking a northerly course towards Iceland, along the shores of the British Islands, and running about 200 miles, at a rate which decreases from 0·6 to 0·3 feet per second, the breadth of the current meanwhile increasing from 100 to 105 miles. When the stream reaches the neighbourhood of Iceland, it sends off a branch which skirts the south coast of that island, afterwards taking a direction north-west towards the Polar current of the east coast of Greenland, which it seems partly to follow in its march southwards. As to the main stream, it inclines to the east after passing the extreme north of Scotland, and then runs to the north-east, along the west coast of Norway, until it ends its wanderings in the Icy Sea.

As to the Polar Current we feel authorised to mention the following statements:—From the region of the Icy Sea, the most northerly of which we have any knowledge, from the neighbourhood of Spitzbergen about the 80th degree of N. latitude, there descends to the south-west a great polar current loaded with floating ice. It reaches the coast of Greenland at about 70° N. latitude, and follows it as far as Cape Farewell; its breadth being nearly 40 miles and its speed $\frac{2}{3}$ of a foot per second. After passing Cape Farewell, it curves round to northward and follows the west coast of Greenland for some distance into Davis Strait. After having run for a few degrees in this direction it bends to the south-west, towards the coast of Labrador, along the whole length of which it runs, then proceeding to the south-east, enlarged by the polar current which comes from Baffin's Bay. On quitting Labrador, where its speed is $\frac{5}{6}$ of a foot per second, and its breadth 50 miles, the polar current on rounding the east coast of Newfoundland makes for the Gulf Stream, and, after doubling Cape Race, sends a branch to the south-west between the Gulf Stream and the American coast, which branch can be traced as far as Florida. As to the part of the polar current which does not take this route, it is generally admitted that it flows underneath the Gulf Stream on the east of Newfoundland, and that it runs uninterruptedly to the south-east, towards the African coast, where the waters of the ocean are of a temperature comparatively low.

In order to explain the causes of these immense ocean currents by the aid of the laws of the movement of water in ordinary conduits, it is necessary first of all to know the forces which produce and maintain the movement of these currents. Captain Maury, who has made a special study of this question, has given it as his opinion that these ocean currents are due to the differences caused by the changes of temperature and of saltness in the specific gravity of the water of the sea. In order to make this theory more easy of comprehension, Maury imagines a globe like the earth covered over the whole of its surface with a sea 200 fathoms in depth, the water throughout being of the same density; at the same time he supposes the surrounding circumstances to be the same at all points, and that there being neither evaporation nor precipitation, there can of course be neither winds nor currents upon the imaginary globe. He next supposed the water contained between the tropics suddenly transformed into oil to a depth of 100 fathoms. From this moment the equilibrium is destroyed, and there results a general system of currents and counter-currents; for the oil, being lighter than the water, will rush along the surface towards the poles, while the water of these regions makes for the equator in the shape of a submarine current. As the oil reaches the polar sea, it is supposed to be transformed into water, which returns to the equator, where it is changed anew into oil that again rises to the surface and again makes its way to the poles, and so on. If then this globe turns, like the earth, on its axis once in the twenty-four hours from west to east, each particle of oil, according to Maury, will proceed towards the pole in a spiral course with a speed towards the east always increasing; on reaching the pole it will turn at a rate equal to that at which the earth revolves at the equator, viz., 225 miles an hour. But, says Maury, when the oil has been changed into water, it will return towards the equator describing a curve in a westerly direction. If the sea in question should be bounded by land, as is the case on the surface of the earth, the uniformity of these currents will be broken up by different local circumstances; and the speed of the currents will vary at various places, but there will always be a system of equatorial and polar currents. Is it not admissible then to suppose, asks Maury, that the cold waters coming from the north and the warm waters issuing from the Gulf of Mexico and made lighter by the heat of the tropics, will act relatively to each other in the same way as the water and the oil in the preceding example?

The Gulf Stream was at one time regarded as a branch of the Mississippi; but this notion must be abandoned since it has been proved that the volume of the Gulf Stream is many thousand times greater than that of the river, and that its water is salt, while the water of the Mississippi is fresh. Next, Benjamin Franklin's idea was generally adopted, viz., that the trade-winds drive the waters before them into the Carribean Sea, whence they issue more slowly in forming the Gulf Stream. Maury, however, refuses to accept this explanation; he admits that the trade-winds may increase the speed of the stream in the strait of Florida, but he maintains that it is impossible for these winds to give such an impetus to the Gulf Stream as would make it traverse the whole of the Atlantic as a markedly distinct current. He caps his objections to the theory of Franklin by remarking, that as surely as a river flows along its bed only under the influence of gravity, so the course of the Gulf Stream in the midst of the ocean necessitates the existence of a never-ceasing moving force; in short, he says, if gravity did not exist, the waters of the Mississippi would never leave their source, and, were it not for a difference of specific weight, those of the Gulf Stream would remain for ever in the tropical regions of the Atlantic. But as Maury disputes the correctness of Franklin's statement, viz., that the surface of the sea is above the normal level in the Gulf of Mexico, and that the water tends by virtue of its weight to rush towards the north, and as observation has proved that along the western edge of the Gulf Stream there flows a current of those cold waters which descend southwards as far as Florida Strait, he can no longer maintain his first opinion as to the cause of the Gulf Stream. He is forced to resign the hypothesis that the water of the Gulf Stream, on account of its greater degree of saltness, has a specific gravity greater than the water of the polar seas, to which it flows in virtue of its great density, causing a current in a direction contrary to the lighter waters of these colder regions. But from the moment that Maury supposes that the ocean currents have their origin at the time when the water of the tropics is lighter, and that of the Gulf Stream heavier than the water of the Polar seas, his point of view becomes uncertain and difficult to sustain; and he fails all the more signally in presenting the question of the currents in its true light, from the fact that at that time there existed no exact method of obtaining the specific gravity of the water of the ocean, the degree of saltness of the different seas being then unknown.

(*To be continued.*)

*PHYSIOLOGY FOR WOMEN**

BY Physiology we should understand a knowledge of the functions of the human body, and of the laws which regulate and maintain its various actions. The physiology of plants and of the lower tribes of animals (Botany and Zoology) are described by two other Professors in the University, and there will be little enough time for me to condense and give an account of what is now known of the subject, even as I have limited it. Whatever useful information, however, can throw light on human physiology, derived from every collateral science, will be made use of to assist inquiry. After some preliminary lectures on the histology, chemistry, the physical and vital properties of the tissues, I shall more especially dwell on the two great functions of nutrition and innervation. The former involves an acquaintance with what constitutes a proper food for man—how it is prepared by mastication, insalivation, digestion, chymification, sangnification, and respiration, to form the blood; how out of this blood the tissues are formed; and how, after these have fulfilled their proper uses, they are separated from the body in the act of excretion. The latter comprehends a description of the functions of mind, including the mental acts, sensibility, sensation, volition, and the varied kinds of motion; of the functions of the nerves; of the special senses, such as smell, taste, touch, sight, hearing, and the muscular sense of voice and speech; and lastly, of sleep, dreams, somnambulism, catalepsy, trance, witchcraft, animal magnetism, &c. &c. Of the subjects included under these heads it is impossible to overrate the importance in reference to their relation to the health and happiness of man, his physical and moral welfare, his social relations, his national resources, and the prosperity of his race. I have long formed the opinion that physiology, besides being essential to the medical student, should be introduced as an elementary subject of education in all our schools—should be taught to all classes of society. It is an ascertained fact that 100,000 individuals perish annually in this country from causes which are easily preventible, and that a large amount of misery is caused by an ignorance of the laws of health. The clergy should especially study it—first, with a view of diminishing the difference in thought existing between literary and scientific men; and, secondly, because their influence on the people from the pulpit, and as parish ministers, is so important. All other professions and trades, however, might beneficially study physiology, especially newspaper editors and reporters, who diffuse a knowledge of useful things among the public; and architects, who have not yet learnt to build dwelling-houses and public halls properly ventilated. But women, in all classes and degrees of society, have more to do with the preservation and duration of human life even than men. It has been argued that, inasmuch as even the brutes know instinctively how to take care of their young, so must women be able to do the same. But the human infant is the most helpless of creatures, and nothing is more lamentable than to witness the anxieties and agony of the young mother as to how she should manage her first-born. In no system of education are women taught the structure and requirements of the offspring which will be committed to their charge; and certainly no error can be greater than to suppose that the senses and instincts are sufficient for teaching man as to his physical, vital, and intellectual wants. The enormous loss of life among infants has struck all who have paid attention to the subject, and there can be no question that this is mainly owing to neglect, want of proper food or clothing, of cleanliness, of fresh air, and other preventible causes. Dr. Lankester tells us, when ably writing on this topic, that, as coroner for Central Middlesex, he holds one hundred inquests annually on children found suffocated in bed by the side of their mothers, and he calculates that in this way 3,000 infants are destroyed in Great Britain annually alone, attributable in nine cases out of ten to the gross ignorance of those mothers of the laws which govern the life of the child.* But women are the wives and regulators of the domestic households. They also constitute the great mass of our domestic servants. On them depends the proper ventilation of the rooms, and especially the sleeping rooms, in which all mankind on an average spend one-third of their lives. Children are too often shut up all day in crowded nurseries, and when ill, are subjected to numerous absurd remedies before medical assistance is sent for. Their clothing is often useless or neglected, the dictation of fashion rather than of comfort and warmth being too frequently attended to. The cleanliness of the house also depends on women, and the removal of organic matter from furniture and linen, the decomposition of which is so productive of disease. Further, the proper choice and preparation of food is entrusted to them,—all these are physiological subjects, the ignorance of which is constantly leading to the greatest unhappiness, ill health, and death. Among the working classes it is too frequently the improvidence and ignorance of the women which lead to the intemperance and brutality of the men, from which originate half the vice and crime known to our police offices and courts of justice. Additional arguments for the study of physiology by women may be derived from the consideration of—(1) the effects of fashionable clothing—the tight lacing, naked shoulders, thin shoes, high-heeled boots—often subversive of health; (2) the great objects of marriage—the production of healthy offspring—and all the foresight, care, and provision required, but too often neglected through ignorance, to the danger both of mother and child; (3) the proper employment of women, which should be regulated with regard to their conformation and constitutions; and (4) nursing the sick, which is one of the most holy occupations of women, and which would be much more intelligently done if

* Abstract of the Opening Lecture of the Ladies' Course of Physiology, delivered in the University of Edinburgh, Nov. 2, by Prof. Bennett.

* See his excellent pamohlet, "What shall we Teach; or Physiology in Schools." London: Groombridge & Sons, 1870.

they possessed physiological knowledge. Doubtless those who regard this study as too difficult and technical for young men, will decry it also for women; yet it so happens that for them nothing is so truly interesting as this science. The examination-papers of school-girls of the Ewart Institution, Newton-Stewart, contain an amount of information in physiology perfectly astonishing. Seldom have medical students given better answers. And yet it has been argued that physiology was far too difficult and technical a subject to be studied even by the students in Arts of our University. Hence women in all ranks of society should have physiology taught to 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.

SOCIETIES AND ACADEMIES

London

Geological Society, November 8.—Joseph Prestwich, F.R.S., president, in the chair. Mr. Henry Hicks was elected a Fellow, Dr. Franz Ritter von Hauer, of Vienna, a Foreign Member; and M. Henri Coquand, of Marseilles, a Foreign Correspondent of the Society. The following communications were read:—1. A letter from the Embassy at Copenhagen, transmitted by Earl Granville, mentioning that a Swedish scientific expedition, just returned from the coast of Greenland, had brought home a number of masses of meteoric iron found there upon the surface of the ground. These masses varied greatly in size; the largest was said to weigh 25 tons. Mr. David Forbes, having recently returned from Stockholm, where he had the opportunity of examining these remarkable masses of native iron, took the opportunity of stating that they had been first discovered last year by the Swedish Arctic expedition, which brought back several blocks of considerable size, which had been found on the coast of Greenland. The expedition of this year, however, has just succeeded in bringing back more than twenty additional specimens, amongst which were two of enormous size. The largest, weighing more than 49,000 Swedish pounds, or about 21 tons English, with a maximum sectional area of about 42 square feet, is now placed in the hall of the Royal Academy of Stockholm; whilst, as a compliment to Denmark, on whose territory they were found, the second largest, weighing 20,000 lbs., or about 9 tons, has been presented to the Museum of Copenhagen. Several of these specimens have been submitted to chemical analysis, which proved them to contain nearly 5 per cent. of nickel, with from 1 to 2 per cent. of carbon, and to be quite identical, in chemical composition, with many aërolites of known meteoric origin. When polished and etched by acids, the surface of these masses of metallic iron shows the peculiar figures or markings usually considered characteristic of native iron of meteoric origin. The masses themselves were discovered lying loose on the shore, but immediately resting upon basaltic rocks (probably of Miocene age), in which they appeared to have been originally imbedded; and not only have fragments of similar iron been met with in the basalt, but the basalt itself, upon being examined, is found to contain minute particles of metallic iron, identical in chemical composition with that of the large masses themselves, whilst some of the masses of native iron are observed to enclose fragments of the basalt. As the chemical composition and mineralogical character of these masses of native iron are quite different from those of any iron of terrestrial origin, and altogether identical with those of undoubted meteoric iron, Prof. Nordensckjold regards them as aërolites, and accounts for their occurrence in the basalt by supposing that they proceeded from a shower of meteorites which had fallen down and buried themselves in the molten basalt during an eruption in the Miocene period Notwithstanding that these masses of metallic iron were found lying on the shore between the ebb and flow of tide, it has been found, upon their removal to Stockholm, that they perish with extraordinary rapidity, breaking up rapidly and falling to a fine powder. Attempts to preserve them by covering them with a coat of varnish have as yet proved unsuccessful; and it is actually proposed to preserve them from destruction by keeping them in a tank of alcohol. Mr. Maskelyne stated that the British Museum already possessed a specimen of this native iron, and accounted for its rapid destruction on exposure by the absorption of chlorine from terrestrial sources, which brought about the formation of ferrous chloride. This was particularly marked in the case of the great Melbourne meteorite in the British Museum; he had succeeded in protecting this, as well as the Greenland specimen, by coating them externally, after previously heating them gently, with a varnish made of shellac dissolved in nearly absolute alcohol. He considered it probable that a meteoric mass falling with immense velocity might so shatter itself as to cause some of its fragments to enclose fragments of basalt, and even to impregnate the neighbouring mass of basalt with minute particles of the metallic iron; but he considered the question of meteoric origin could only be decided by examining the same mass of basalt at some greater distance from the stones themselves, so as to prove whether the presence of such metallic iron was actually characteristic of the entire mass of the rock. Prof. Ramsay referred to the general nature of meteorites and to their mineral relationship to the planetary bodies, and remarked that, supposing the earth to have in part an elementary metallic core, eruptive igneous matter might occasionally bring native iron to the surface. Mr. Daintree mentioned that he had been present at the exhumation of the Melbourne meteorite, and that at that time there was little or no trace of any exudation of ferrous chloride, the external crust on the meteorite being not above $\frac{1}{32}$ inch in thickness. 2. "On the Geology of the Diamond-fields of South Africa." By Dr. J. Shaw, of Colesberg. Communicated by Dr. Hooker, F.R.S. The author described the general structure of the region in which diamonds have been found. He considered that the diamonds originally belonged to some metamorphic rock, probably a talcose slate, which occupied the heights during a late period of the "trappean upheaval," to which he ascribed the origin of the chief physical features of the country. This upheaval was followed by a period of lakes, the traces of which still exist in the so-called "pans" of the region; the Vaal river probably connected a chain of these lakes; and it is in the valley of the Vaal and the soil of the dried up "pans" that the diamonds are found. The author referred also to the frequent disturbance and removal of the diamentiferous gravels by the floods which prevail in these districts after thunder-storms. 3. "On the Diamond-gravels of the Vaal River, South Africa." By Mr. G. W. Stow, of Queenstown, Cape Colony. Communicated by Prof. T. Rupert Jones. The author described the general geographical features of the country in which diamonds have been found, from Mamusa on the south-west to the headwaters of the Vaal and Orange Rivers. He then indicated the mode of occurrence of the diamonds in the gravels, gravelly clays, and boulder-drifts of the Vaal Valley, near Pniel, including Hebron, Diamondia, Cawood's Hope, Gong Gong, Klip Drift, Du Toit's Pan, and other diggings. By means of sections he showed the successive deepenings of the Vaal Valley and the gradual accumulation of gravel-banks and terraces, and illustrated the enormous catchment area of the river-system, with indications of the geological structure of the mountains at the headwaters. The specimens sent by Mr. Stow, as interpreted by Prof. T. R. Jones, showed that both igneous and metamorphic rocks had supplied the material of these gravels. The author concluded that a large proportion of these materials have travelled long distances, probably from the Draakensberg range; but whether the original matrix of the diamonds is to be found in the distant mountains or at intermediate spots in the valleys, the worn and crushed condition of some of the diamonds indicates long travel, probably with ice-action. Polished rock-surfaces and striated boulders, seen by Mr. Gilfillan, were quoted in corroboration of this view. Mr. Woodward mentioned that Mr. Griesbach and M. Hübner had been over the country described in these papers, and had communicated a map of it to Petermann's Journal. Mr. Griesbach stated that the rock described as metamorphic in the paper was by M. Hübner regarded as melaphyre, and that in some parts of the Vaal Valley the beds of the Karoo formation might be seen *in situ*. He disputed the possibility of any of the gravels being of glacial origin. He was convinced that there were no metamorphic rocks on the western side of the Draakensberg; those regarded as such probably belonged to the Karoo formation. Prof. Tennant commented on the large size of the diamonds from the Cape, of which he had within the last few months seen at least 10,000, many of them from 30 to 90 carats each. Some broken specimens must, when perfect, have been as large as the Koh-i-Noor. Mr. Tobin corroborated the information given by Mr. Stow, and stated that the source of the Vaal was in sandstone, and that it was not until it had traversed some distance that agates, peridot, and spinel were met with. The large diamonds, in his view, occurred principally in old high-level gravels, at a considerable elevation above the river, which had much deepened its valley since the time of their deposit. At Du Toit's Pan, however, none of the diamonds, nor indeed any of the other stones, showed any signs of wear; and he considered that at that spot was one of the centres at which diamonds had been found in their original matrix. Mr. Daintree stated that in Australia there were agate-bearing beds of amygdaloid greenstone similar to those in South Africa, and that he had called attention to their existence in the neighbourhood of the Burnett River, where since then a diamond of the value of 80*l*. had been discovered. Mr. Maskelyne commented on the dissimilarity of the minerals found in the diamond-bearing beds of Brazil from those of Du Toit's Pan or of South Africa generally. He thought that possibly the minerals described as peridot and spinel might be bronzite and garnet, which, however, came from igneous rocks; and the remarkable fact was that with them occurred unrolled natrolite and diamonds in an equally unrolled condition, which was suggestive of their having been due to a common origin. Mr. Ward gave an account of an examination of some of the rock from Du Toit's Pan, with a view of discovering microscopic diamonds, none of which, however, had been found. Prof. Rupert Jones had been equally unsuccessful in the search for minute diamonds, both in sand from Du Toit's and in the ochreous gravel from Klip drift. He pointed out the waterworn condition of the agates from Du Toit's Pan, which showed aqueous action, though there were also several other minerals present in a perfectly fresh and unrolled condition. He thought a careful examination of the constituent parts of the gravel might ultimately throw light on their origin. That fluviatile action was sufficient to account for their presence had already been shown by Dr. Rubidge and others, who had treated of the grand plateaux and denudations of the district under notice.

Royal Geographical Society, November 13.—Major-General Sir H. C. Rawlinson, K.C.B., president, in the chair. The president, on opening the session, delivered an address, in which, after paying an eloquent tribute to the worth of the late president, Sir Roderick Murchison, and expressing his sense of the loss which the Society had sustained in his death, he reviewed the progress of geography since the last meeting of the previous Session. He congratulated the Fellows on being again permitted to meet in the handsome and commodious hall of the London University; and stated that the Council felt that the Senate of that body, in granting the use of the hall, conferred an obligation not only on the Society but on the public at large, whose instruction and education in geography formed the especial objects of their study. He also announced that the Society had, during the recess, taken up its permanent quarters in Savile Row, where it was now located on its own freehold estate. In Physical Geography the important subject of Oceanic Circulation, and Dr. Carpenter's researches thereupon, was prominently noticed; and he stated that Dr. Carpenter, during his Mediterranean voyage of the past summer, had met the objections of his critics in so far as related to the under-current outwards at the Straits of Gibraltar by experimentally proving that such a current really does exist. In Arctic exploration the recent German expeditions were noticed, particularly the voyage of Messrs. Payer and Weyprecht, who, last summer, had found an open sea, in lat. 70°, between Spitzbergen and Nova Zembla. In Central Asia and Eastern Persia much accurate information had recently been obtained by English travellers and surveyors; and in Syria their medallist, Captain Burton, had recently, in company with Mr. Drake, examined the Anti-Libanus and the little-known district east of Damascus,—subjects on which this indefatigable traveller would read papers at a subsequent meeting. An excellent descriptive paper had been received from the well-known and able traveller Captain Blakiston, on the subject of the island of Yezo, the circuit of which he had recently explored in the capacity of an official of the Japanese Government. No direct news had been recently received either from Dr. Livingstone or Sir Samuel Baker; but authentic intelligence of Livingstone could not be much further delayed, as an able and adventurous American gentleman, Mr. Stanley, left Zanzibar for the shores of Lake Tanganyika in February last, taking with him "Bombay," one of Speke and Grant's "faithfuls," as guide. He (the president) added that if Mr. Stanley succeeded in restoring Livingstone to us, or in assisting him to solve the great problem of the upper drainage into the Nile or Congo, he would be welcomed by the Society as heartily and warmly as if he were acting under their own immediate auspices.—A paper was then read "On the Exploration of the Limpopo River," by Captain Frederic Elton. This remarkable journey was performed between July 6 and August 8, 1870, the author starting from the Tati gold-fields and proceeding by an easterly route to the junction of the Tuli River with the Limpopo, and thence descending the great stream or marching along its banks to beyond the junction of the Lipalule, whence he struck across to Lorenço Marques, in Delagoa Bay. The middle part of the Limpopo, between the Tuli and Lipalule, was found to be encumbered with rapids and waterfalls, some of which, especially the cataracts called Tolo-Azime, were truly magnificent, the river, after a series of rapids five miles in length, here plunging over a ledge into a deep chasm. These falls mark the spot where the Limpopo leaves the great interior plateau of Africa and descends abruptly into the plains which extend henceforth to the sea. The paper described the country traversed as rich and abundant in game of all descriptions.

Mathematical Society, November 9.—Dr. Spottiswoode, president, in the chair. The following gentlemen were elected to form the council for the ensuing session:—President: Dr. Spottiswoode. Vice-Presidents: Profs. Cayley, Henrici, H. J. S. Smith, and Mr. S. Roberts. Treasurer: Dr. Hirst. Honorary Secretaries: Messrs. M. Jenkins and R. Tucker. Other members: Profs. Clifford and Crofton, Dr. Sylvester, Hon. J. W. Strutt, Messrs. T. Cotterill, Merrifield, Stirling, and Walker. Mr. A. Freeman was proposed for election. It being unanimously agreed upon that the number of honorary foreign members should be increased to six, the president read out the names which the council recommended for nomination, viz.: Dr. Clebsch, M. Hermite, Prof. Cremona, Dr. Hesse, and Prof. Betti. The only foreign member at present is M. Chasles. Dr. Sylvester then gave an account of his communication "On the partition of an even number into two primes." In one of his minor papers Euler has ennunciated as a theorem, resting entirely on intuition from a comparatively small number of instances, that every even number may be decomposed into a sum of two primes. The object of Dr. Sylvester's communication was to obtain some measure of the probable number of ways in which such decomposition can be effected for any given number; if it can be shown to be probably greater than the square root of the number itself, it will follow from generally admitted principles of the theory of chances, that the probability of the theorem being universally true above any assigned limit, if *proved* to be true up to that limit, may be represented by an infinite product of terms, which will approach as near as we please to unity the higher the limit in question is taken. The mere fact of the theorem, as Euler gave it, being proved up to 100,000,000, or any other number however great, would leave the probability of its being universally true, absolutely zero, just as the fact of the sun having risen 100,000,000 times would not contribute an atom of probability to the supposition that it would continue to rise for all time to come. In the case before us, on the contrary, the probability of the theorem being universally true by a sufficiently copious induction, may be made to approach as near as we please to absolute certitude. The author considers that he has established beyond the reach of reasonable doubt that the magnitude which represents the mean probable value of the number of modes of effecting the resolution of a very large even number into two prime numbers is that of the square of the number of primes inferior to the given number divided by the number itself, or which (thanks to the discoveries of Legendre and Tchebicheff) we know to be the same thing, the number of the decompositions in question bears a finite ratio (assignable within limits) to the number to be decomposed, divided by the square of its Napierian logarithm. If we agree provisionally to call preter-primes in respect to n, those numbers which are prime themselves, and also when subtracted from n leave prime remainders, the author shows that the probable number of such preter-primes (*i.e.*, the most probable value attainable under our present conditions of knowledge) may be found approximately by multiplying the number of ordinary primes inferior to n by the product of a set of fractions, depending in part on the magnitude and in part on the constitution of the number n. If n is the double of a prime, the product in question is got by multiplying together all the quantities $\frac{p-2}{p-1}$ where p is every odd prime between unity and the square root of n; but if n itself contains any such primes among its factors, then the corresponding factors are to be omitted out of the product. We thus see that if two even numbers of considerable magnitude lie adjacent or tolerably near to each other, one of which is the double of a prime, but the other six times a prime, the number of preter-primes relative to the latter will be about twice as many as those relative to the former. For the purpose of greater simplicity of explanation, the formula of approximation has been stated above with less accuracy than it admits of being stated with. Instead of the total number of odd primes being multiplied by the product of factors last described, those only should have been taken which are not intermediate between 2 and $\sqrt{n}$, and the result so modified should have been stated to be the probable value not of the total number of preter-primes, but only of such of them (by far the larger number) as are not of the excluded class above described, nor subtracted from n, give rise to remainders belonging to such class. The author has found by actual trial on an extensive scale, that the estimated values of the number of decompositions never differ by more than a moderate, and in some cases exceedingly slight, percentage from their actual values determined by the use of Borchardt's tables. The same methods enable him also to assign a probable value to the number of modes of resolving an odd number into the sum of one prime and the double of another, and in general lead to an approximate representation of the number of solutions in prime numbers of any system of linear equations of which the total number of solutions is limited, and even to resolve approximately such questions as that of determining how many prime numbers there are inferior to a given limit, which are followed by prime numbers differing from them by any assigned interval. Since the communication made to the Mathematical Society, the secretaries have been favoured with a note from which they understand that Dr. Sylvester has verified his results by quite a different method. The exact number of the solutions of the equation $x + y = n$ in prime numbers may be expressed algebraically by means of the method of generating functions in terms of the inferior primes to n. The expression will be found to consist of two parts, one a constant multiple of n, the other, a function of the roots of unity corresponding to the several inferior primes and their combinations. The former non-periodic part may obviously be regarded as the even value of the expression, and Dr. Sylvester has found that it is identical with the value obtained by the method of averages previously employed. In order to prove strictly Euler's theorem, it only remains to show that the entire expression can never become zero. This Dr. Sylvester believes he has the means of doing, and at the same time of assigning exact limits to the number of solutions in question; but in a matter of so much moment, and of such singular interest, does not wish to express himself in a more decided manner, until he has had the opportunity of subjecting his method to a further rigorous examination.

Royal Astronomical Society, November 17.—Mr. W. Lassell, president, in the chair. The Astronomer Royal showed a drawing of Encke's comet made by Mr. Carpenter of Greenwich; it gave the impression of a somewhat shuttlecock-shaped nebulous haze, with two wings of much fainter light, extending on either side, giving a flattened appearance to the head of the comet. Dr. Huggins made a drawing which coincided in all essential particulars with that of Mr. Carpenter. He thought that he had detected a very minute but distinctly-marked nucleus in the paraboidal-shaped head of the shuttlecock. The whole light of the comet was very faint, but he had succeeded in obtaining its spectrum, which, as in the case of that of Comet II, 1868, consisted of three bands, apparently identical with the bands in the spectrum of the vapour of carbon. The middle band situated near "little b" was much brighter than than the other two, and he was quite satisfied of its identity with the middle bands of carbon vapour; the two outlying bands were much too faint for him to speak with confidence of their identity, but they appeared to correspond. The Astronomer Royal showed a celestial globe, on which he had fixed a small white wafer in the place occupied by the sun, and a piece of white paper cut out to represent the comet. He pointed out that its longer axis was directed almost exactly to the sun, and that its head and nucleus were turned away from the sun. This appears to be the almost universal rule with the smaller class of comets. Unlike the sheep of little Bo Peep they carry their tails before them, and not until their smaller fan-shaped appendages have been well warmed by the sun's rays, do they begin to shoot out large tails in the other direction.—A paper was read by Prof. Grant, in which he pointed out that as early as the year 1852 he had recognised the continuity of a red envelope enclosing the sun, of which the prominences were merely the more elevated portions; he had come to this conclusion from a comparison of the observations made during the total eclipses of 1842 and 1851.—A discussion then followed as to whether there were any permanent markings upon Venus. Dr. W. De la Rue and Mr. Browning affirmed that they often saw spots and other irregularities of surface. The authority of Mr. Dawes, and many other observers of note, was cited to the contrary.—Some careful drawings of the Zodiacal light as seen by Captain Tupman while cruising in the Mediterranean were handed round. It was pointed out by Mr. Ranyard that the axis of symmetry of the light was in many instances greatly inclined to the ecliptic, and that the distance of the node of the axis from the sun was in some instances more than 40°.

BOOKS RECEIVED

English.—The Geology of Oxford and the Thames Valley: J. Phillips (Macmillan and Co.)—Weale's Treatises: Rudimentary Geology: Historical: R. Tate (Lockwood and Co).—Profitable and Ornamental Poultry: H. Piper (Groombridge and Sons).—Ganot's Elementary Treatise on Physics, Experimental and Applied: Translated by G. Atkinson, 5th edition (Longmans and Co.).—Tables of Velocity, Time of Flight, and Energy of Various Projectiles; Bashforth Chronograph (E. and F. Spon).—The Discovery of a New World: G. Thomson (Longmans and Co.)

Foreign.—(Through Williams and Norgate.)—Les Migrations Humaines en Océanie d'après les faits naturelles: Jules Garnier.

DIARY

THURSDAY, November 23.

Royal Society, at 8.30.—On the Behaviour of Supersaturated Saline Solutions when Exposed to the Open Air: C. Tomlinson, F.R.S.—On Experimental Determination of the Velocity of Sound: E. J. Stone, F.R.S.; (1) Second Paper on the Numerical Value of Euler's Constant, &c.; (2) Second Paper on the Numerical Values of ϵ, log ϵ^2, log ϵ^3, log ϵ^5, and log ϵ^{10}, &c.: W. Shanks.

Society of Antiquaries, at 8.30.—On Mediæval Representations of the Months and Seasons: James Fowler, F.S.A.—On some Casts of Ivories from Cologne: Augustus W. Franks.

London Institution, at 7.30.—The Influence of Geological Phenomena on the Social Life of the People: Harry G. Seeley, F.G.S.

FRIDAY, November 24.

Quekett Microscopical Club, at 8.—On the Minute Structure of Tremelloid Uredines: M. C. Cooke.

MONDAY, November 27.

Royal Geographical Society, at 8.30.—Exploration of the Volcanic Districts East of Damascus: Capt. R. F. Burton.—Journey in Southern Arabia: Baron de Maetzan.

London Institution, at 4.—Smell, Taste, and Touch: Prof. Huxley, LL.D., F.R.S. (Course on Elementary Physiology).

WEDNESDAY, November 29.

Society of Arts, at 8.—On Tramways and their Structure, Vehicles, Haulage, and Uses: W. Bridges Adams.

Archæological Association, at 8.

THURSDAY, November 30.

Royal Society, at 8.30.—President's Address.

Society of Antiquaries, at 8.30.

London Institution, at 7.30.—Science and Commerce, illustrated by the Raw Materials of our Manufactures. (II.) P. L. Simmonds, F.R.C.I.

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THURSDAY, NOVEMBER 30, 1871

ARCTIC EXPLORATION

In 1865 Captain Sherard Osborn proposed an exploration of "the blank space around our Northern Pole," by a route which he and his brother Arctic explorers, from considerations based on the history of the subject during three centuries, and on their own experience in the ice, were convinced was the best, and the most sure to lead to useful scientific results.

Their reasons for adopting the views then set forth, the correctness of which has since been confirmed by Swedish and German explorers, were as follows :—

The immense tract of hitherto unvisited land or sea which surrounds the northern end of the axis of our earth, is the largest, as it is the most important field of discovery that remains for this or a future generation to work out. The undiscovered region is bounded on the European side by about the 80th parallel of latitude, except where Parry, Scoresby, and a few others have slightly broken through its circumference; but on the Asiatic side it extends south to 75° and 74°, and westward of Behring's Strait our knowledge is bounded by the 72nd parallel. Thus in some directions it is more than 1,500 miles across, and it covers an area of upwards of 2,000,000 square miles, with the North Pole towards its centre. Unlike the ocean-girt region of the Southern Pole, the northern Polar region is surrounded, at a distance of about 1,000 miles from its centre, by three great continents, while the glacier-bearing mass of Greenland stretches away towards the Pole for an unknown distance. There are three approaches by sea to this land-girt end of the earth, namely, through the wide ocean between Norway and Greenland, through Davis Strait, and through Behring's Strait. One wide portal and two narrow gates.

It was through the wide portal that men naturally sought, in the first instance, to reach the mysterious region of the Pole; and they continued to persevere in that direction until experience had taught those who were capable of learning from it that, as in other cases, the longest way round was the shortest way home. The first true Arctic voyager was William Barents, who sailed from the Texel in 1594. He discovered all we now know respecting the Spitzbergen seas; first, the open lane of water which almost always enables vessels to sail up the western side of that land; second, the impenetrable Polar pack to the north, and between Spitzbergen and Novaya Zemlia; third, that the young ice formed in the early autumn and rendered the sea unnavigable; and, fourth, that winds and currents caused open water even in the winter and early spring, but again drove the ice upon the coast at every change of wind. Hudson, in two voyages, explored the whole of the pack-edge from Greenland to Novaya Zemlia, and found it to be impenetrable; and many others followed him with the same result. In later years four expeditions sailed up the west coast of Spitzbergen beyond the 80th parallel, and Dutch and English whalers collected a vast mass of information, which has been ably brought together by Scoresby and Jansen, and which pretty well exhausts the subject.

During the winter and early spring the ice extends in a line from the east coast of Greenland to the northward of Jan Mayen Island, crossing the meridian of Greenwich between the 71st and 72nd parallel, then passing up north for several degrees, and leaving a deep bay, and finally stretching away to Novaya Zemlia. The deep bay in the ice, left to the eastward of the Greenwich meridian in the winter, is probably caused by the so-called Gulf Stream. It forms the route by which the whalers proceed to their fishing-ground, and is known as "the whale-fisher's bight." In the spring the Polar pack begins to drift to the southward and westward, so that the western or lee sides of large masses of land, such as Spitzbergen, are usually left with open navigable lanes of water; while the eastern or weather sides are generally close packed with ice. The pack, consisting of vast fields of thick ribbed ice, has never been penetrated, though whalers annually sail through streams of lighter floes until they reach its edge. The Polar pack is met with in different parallels according to the season and the meridian. Between Spitzbergen and Novaya Zemlia it is usually in 75° or 76°; but occasionally vessels have reached as far as 81° without encountering it, and in the very exceptional year when Parry attempted to reach the Pole, he was only coming in sight of it at his extreme point in 82° 43′, although he had been travelling for 92 miles over closely-packed floes of ice through which no steamer could have forced her way. In another exceptional year, that of 1806, Scoresby sailed along the edge of the pack for 300 miles, between the parallels of 81° and 82°; and at his extreme point in 81° 30′, on the meridian of 19° E., the margin of the ice trended to E.N.E., while to the eastward there was an open sea to the horizon, with no ice blink. Farther east a latitude of 82° or even 83° might possibly have been attained in that year, before arriving at the edge of the Polar ice. Analogous conditions of the ice were found by James Ross in the Antarctic sea. He sailed through pack ice met with in the 62nd parallel, which was drifting north, and then reached the edge of the impenetrable Polar pack which he found extending for 400 miles in a wall 150ft to 180ft. high in the parallel of 78° 30′ S. In the northern sea the Gulf Stream flows up until it meets the ice-laden Polar current between Spitzbergen and Novaya Zemlia. It keeps the ice off the shores of Norway and Lapland, but the parallel on which the warm current meets the ice-bearing stream, and is cooled down to 27°, varies in different seasons. Even if it were possible, by extraordinary luck, to force a steamer through the pack to the open water supposed to be left by its southerly drift, the autumn would be so far advanced by the time she reached it that young ice would be forming on the surface, and all navigation would be at an end. In 78° N. ice forms on the sea during eight months in the year, and Scoresby often saw it grow to a consistency capable of stopping the progress of a ship, even with a brisk wind blowing.

These facts, the results of thousands of observations extending over many years, proved that an attempt to force a vessel through any part of the Polar pack between Greenland and Novaya Zemlia was not the best way to explore the unknown region of the north.

Sir Edward Parry was the discoverer of the true method of Polar exploration, by sledge travelling. He proposed to attempt to reach the North Pole, in 1827, by travelling with sledge boats over the ice to the north of Spitzbergen; and he actually reached the farthest northern point that has yet been attained by civilised man. But the rainfall was exceptional that year; and the ice was in a very unfavourable condition. It was not until he reached 82° 43′ N. that he descried the strong yellow ice blink overspreading the northern horizon, and denoting the vast ice fields over which he hoped to travel. His provisions then only sufficed to take him back to his ship, and he was obliged to return. He made a mistake in the route and in the time of year; but he has the credit of having been the pioneer of Arctic travelling, and of having pointed out the true way of exploring the unknown Polar region.

In deciding upon the best route, Sherard Osborn had his own great experience in the ice, and the recorded observations of Parry and Ross, and of generations of previous explorers to guide him. The first Arctic canon is, "Never take the pack if you can possibly avoid it, but stick to the land floe." The second is, "Reach the highest possible parallel in your ship, and then complete the exploration by sledge travelling." A glance at a Polar chart will show that the first canon can only be followed by passing up the west coast of Spitzbergen, or the west coast of Greenland. But the Greenland coast reaches a higher parallel than that of Spitzbergen. Therefore the Greenland coast is the route to follow,—up Smith Sound and Kennedy Channel to the farthest point attainable. A vessel can almost always reach Smith Sound in one season, for the same reason that a vessel seldom finds it difficult to sail up the west coast of Spitzbergen, namely that she is to windward of the ice. She sticks to the land floe and lets the pack drift past her. Out of thirty-eight exploring vessels that have gone up Baffin's Bay from 1818 to 1860, only two have failed to reach the open water at its head which leads to Smith Sound, before the navigable season was over. From the position that may thus always be reached by an exploring ship, sledge parties could be despatched to the North Pole and back—a distance of 968 miles—a distance often exceeded by the Arctic sledge travellers in search of Franklin; as well as to complete the exploration of the northern coast of Greenland, and of the land to the westward. Such was the plan proposed by Osborn in 1865. It was feasible; it promised useful scientific results; it ensured a vast accession of new geographical knowledge; and the Government could scarcely have refused to adopt it if there had been unanimity in the counsels of geographers nd explorers.

But a fatal apple of discord was thrown into their midst by the eminent geographer of Gotha; and the Admiralty seized on this want of unanimity as an excuse for postponing indefinitely the consideration of the subject. Dr. Petermann has done serious injury to the cause of Arctic exploration by thus forcing his theories into notice at a time so extremely inopportune. It was in 1852 that he first brought forward the theory that there is an open navigable sea between Spitzbergen and Novaya Zemlia leading straight to the Pole especially late in the autumn. He assured the Admiralty that the *Erebus* and *Terror* were somewhere near the Siberian coast, and that they could be reached without serious difficulty by this wonderful route. Had he been listened to, and had our gallant countrymen been then alive, it makes one shudder to think of the consequences if the searchers had thus been led off the true scent. That time no harm was done. But in 1865 Dr. Petermann found more willing listeners. He again declared that the sea between Spitzbergen and Novaya Zemlia was the easiest and most navigable entrance to the unknown region; and he added two new discoveries; first, that Parry, at his farthest point, found a perfectly navigable sea extending far away to the north; and second, that Smith Sound is a *cul de sac* (of which he published a map), and unconnected with the Polar Ocean. The first discovery is surely a dream, for Parry himself saw a strong ice blink overspreading the northern horizon at his farthest point. The second exists only in Dr. Petermann's imagination, and, before he announced it, he should have called to mind the fate of a certain range of mountains named after the late Mr. Wilson Croker. The only tangible grounds for believing in an open Arctic ocean navigable to the Pole, are that the Russian explorers Hedenstrom, Anjou, and Wrangell, saw patches of open water and rotten ice off the northern coast of Siberia in March and April, and that Dr. Kane's ship's steward reported having seen a wide extent of open water in June to the north of Smith Sound. The Russian *polynias* or water holes are in all probability caused by winds and currents acting on a shallow sea, and, so far as we yet know, they are merely local. The same thing was observed by Barents off Novaya Zemlia in November, and an off-shore wind will carry the ice from the head of Baffin's Bay at all seasons. But this does not make the sea navigable. The open water of Dr. Kane's steward in June was only what might be expected at that season, though Dr. Hayes found the same spot covered with ice within a few days of the same time of year, in 1861. Dr. Petermann's arguments unfortunately had the effect of destroying that unanimity, without which it was hopeless to attempt a successful representation of the importance of Arctic exploration at the Admiralty.

The ostensible reason given by the Duke of Somerset for postponing the question, was in order that the results might be learnt of a Swedish expedition then engaged in exploring Spitzbergen, under the direction of Professor Nordenskiöld. Those results fully confirmed the correctness of Sherard Osborn's views. Nordenskiöld reported that no vessel could force its way through the closely-packed ice north of Spitzbergen; but that the ice moves, after long southerly winds, considerably to the north. "All experience seems to prove," adds Nordenskiöld, "that the polar basin, when not covered with compact, unbroken ice, is filled with closely-packed, unnavigable drift-ice, in which some large apertures may be found; though in favourable years it may be possible to sail a couple of degrees north of the 80th parallel in September or October."

Dr. Petermann has since promoted the equipment of Arctic expeditions, which were expected to prove his theory, and to disprove the opinions of Captain Osborn. But he has sent prophets to curse his opponent, and behold, they have blessed him altogether! In 1868 the first German Arctic Expedition sailed under the command of Captain Koldewey, with instructions to penetrate as far north as possible along the east coast of Greenland, or to try to reach Gillis Land, east of

Spitzbergen. They made four attempts to press through the ice, and failed, as all their predecessors had failed. But it is stated by German writers that this expedition attained the highest point ever reached by a sailing vessel, namely, 81° 5′ N. This is a mistake. Parry reached 81° 5′ N. in the *Hecla*, and 81° 13′ in his boats, and Scoresby reached 81° 30′ N. in 1806, on board the *Resolution* of Whitby. In 1869 the second German expedition sailed, also under command of Captain Koldewey, with instructions from Dr. Petermann to penetrate through the belt or girdle of ice which encircles the open polar basin of his imagination, to winter at the pole, and then to sail across it and explore the Siberian islands. All very easy to write at Gotha! But, as usual, Captain Koldewey was stopped, as all his predecessors had been, by the closely-packed ice, and wintered on the east coast of Greenland, at a part which was visited by Sabine and Clavering in 1823. The German explorers made careful scientific observations, and partly examined a very interesting navigable fiord running into the heart of Greenland. The expedition returned to Bremen in September 1870, and the experience acquired by two seasons in the ice has enabled its talented and energetic commander to orm an authoritative opinion on the best route for north polar exploration. Captain Koldewey, the first German authority on Arctic navigation, fully concurs with Captain Osborn that the way to explore the unknown region is by sending an expedition up Smith Sound.

The other Arctic voyages that have been made since 1865 are of minor importance. In 1869 Dr. Bessels crossed the sea between Spitzbergen and Novaya Zemlia, and met with field ice between 76° and 77° N. in August. Norwegian fishermen named Ulve, Carlsen, and Johannesen, found the Sea of Kara comparatively free of ice in 1869—70, and the latter is said to have sailed round Novaya Zemlia. In 1870 Count Zeil and von Henglin made some useful observations on the east side of Spitzbergen during a yacht voyage, and obtained a sight of the still more eastern Gillis Land. In the present year Lieut. Payer, who served under Captain Koldewey, made a voyage towards the Polar pack, between Spitzbergen and Novaya Zemlia, and he reports having nearly reached the 79th parallel, between the 40th and 42nd meridians east from Greenwich, and again in 60° E., finding open water. But Mr. Smith, an English yachtsman, in the same season, was more lucky or more adventurous. He reached the latitude of 81° 13′ N., the highest that has ever been ever observed on board a ship. Scoresby, indeed, reached an *estimated* latitude of 81° 30′ on May 24, 1806, but his highest *observed* latitude was 81° 12′ 42″ on the 23rd. These voyages merely confirm the observations of Nordenskiöld and earlier explorers, that, though the pack is usually met with, east of Spitzbergen, between 75° and 77° N., it may not be reached in exceptional years until the 81st, or even the 82nd parallel is attained.

Such have been the results of Arctic exploration since Sherard Osborn submitted his proposal in 1865. They fully confirm the correctness of his views; and the best English and German Arctic authorities are now in complete accord. There is, therefore, no longer any reason for postponing the consideration of this question. Six years have been wasted, and the men who were available to lead an expedition in 1865, may be unable to do so now. But the navy of England still abounds in the same stuff that made a Parry, a James Ross, a McClintock, and an Osborn in former years; and it must always be remembered that it is out of young Arctic explorers that Nelsons are formed. The arguments for Osborn's scheme of exploration by Smith Sound are now strengthened by the experience of Nordenskiöld and Koldewey. The same evidence of the important scientific results to be obtained by an Arctic expedition that was produced by the highest authorities in 1865, is forthcoming now. The argument that such enterprises in the pursuit of Science have an excellent effect upon the naval service is as strong now as it was then. We may, therefore, reasonably hope that (the Duke of Somerset's reason for postponing the question having been entirely removed) the Admiralty would take the subject of Polar exploration into favourable consideration, if the scientific societies once more submitted it, with the same arguments as were used six years ago.

C. R. MARKHAM

ORD'S NOTES ON COMPARATIVE ANATOMY

Notes on Comparative Anatomy: a Syllabus of a Course of Lectures delivered at St. Thomas's Hospital. By W. M. Ord, M.B. (Churchill, 1871.)

DR. ORD may be congratulated on having put together this compact, lucid, and well-arranged Syllabus. It is well adapted to serve as a framework, for lecturers on Comparative Anatomy to fill up, and students may also use it to refresh the memory when once stored with more slowly acquired information. The abuse of it will be for men to bolt this condensed extract of scientific food in order to produce it again under examination. The author seems to have foreseen this danger, and not only warns against it, but has been careful to preserve the bald and dry style which ought to repel those who do not know how to use the book as he intends. Still, experience of the way in which Prof. Huxley's "Introduction to Classification" is misused by being literally learned by rote, shows to what ill uses such compendia may be put.

The Syllabus begins with a short summary of the distinctive characters of the organic and of the animal kingdoms, followed by a scheme of classification which follows that of the introduction just referred to. The several animal classes from Protozoa to Mammalia are then treated, so that the arrangement is a zoological one. It would perhaps have been better if the author had devoted less space to the enumeration of the characters of orders and classes, since these are found in other manuals, and if anatomical points of difficulty had been more fully explained. For example, more detailed exposition of subjects like the morphology of the compound Hydrozoa, the development of Echinoderms, and the formation of the placenta, would have been exceedingly valuable. For such an object, however, diagrams are almost essential, and, accepting Dr. Ord's plan, it must be admitted that he has carried it out with a due regard to symmetry. The only subject which the Syllabus appears comparatively to neglect is the difficult but important one of Embryology. The account given of the Annulata and Entozoa is particularly clear and excellent. The following extract is a fair specimen of the author's style and method:—

"CL. BRACHIOPODA.—Solitary bivalves, in which the valves are dorsal and ventral, like the two parts of a cabriolet in relation to the animal within, instead of lateral (wing-like) as in Lamellibranchs. Valves joined by hinge or not; never with elastic spring. When not hinged, the valves imperforate; when hinged, one, the larger, is perforate for the transmission of an anchoring ligament, in the non-hinged the ligament passes out between the valves. The class is divided into two orders or subclasses, —the Articulate and the Inarticulate. The Articulate, of which Terebratula is type, have usually curious shelly processes developed from the inner surface of the imperforate valve for the support of the arms, and have in the adult condition no anus; the Inarticulate, of which Lingula is type, have no arm-supporting processes and have no anus."

The account given of the vertebrate skeleton, and especially of some disputed questions of homology, is not so satisfactory as most other parts of the Syllabus. It may be doubtful whether it is desirable to introduce into elementary lectures the difficult subject of the representatives of the tympanic bones in the lower vertebrata; but if so, it is quite useless for men to learn to repeat the "views" of Owen, Huxley, Peters, Parker, and Humphry, and to assign the right view to the right man, unless they are familiar with the facts of embryology, on which alone a judgment can be formed. Now, whether the incus belongs to the first visceral arch, as here stated (p. 113), or to the second, as is believed by some original observers, makes all the difference as to the correctness or incorrectness of the statements which follow. Again, whatever doubt still remains as to the homologies of the pelvis and shoulder girdle, surely no one who has read Prof. Flower's paper on the subject and his subsequent remarks in the "Osteology of the Mammalia," can accept the correspondence of the pubes with the clavicle. The former may very probably answer to a procoracoid, as Gegenbaur and other anatomists suppose, but its mode of development its position in reptiles, and its relation to the great nerves and vessels of the hind limb, are all conclusive against the homology given in p. 116, and more fully in p. 146. No reason is assigned for the query affixed to the statement (p. 171) that the elephant's placenta is deciduous and zonary, which zoologists have hitherto accepted on the testimony of more than one careful and independent observer. The statement as to the number of the cervical vertebræ in mammalia (p. 172) is not exact. No Cetacean has yet been found in which the full number cannot be distinguished, however much fused together the vertebræ may become. On the other hand, the manati has never more than six, and the same appears to be true of one species of *Cholopus* (not Cholœpus).

No mention is made of the order Dipnoi in the classification of fishes taken from Müller (p. 117), or again in the characters of the orders (pp. 133-135). So remarkable a form as *Lepidosiren* should not have been omitted, even if Dr. Ord accepts the conclusion which Dr. Günther has very lately stated in these columns (vol. iv. Nos. 99 and 100). The new genus *Ceratodus*, now that its anatomy has been so fully investigated, forms no doubt a very complete link between the Ganoids and the Dipnoi, and many zoologists will agree with the classification proposed in the admirable paper just referred to; but books intended for students should scarcely pursue the "latest views" so closely.

In conclusion it is only fair to repeat that these Notes deserve commendation for their general accuracy, and contrast very favourably with some other manuals for students on the same subject. They will, if well used, be valuable to learners, and perhaps still more so to teachers.

P. H. PYE-SMITH

OUR BOOK SHELF

Note-book on Practical Solid or Descriptive Geometry, containing Problems with help for Solutions. By J. H. Edgar, M.A., Lecturer on Mechanical Drawing at the Royal School of Mines, &c., &c., and G. S. Pritchard, late Master for Descriptive Geometry, Royal Military Academy, Woolwich. (London and New York: Macmillan and Co., 1871.)

WHEN our Civil and Military Engineering Examinations are daily making larger demands for geometrical proficiency a new and exceedingly lucid Note-book on Descriptive Geometry comes well-timed. Though much has been done to expand this collateral offshoot of geometrical science since M. Monge, of the Ecole Polytechnique, first started it, the co-ordinative characteristic of a science has hitherto been wanting; it has contained, doubtlessly, all the abstract principles of orthographic projection, but principles, to be available, must be interdependent and derivative. Messrs. Edgar and Pritchard have felt this deficiency, and have done much to remove it Their book, unlike the majority of cheap hand-books, is neither "patchy nor scrappy," but a continuous and coherent whole. "Elementary Explanations, Definitions, and Theorems" come first, followed by twenty-eight problems on "The Straight Line and Plane;" to these succeed Solids, first singly, and then in "Groups and Combinations." In like logical order we next have "Solids with the inclinations of the plane of one face, and of one edge or line in that face given," and then "Solids with the inclinations of two adjacent edges given," and, lastly, in this category, "Solids with the inclinations of two adjacent faces given." So far we have the principles of projection in a much more perfectly co-ordinated arrangement than we have hitherto found them in, and we must say that the mere act of mentally assimilating this interdependence of principles would be wholesome discipline, even if it did not, as it unquestionably does, facilitate each successive step in progress, and, most of all, conduce to an integral entertainment of the subject. Again, as naturally derivable from the consideration of the inclined faces of solids, we arrive at "Sections by oblique planes," and "Developments," or the spreading out in one plane of the adjacent faces of such solids; and, finally, the development of curved surfaces. "Miscellaneous Problems" now have place, and amongst them we notice one from the "Science Examinations" of last year. The sequence of the four next chapters is judicious. "Tangent Planes," "Intersections of solids with plane surfaces," "Intersections of solids with curved surfaces," "Spherical Triangles." A short chapter on Isometric Projection (quite as long as it deserves) ends the work, the authors of which we rejoice to find (in these days of "result-seeking") much more desirous of results actual than results visible, and accordingly, foregoing a somewhat too popular profusion of diagrams, which, while it undoubtedly facilitates the bare apprehension of subject-matter, by no means enforces that comprehension of the subject which attends upon the act of accomplishing a mental diagram for ourselves. In this expression of their conviction the authors, we observe, are at one with Mr. Binns, who, with the same sincerity, and for like reason, resisted the systematic use of models in the teaching of "mechanical drawing."

Messrs. Edgar and Pritchard have produced an inexpensive, but a well-digested, comprehensive, lucid, and typographically attractive *vade mecum*.

On the Constitution of the Solid Crust of the Earth. By Archdeacon Pratt, F.R.S. (Phil. Trans., 1871.)

ANOTHER contribution to a subject on which the author has laboured for many years—never perhaps very brilliantly, but always in the main soundly. Such unmitigated nonsense has been talked on the subject of the thickness of the solid crust of the earth, even by scientific men of real power—generally mere mathematicians, sometimes geologists, rarely indeed physicists—and such extravagant views on the subject are still propounded and defended by men like Delaunay, who have done good work in closely allied questions, that it is really refreshing to read Archdeacon Pratt's paper. Yet its tone is somewhat hesitating, almost apologetic, and he finally arrives at the conclusion that what seems to us to be at least a natural assumption to make at starting (viz., that a level surface may be drawn, not very many miles under the surface of the earth, such that in spite of hills and ocean beds the amount of matter shall be the same in every vertical line between these two surfaces) leads to results not after all very inconsistent with those derived from actual pendulum observations made over the Indian Continent. Sir W. Thomson's bold investigation of the tides in the solid earth, due to elastic yielding, furnishes us with by far the most powerful mode of attacking the general question which has been devised since Hopkins's celebrated suggestion of the information to be derived from precession and nutation; and it is to be hoped that the labours of the Tidal Committee of the British Association will soon furnish, from observation, the data required for its numerical application.

LETTERS TO THE EDITOR

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

Instruction in Science for Women

IN thanking you for the useful account given in your last number of the various attempts being made in different parts of England to improve the scientific education of women, may I give you a few more details of the effort now being made at Cambridge to assist the training of those ladies who live too far from any educational centre to be able to get oral instruction?

Correspondence classes have been formed in some of the subjects selected for the University Examination of Women, and the teachers (chiefly resident fellows of colleges) are attempting to assist the reading of their correspondents by means of advice, examination papers at fixed intervals, and free criticism.

Of course this scheme cannot offer the advantages which the lecture systems of London, Edinburgh, and Cambridge itself afford; but that it does meet a real want in what I may venture to call the "rural districts" is shown by the fact that more than seventy women have joined the scheme within a month. Among the subjects of which you take notice in your article, Mr. Stuart of Trinity has undertaken the higher mathematics, Mr. Hudson of St. John's the arithmetic (how woefully ill-taught in the average girls' school no one but the examiner can appreciate), Mr. Bonney of St. John's the geology, and myself the botany. I should add that it is not at all the wish of the promoters to limit the scheme to possible candidates for the Cambridge examinations, but as far as possible to assist any woman who may be struggling with the difficulty of reading a new subject by herself.

All women who wish to avail themselves of this scheme are requested to communicate with the Hon. Sec., Mrs. Peile, of Trumpington, near Cambridge. F. E. KITCHENER

Rugby, Nov. 25

True and Spurious Metaphysics

DR. INGLEBY is evidently a strategist of no mean order. The appalling suddenness of his totally unexpected personal attack, and the skill with which he has almost made it impossible for me to reply without laying myself open to the charge of Egotism (second only in gravity to a charge of Immorality), shows that he is a good deal more than a mere metaphysician. Of metaphysics anon—meanwhile about mathematicians.

I altogether repudiate the Trichotomy, as Dr. Ingleby gives it. The man is either a Mathematician or a Non-Mathematician. There is no intermediate class. Merely to be able to integrate, to solve differential equations, to work the hardest of Senate-Houes Problems, &c., &c., is *not* to be a Mathematician. To deserve the name a man *must* have some of the creative faculty; must be the Ποιητής, if ever so little. And to be a Creator in this sense it is not necessary that one should have devised a new Calculus. Are Stokes, Thomson, Clerk-Maxwell on the one hand, or Cayley, Sylvester, Clifford on the other mere Experts? Yet there can be no doubt that, in Dr. Ingleby's classification, this is their rank.

As regards Hamilton's having placed Metaphysics higher than Mathematics, I may avail myself of the remark, which I heard not long ago in conversation, that "what Hamilton thus exalted was the Metaphysics of the great thinker (and Mathematician) Kant, not the common Cant of Metaphysicians." The distinction implied in this poor pun is one of enormous importance. For there are Metaphysicians and Metaphysicians. Here I am happy so far to agree with Dr. Ingleby, and I shall dichotomise, but not quite as he proposes.

Metaphysicians A. The genuine article. To this class all men worthy of the name of Mathematicians necessarily belong, as do the higher Physicists, &c., &c., such as Faraday. Hence, of course, Archimedes, Descartes (Cartesius, not *Cartes!*) D'Alembert, Hamilton, &c., &c., appear in the list. Leibnitz was, I fear, simply a thief as regards Mathematics, and in Physics he did not allow the truth of Newton's discoveries; so he does not belong to this class.

Metaphysicians B. The spurious article, which has somehow managed to arrogate to itself the title belonging of right to the genuine one. Test this class by what it has to show "even in the present advanced state of metaphysics" (as Dr. Ingleby has it): what have we but stagnation, ropes of sand, bitter quarrels as to the meaning of unintelligible words, and, above all, complacent pride in being "not as other men" but dwellers in a sublimer sphere? Even Longfellow's idiotic "Youth," who ascends the Matterhorn when "the shades of night are falling fast," carrying a pompous "banner with a strange device," does not so ridiculously contrast with the practical Whymper and Tyndall carrying their ropes and ice-axes, as do the Metaphysicians B with the Metaphysicians A:—the Drones with the Working-Bees.

When I asked for the name of a Metaphysician who was also a Mathematician, it was of course of Class B that I spoke, the class containing Hegel and Sir William Hamilton, Bart. (the former of whom proved that Newton did not understand Fluxions nor even the Law of Gravitation, while the latter asserted that the pursuits of the Mathematician reduce him either to passive Credulity or to absolute Unbelief!), the class which is popularly, and (almost *therefore*) erroneously, known by the name. P. G. TAIT

"Wormell's Mechanics"

I REQUEST to make a few observations upon Mr. Wormell's letter in your last number. I shall refer to the paragraphs he has numbered.

1. It is true that, by a collation of two passages, a really intelligent student might be able to eliminate the error from the first statement in Mr. Wormell's book to which we have taken exception. I consider that such collation should be unnecessary in a text-book.

2. A mathematician would, of course, understand what Mr. Wormell means, however he might disapprove of its logic; but Mr. Wormell writes for beginners, and should state his demonstrations without ambiguity.

4. "Curious" is not the adjective we are tempted to apply to such a blunder as that on p. 112. This has not been corrected in even the second edition of the book, notwithstanding the "schoolboy's" aid.

5. We had read Sec. 71, and consequently made the remark about the block and tackle to which Mr. Wormell objects. We now re-assert that the effect of friction upon the mechanical powers is too important to have been omitted in a book professing to treat of Theoretical and *Applied* Mechanics.

Nov. 25 THE REVIEWER

Solar Halo

THE following description and drawing of a solar halo and mock suns seen on the morning of the 13th inst., by the Rev. J.

A. Lawson, at Brancepeth, near Durham, is so perfectly similar to its appearance as drawn and described to me by another observer at Woodburn, at the same hour on the same morning, about twenty-miles north-west from Newcastle, and about thirty miles from Durham, that its unusually bright appearance near Durham may not impossibly correspond with equally favourable views of it obtained by observers at more distant places. The sky, which remained clear during the day, clouded over towards midnight on the 13th, and the stars were completely hidden during the remainder of the night. A slight rain, which began in the morning, also continued to fall during the day of the 14th, and the sky here remained entirely overcast on that evening until after midnight. Shortly before four o'clock on the morning of the 15th the clouds cleared off, and the appearance of several meteors, one of which was as bright as Jupiter, gave evident signs of the progress of the November star shower. The perfect clearness and darkness of the sky, in the absence of the moon, at the same time gave especial brightness to the meteors and to their phosphorescent streaks. Between four o'clock and the first approach of daylight, at six o'clock, thirty-two meteors were counted, or at the rate of sixteen per hour, of which three were as bright, or brighter, than first magnitude stars, nine as bright as second, six as bright as third, and eight no brighter than stars of the fourth or lesser magnitudes. Twenty-six of these meteors were directed from the usual radiant point in Leo, which on this occasion, although not very well defined, appeared to be approximately close to the star Zeta, in Leo's sickle. About one half of their number left persistent streaks, which sometimes appeared to grow brighter after the meteors had disappeared, and I vainly endeavoured to bring them into the field of view of the direct-vision prisms of a small spectroscope, the duration of the brightest streaks noted scarcely ever exceeding one or two seconds. A very brilliant meteor, casting around a flash like that of lightning, was seen here shortly after nine o'clock on the evening of the 13th (and its appearance was also noted at Woodburn), traversing the north-west sky. These particulars, imperfect as they were, unfortunately, rendered by the cloudy weather, are the only descriptions of the November star-shower which its appearance here has hitherto enabled me to supply.

Newcastle-on-Tyne, Nov. 17 — A. S. HERSCHEL

"I had occasion to be at the station at 8.30 A.M. I then first saw them. The night had been hard frost with a clear sky. The ground was covered with hoar. There was no mist. The sun was intensely bright, but the air was very chilly. I went home and looked at my thermometer in the porch at the north side of my house; it stood at 29° F. I then went to the top of a hill to have a better view. I instantly made a sketch of the phenomenon,

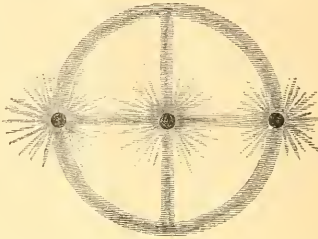
a copy of which I enclose. The lower part of the circle was hidden by a bank of dark clouds. The upper part presented the most marked appearance, and was intensely white. The lump to the north side was more intense in colours than the southern, but both were distinct as to quantity of reflected light. The colours were prismatic, but a bright amber prevailed. The disappearance began at a few minutes before ten, and by five minutes past ten all had cleared away. With the exception of the bank of clouds beneath, there were only a few pencils of cirrus cloud in the sky.

"Brancepeth, Durham, Nov. 13"

Paraselene

IN NATURE, Nov. 9, there appeared the description of a remarkable paraselene observed at Highfield House on the 25th of Oct. A similar phenomenon was seen at Penrith the same night from about 10.30 to 11. As this, however, differed altogether in detail from that observed by Mr. Lowe, I now offer a sketch of what we saw.

Thin mists and white flying scuds travelled across the sky. A luminous ring of perhaps at a guess 150° radius encircled the moon. Within this was a cross of the same brightness as the encircling ring. The bars of the cross were to the eye horizontal



and vertical, intersecting in the moon. Where the horizontal bar cut the luminous ring there were bright patches of light (mock moons), rivalling the moon, as seen through the mist, in brilliancy, but without its defined outline. Where the vertical bar cut the ring there was no increase of brightness. Such a portent in ages gone by might well have filled crusaders with hope, and perhaps thus turned the tide of battle on the morrow. We may make a useful note for future guidance by remarking what followed its appearance in this district. Up to the 25th we had for some time had very fine weatherr After the 25th we had five stormy days of wind and rain. — T. McK. HUGHES

The Solar Parallax

PROF. NEWCOMB wishes apparently to make this discussion as personal as possible. Though I do not intend to follow him in this respect, I must answer him.

He asserts that my abstract of his notes was inadequate; that I "hid the point of the most remarkable of" my "inaccuracies, and ignored the imperfections entirely." This is not so. My abstract was strictly accurate and very much fuller than the utter triviality of his objections warranted. I distinctly stated why I did not discuss the matters which he is pleased to regard as imperfect—his comments being too vague. But this was not ignoring them. His memoranda were not in a state to be printed in full, nor did he even hint that he wished them to be.

As he himself characterises my mistake about his own researches as "the most remarkable of my inaccuracies," it is fortunate that this mistake is also one I am forced to explain at length, owing to the tone Prof. Newcomb has taken respecting it. I certainly did omit a part of Prof. Newcomb's charge; but in his own interest, for it was worded in the very tone to which I now take exception.

In the first place, it is not to be inferred that, because an author comments on such and such a work, he thereby wishes it to be understood that he has himself studied the original memoir in which the work was presented to the world. For instance: many very eminent men have commented on the work of Adams and Leverrier in the matter of Neptune who have not read a line of the original reasoning of these astronomers. That I, of all men (who have expressed something like contempt for memoir-hunting, and have always cared rather to explain methods and describe facts than to write the history of astronomy), should be expected to read every memoir to which I refer, is preposterous in the extreme. It may seem only natural to Prof. Newcomb that when I heard of his having discussed the transit of Venus, I should hurry to obtain his memoir that I might study it *ab initio usque ad finem*; but, as a matter of fact, a paper of the sort, even if placed in my hands, would scarcely tempt me to take up my paper-knife.

Here are the facts of the case.

I read in the *Astronomical Register* a letter which may be called anonymous, if we please, but which was referred by every one who read it to the Astronomer-Royal for Scotland, who showed not the slightest wish to conceal his identity. Doubtless on hearsay evidence (in which, however, he placed, I am sure, as much reliance as I placed in his own statement), Prof. Smyth asserted that Newcomb had anticipated Stone's labours. I took it for granted that it was so, since I saw no room or reason for doubt. There was my error. But, says Prof. Newcomb, whence comes the value 8″·87 "*which it will be noted is Mr. Petrie's pyramid value?*" and on what does Mr. Proctor found his comments "about my treatment of contacts? I am as much in the dark as ever." I will tell him. The value 8″·87 has nothing on earth to do (so far as I am concerned) with Mr. Petrie's pyramid value. It is simply the value insisted upon by Prof. Newcomb in a paper which appeared in the *Monthly Notices* of the Royal Astronomical Society for November 1868; respecting which Mr. Stone remarked (see the same number of the *Notices*) that "the point Mr. Newcomb has raised is a question of only 0″·04, viz. between my value and 8″·87—a question, therefore, of comparative insignificance." Most just remark! With my belief as to Prof. Newcomb's prior work, was it wonderful that I concluded that 8″·87 was his own pet figure for the parallax? Then it chanced that the Royal Astronomical Society, venturing to ignore Prof. Newcomb's objections, bestowed on Mr. Stone, in 1869, the Gold Medal of the Society for his researches into the Venus transit; and in the remarks which accompanied the presentation, it was stated that all preceding researches were imperfect in this respect, that (to use my own words) "no fixed rule had been adopted for interpreting the observations of internal contact." Prof. Newcomb cannot fail to see how this statement accounts for the estimate (not *my* estimate) of his supposed researches.

As a matter of fact, however—apart from the inference to which Prof. Newcomb is so anxious to give point—I am somewhat hardly treated in this matter. When I came to the part of my book where Prof. Newcomb's supposed researches should be dealt with, I thought thus in my mind: "Assuredly Newcomb has done this thing, for Prof. Smyth says so. Shall I leave his researches unnoticed because I can find no trace of them? That would be scarcely fair. Moreover, he is an American, and to omit all notice of his work will be so much the more objectionable. Verily I will repeat the statement of my esteemed friend at Edinburgh, and I will combine with it the weighty judgment of my friends at the council-board of the Astronomical Society. Thus will the researches of Newcomb be recorded, and due credit be assigned to him for his industry and skill, while yet no undue weight will be given to the numerical result of his labours."

That I thus fell into error I have already admitted. But the error is venial in its nature, and utterly insignificant in its effects. As I am conscious that it arose chiefly from my desire (shown in other ways and places) to do justice to our American fellow-workers in science, I am in no way ashamed of it; and I conceive that Prof. Newcomb should have been the last to comment in the manner he has done on the subject.

I shall not follow him in his discussion respecting irradiation, leaving Mr. Stone to deal, in his own good time, with the arguments by which two Continental astronomers (and one American mathematician) have sought to deprive him of his justly-earned credit.

I would submit, in conclusion, that February 1869 (the date of the presentation of the Astronomical Society's medal to Mr. Stone) can scarcely be described as "five years" ago even now, and my treatise on the sun was published in February 1871, Chapter I. being in type in November 1870. Nor has the council of the Astronomical Society (or any member of it) expressed any doubt, as yet, regarding the justice of the decision arrived at in 1869. Yet not a few members of the council have paid marked attention to Prof. Newcomb's attacks upon Mr. Stone. *Verbum sat.*

RICH. A. PROCTOR

Brighton, Nov. 24

The Density and Depth of the Solar Atmosphere

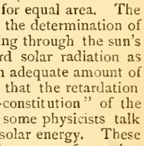
THE demonstration relating to the density and depth of the solar atmosphere, published in NATURE October 5, 1871, page 449, has been entirely misconceived by Mr. Ball. The volume of the terrestrial atmosphere is an element which obviously has nothing to do with the question. Atmospheric air, if raised to a temperature of 3,272,000° Fah., will expand 6,643 times; hence a vertical column forty-two miles high will reach a height of 279,006 miles, if brought to the stated temperature. The basis of computation adopted by Captain Ericsson being an area of one square inch, he shows that a medium similar to the terrestrial atmosphere containing an equal quantity of matter for corresponding area, transferred to the solar surface, will, owing to the superior attraction of the sun's mass, exert a pressure of $14·7 \times 27·9 = 410$ pounds. And that, if the said medium be heated to a mean temperature of 3,272,000° Fah., it will expand to a height of $\frac{279·006}{27·9} = 10,000$ miles above the solar surface. But, if a gas composed chiefly of hydrogen 1·4 times heavier than hydrogen the specific gravity of which is $\frac{1}{14}$ of that of air, be substituted, the height will be $\frac{14 \times 10,000}{1·4} = 100,000$ miles. Admitting that the ascertained coefficient of expansion, 0·00203 for 1° Fah., holds good at the high temperature before referred to, the stated altitudes of the solar atmosphere cannot be disputed. Mr. Ball's announcement concerning the properties of spheres, it is scarcely necessary to observe, has no bearing on the foregoing calculations. With reference to the effect of intense heat, it will be well to bear in mind that the before-mentioned rate of expansion holds good for atmospheric air—within an insignificant fraction—under extreme rarefaction as well as under high temperatures. We have no valid reason, therefore, to suppose that any deviation from the ascertained law of expansion takes place in the solar atmosphere, sufficient to alter materially the before-mentioned computations of its depth.

Mr. Ball, in expressing the opinion that we shall not gain much correct knowledge of the solar atmosphere by the inquiry instituted by Captain Ericsson, forgets that the retardation which the radiant heat suffers in passing through our atmosphere has been ascertained, and that the properties of atmospheric air with reference to temperature and expansion are nearly identical with those of hydrogen, now admitted to be the chief constituent of the solar atmosphere. It is evident that Mr. Ball does not comprehend the object of adopting the terrestrial atmosphere as a means of determining the extent and depth of the solar atmosphere, since he does not perceive that the comparison instituted by Captain Ericsson has brought out the fact that either the depth of the sun's atmosphere exceeds 100,000 miles, or it contains less gaseous matter than the earth's atmosphere for equal area. The importance of this conclusion with regard to the determination of the retardation of the radiant heat in passing through the sun's atmosphere is self-evident to all who regard solar radiation as energy which cannot be absorbed unless an adequate amount of matter be present. Mr. Ball's suggestion that the retardation depends on the "chemical, *i.e.* molecular-constitution" of the solar atmosphere, calls to mind how glibly some physicists talk of "arresting" one half, or more, of the solar energy. These reasoners apparently do not perceive that the means of arresting such stupendous energy is more difficult to conceive than the means of producing it.

Respecting the experiments which have been made with incandescent cast-iron spheres, and inclined discs, it is important to mention that previous experiments had established the fact that the radiant heat of flames transmits equal temperature, under similar conditions, as incandescent cast iron. The inference, therefore, which has been drawn by Captain Ericsson from the results of his experiments with incandescent cast-iron spheres regarding the feebleness of radiant heat emanating from the sun's border is not unwarrantable as supposed by Mr. Ball.

New York, Nov. 10 THULE

An Aberrant Foraminifer



I CHANCED upon an aberrant form of Peneroplis the other day, in which the free terminal series of chambers of this Foraminifer, ordinarily single, is constricted into two distinct tubes.

Though new to me, it may not be so to some of your readers; Dr. Carpenter, however, does not mention it in his monograph.

St. John's College, Cambridge W. JOHNSON SOLLAS

"New Original Observation"

ERNST FRIEDINGER, of Vienna, begins a communication on the subject of "which cells in the gastric glands secrete the pepsine?"* as follows:—"Kölliker erwähnt zuerst das Vorkommen von zweierlei Zellen in den Pepsindrüsen des Hundes." On referring to Kölliker I find, "Bei Thieren sind, wie Todd-Bowman zuerst beim Hunde, *ich* und Donders bei vielen andern Säugern gezeigt haben, die Magendrüsen überall doppelter Art," &c. In Todd and Bowman, published some years before this, the two kinds of glands are figured (the drawings being better than those of Kölliker), the difference between them in anatomical characters, the difference of the two parts of the gland, and the difference in the function discharged by the two kinds of cells of each of the two kinds of glands, pointed out. Friedinger does not even mention the names of the English observers. L. S. B.

* Aus dem lxiv. Bande der Sitzb. der k. Akad. der Wissensch. II. Abth. Oct.-Heft. Jahrg. 1871.

New Zealand Forest-Trees

In your paper of Nov. 9 I observed a letter about New Zealand Forest-Trees, signed by Mr. John R. Jackson of Kew. Mr. Jackson refers to several of the magnificent varieties of forest trees belonging to the natural order of Coniferæ, which are widely distributed in New Zealand; omitting, however, some of the most common and most valuable, especially the Kahikatea or "white pine" of the settlers. This tree affords timber of a white colour, much like yellow deal in appearance and quality, which is admirably adapted for use as weather-board, flooring-boards, and scantling for all in-door work as well as for ordinary furniture. It is most extensively used for all those purposes. The "Totara" is particularly used for making shingles, which form a good substitute for slates as a covering for roofs.

The Rimu is used for such work as requires a more durable wood, and for the making of superior furniture, the wood being much harder and more difficult to work, than that of the Kahikatea, while its beautiful colour renders it very suitable for ordinary cabinet work.

Varieties of the acacia, called Kowai by the natives, supply timber which is specially adapted for the making of pales and fencing, and which is as durable as English oak; and there are many varieties of trees suitable for all purposes.

It is, however, in reference to that which is mentioned as the "Makia" that I think it worth while to trouble you, as I believe that I may be able to suggest what the word so referred to really is. I know of no tree or shrub so called, but Manuka, pronounced Manooka, is the name of the tree from which the natives in former times used to make all sorts of implements, especially the spears, which formed at once the weapons and the sceptres of the chiefs. That hardly deserves to be called a forest-tree, as it rarely attains any great size.

It belongs, I believe, to the family of "Diosma," and its wood is used to make axe-handles, ramrods for guns, &c. The leaves have a pleasant aromatic odour, and an infusion of them forms a passable substitute for tea, to which we were frequently glad to resort in the early times of New Zealand settlements. The fresh twigs form an elastic couch, which constituted our favourite bed on exploring parties and in temporary dwellings.

Braintree, Nov. 20 WILLIAM DAVISON

The Food of Plants

Your reviewer takes exception to my empirical description of carbonic acid in "Notes on the Food of Plants," p. 23. I readily admit—and I should have thought it was unnecessary to do so—that to describe carbonic acid as "carbon dioxide combined with water" is not *strictly* correct; but I think it is much more likely that I should have led my unscientific readers astray, had I explained, in more accurate language, the supposed composition of this acid. CUTHBERT C. GRUNDY

The Germ Theory of Disease

In NATURE, October 5, p. 450, Prof. Bastian, *versus* the Germ Theory, says:—"Such germs when present would be sure to go on increasing until they brought about the death of their host." Now, is it not well known that the larvæ of *Trichina spiralis* become encysted in the muscles of the animal infested by them, and are then perfectly harmless to their host, the fever, sometimes with fatal results, being produced by the migration of the parasites from the alimentary canal through the tissues to their favourite muscles.

Is it necessary, for the support of the germ theory, that the organism must be found in the *blood?*

GEORGE DAWSON

Balbriggan, Ireland, Nov. 20

The Origin of Species

Some months since a letter appeared in NATURE, asking the author of the article on "The Origin of Species," published in the *North British Review*, 1867, to explain the following passage which occurs in the article:—"A million creatures are born; ten thousand survive to produce offspring. One of the million has twice as good a chance as any other of surviving, but the chances are *fifty to one* against the gifted individuals being one of the *hundred* survivors." There is an error in this passage; the word "hundred" should be altered to "ten thousand." I presume that with this correction the writer of the letter will have no difficulty in following the argument. I am much obliged to him for drawing my attention to the slip.

THE AUTHOR OF THE ARTICLE

NEW VOLCANO IN THE PHILIPPINES

The island of Camiguin is situated to the north of Mindanao, at some six or eight miles from the coast, is only a few miles in circumference, and consists principally of high land. On the slopes and in the valleys is grown a large quantity of one of the most important staples of the Archipelago, the well-known Manila Hemp—the fibre of the *Musa textilis.*

On the first of May, 1871, after a series of violent earthquakes, a volcano burst out in a valley near the sea. The earth is said to have swelled, cracked, and then opened, ejecting large quantities of stones, sand, and ashes, but no liquid lava. The mischief done by the eruption was limited to a small area of two or three miles in extent, and the loss of life did not exceed eighty or ninety persons, who might have escaped if they had been less anxious to save their little property.

As the eruption and volcanic disturbances continued for some time, the alarmed natives abandoned the island in great numbers, and took refuge in the neighbouring islands of Mindanao, Bohol, &c., from which, after some weeks, the eruption having subsided, most of them returned. During the month of June the volcano ejected smoke and scoria, which latter are said to have been slowly pushed up as it were out of the crater, sliding down the sides over an underlying mass of fine grey ashes which were thrown out in the first instance; and a feeble action has continued by the latest accounts (August).

The eruption, instead of bursting from the top or sides of the higher hills, occurred in a valley between two spurs of high land near the sea and in the immediate neighbourhood of one of the principal villages, which the inhabitants abandoned, and do not seem disposed to re-occupy, though the damage done there was trifling.

As is usual here, the stories circulated were of the most exaggerated kind, and it is only by sifting and comparing the accounts of reliable eye-witnesses that I have been able to write an account at all worthy of attention. The observations made by two intelligent persons, who visited the island expressly for the purpose, have furnished the materials for this memorandum. The accounts as to the height of the cone are mere guesses—from 300 to 1,500 feet. H.M. surveying steamer *Nassau*, Captain Chimmo, is said to have visited the island in June, and we may therefore hope for a careful and scientific account of this phenomenon.

The present year has been remarkable for the extent and frequency of earthquakes over the whole of the Archipelago, though, with the exception of the case of Camiguin, they were not followed by any very serious consequences.

Manila, Sept. 25 WM. W. WOOD

SPECTROSCOPIC NOTES *

On the Construction, Arrangement, and best Proportions of the Instrument with reference to its efficiency.

THE spectroscope consists essentially of three parts—a prism, or train of prisms, to disperse the light; a collimator, as it is called, whose office is to throw upon the prisms a beam of parallel rays coming from a narrow slit; and a telescope for viewing the spectrum formed by the prisms.

Supposing the slit to be illuminated by strictly homogeneous light, the rays proceeding from it are first rendered parallel by the object-glass of the collimator, are then deflected by the prisms and finally received upon the object-glass of the view-telescope, which, if the focal lengths of the collimator and telescope object-glasses are the same, forms at the focus a real image of the slit, its precise counterpart in every respect except that it is somewhat weakened by loss of light and slightly curved.†

If the focal length of the view-telescope is greater or less than that of the collimator, the size of the image is proportionally increased or diminished.

This image is viewed and magnified by the eye-piece of the telescope.

If now the light with which the slit is illuminated be composite, each kind of rays of different refrangibility will be differently reflected by the prisms, and form in the focus of the telescope its own image of the slit. The series of these images ranged side by side in the order of their colour constitutes the spectrum, which can be perfectly pure only when the slit is infinitely narrow (so that the successive images may not overlap), and accurately in the focus of the object-glass of the collimator, which object-glass, as well as that of the telescope, must be without aberration either chromatic or spherical, and the prisms must be perfectly homogeneous and their surfaces truly plane.

Of course, none of the conditions can be strictly fulfilled. An infinitely narrow slit would give only an infinitely faint spectrum; and no prisms or object-glasses are absolutely free from faults. A reasonably close approximation to the necessary conditions can, however, be obtained by careful workmanship and adjustment, and it becomes an important subject of inquiry how to adapt the different parts of the instrument to each other so as to secure the best effect, and how to test separately their excellence, in order to trace and remedy as far as possible all faults of performance.

With reference to the battery of prisms, several questions at once suggest themselves relative to the best angle and material, the number to be used, the methods of testing their surfaces and homogeneity, and the most effective manner of arranging them.

Angle and Material of the Prisms.—As to the refracting angle, the careful investigation of Prof. Pickering, published in the *American Journal of Science and Art* for May 1868, puts it beyond question that with the glass ordinarily employed an angle of about 60° is the best. For instruments of many prisms there is an advantage as regards the amount of light in making the angle such that the transmitted ray at each surface shall be exactly perpendicular to the reflected. For ordinary glass, the refracting angle determined by this condition somewhat exceeds 60°; for the so-called "extra-dense" flint it is a little less.

The high dispersive power of this "extra-dense" glass is certainly a great recommendation. But it is very yellow, powerfully absorbing the rays belonging to the upper portion of the spectrum, and is very seldom homogeneous. It is so soft also, and so liable to scratch and tarnish, that it can only be safely used by casing it with some harder and more permanent glass, as in the compound prisms of Mr. Grubb, and the direct vision prisms of many makers.

For many purposes these direct vision prisms are very convenient and useful, but they are hardly admissible in instruments of high dispersive power designed to secure accurate definition of the whole spectrum, the violet as well as the yellow.

* By C. A. Young, Ph.D., Professor of Natural Philosophy and Astronomy in Dartmouth College. Reprinted from advance-sheets of the Journal of the Franklin Institute, by permission of the Editor.

† The curvature arises from the fact that the rays from the extremities of the slit, though nearly parallel to each other, make an appreciable angle with those which come from the centre. They therefore strike the surface of the prisms under different conditions from the central rays, and are differently refracted, usually more. The higher the dispersive power of the instrument and the shorter the focal length of the collimator, the greater this distortion, which is also accompanied by a slight indistinctness at the edges of the spectrum.

Test for Flatness of Surface.—For testing the flatness of the prism surfaces, probably the best method is to focus a small telescope carefully upon some distant object (by preference the moon or some bright star), and then to scrutinise the image of the same object formed by reflection from the surface to be tested. Any general convexity or concavity will be indicated by a corresponding change of focus required in the telescope; any irregularity of form will produce indistinctness, and by using a cardboard screen perforated with a small orifice of perhaps ¼ inch in diameter, the surface can be examined little by little, and the faulty spot precisely determined.

Test for Homogeneity.—It is not quite so easy to test the homogeneity of the glass. Any strong veins may, of course, be seen by holding the prism in the light, and if the ends of the prism are polished, the test by polarised light will be found very effective in bringing out any irregularities of density and elasticity in the glass. A blackened plate of window glass serves as the polariser; a Nicol's prism is held in one hand before the eye in such a position as to cut off the reflected ray, and with the other hand the glass to be tried is held between the Nicol and the polariser. If perfectly good it produces no effect whatever; if not it will show more or less light, usually in streaks and patches.

On the whole, however, the method of testing which has been found most delicate and satisfactory is the following:—

A Geissler tube containing rarefied hydrogen is set up vertically, and illuminated by a small induction coil.

A small and very perfect telescope of about six inches focus is directed upon it from a distance of seventy-five or one hundred feet, and carefully adjusted for distinct vision.

The prism to be tested is then placed in front of the object-glass of the telescope with its refracting edge vertical, adjusted approximately to the position of minimum deviation, and telescope and prism together then turned (by moving the table on which they stand), until the spectrum of the tube appears in the field of view. This spectrum consists mainly, as is well known, of three well-defined images of the tube, of which the *red* image, corresponding to the C line, is the brightest and best defined, and stands out upon a nearly black background.

Supposing then the *flatness* of the prism surfaces to have been previously tested and approved, the goodness of the glass may be judged of by the appearance and behaviour of this red image; and by using a perforated screen in the manner before described, inequalities of optical density are easily detected and located. Irregularities, which would hardly be worth noticing in a telescope object-glass, where the total deviation produced by the refraction of the rays is so small, are fatal to definition in a spectroscope, especially one of many prisms, and it is very difficult to find glass which will bear the above-named test without flinching. Of course it must be conducted at night, or in a darkened room.

Number and Arrangement of Prisms.—The number of prisms to be employed will depend upon circumstances. If the spectrum to be examined be faint, and either continuous or marked with dark lines, or by diffuse bands, either bright or dark, we are limited to a train of few prisms.

The light of the sun is so brilliant that, in studying its spectrum, we may use as many as we please. The light is abundant after passing through 13, and I presume would still be so if the train were doubled.

Spectra of fine well-defined *bright lines* also bear a surprising number of prisms. The loss of light arising from the transmission through many surfaces is nearly, if not quite, counterbalanced by the increased blackness of the background, and the greater width of slit which can be used.

As to the best arrangement for the prisms, this also must be determined by circumstances.

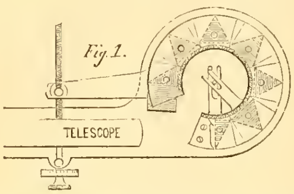
Where exact measurements are aimed at, as, for instance, for the purpose of ascertaining the wave-length of lines, or the dispersion co-efficient of a transparent medium, the prism or prisms ought to be firmly secured in a positive and determinable relation to the collimator. A train of many prisms can hardly be safely used in such work on account of the difficulty in obtaining this necessary fixity, and if high dispersion is indispensable, it can only be obtained by enlarging the apparatus.

But for most purposes it is better that the prisms, instead of being fixed, should be mounted upon some plan which will secure their automatic adjustment to the position of minimum deviation.

Having now thoroughly tried the plan which I proposed and published in this Journal last November, I am prepared to say that I cannot imagine anything more effective and convenient.

The arrangement of Mr. Browning and its extension by Mr. Proctor, are equally effective so far as the adjustment of the prisms is concerned, but are less compact and simple, and do not afford the same facility in changing the number of prisms in use.

In my instrument the light, after leaving the collimator, falls perpendicularly upon the face of a half-prism, passes through the train

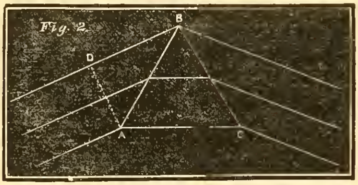


of prisms near their bases; at the end of the train is twice totally reflected by a rectangular prism attached to the last of the train (which is also a half prism), is thus transferred to the upper story of the train, so to speak, and returns to the view-telescope, which is firmly attached to the same mounting as the collimator and directly above it. Both are immovable, and the different portions of the spectrum are brought into view by means of the screw, which acts upon the last prism, and through it upon the whole train. The adjustment for focus is by a milled head, which carries the object-glasses of both collimator and telescope in or out together. Since they have the same focal length, this secures the accurate parallelism of the rays as they traverse the prisms.

The annexed diagram, taken from the paper already alluded to,* exhibits the plan of the arrangement, and requires no explanation, unless to add that, to avoid complication in the figure, I have represented only two of the radial forks which maintain the prisms in adjustment; also, that the prisms are connected to each other at top and bottom, not by hinges, but by flat springs, preventing all shake.

By adding another tier of prisms and sending the light back and forth through a third and fourth story, the dispersion can be easily doubled with very small additional expense, except for the prisms themselves; the mechanical arrangements remaining precisely the same.

I desire, in this connection, to call attention to the great ad-

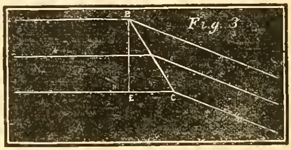


vantages gained by the use of the half prism at the commencement of the train, a point which hitherto seems to have escaped notice.

With a prism of 60°, having a mean refractive index, μ, 1·6, and placed in its best position, the course of the rays is as shown in Fig. 2. The side *a b* is just $1\frac{2}{3}$ times the cross section, *a d*, of the transmitted beam. In other words a prism of the same material and angle described, in order to transmit a beam one inch in diameter, must be one inch high and have sides $1\frac{2}{3}$ inches long.

But when the light is received perpendicularly upon the face of a half prism, as in Fig. 3, then, since $bc = be \div \cos 30°$, the length of the prism side, *b c*, requires to be only 1·155 times as great as the diameter of the transmitted beam.

Thus a train of prisms each 1 inch high, and having the sides of their triangular bases each 1·155 inches long, led by an initial half prism in the way indicated, would transmit a beam 1 inch in diameter, while without the initial half prism the sides would



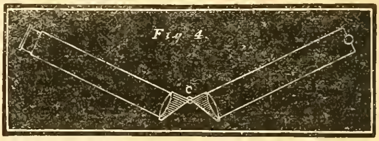
have to be 1·667 long, the surface to be worked and polished would be 1·44 (*i.e.* 1·667 ÷ 1·155) times as great, and the quantity of glass required 2·08 (*i.e.* $1{\cdot}44^2$) times as great. With a higher index of refraction the gain is still greater.

This advantage of course is not obtained without losing the dispersive power of one half prism. But where the train is extensive this loss is comparatively insignificant, and may be made up by a slight increase of the refracting angles. Indeed, in an instrument of the form above described, it is necessary, if the train is led by a *whole* prism, to reduce the refracting angle from 60° to about 55°, in order that the reflecting prism at the end of the train may not interfere with the collimator, while with the initial half prism the full angle of 60° can be used, so that in this case there is practically no loss whatever.

It would seem to deserve consideration, whether in the construction of spectroscopes to be used with some of the huge telescopes now building, it may not be advisable to carry the principle still further, by employing *two* or more half prisms at the beginning of the train in order to economise material and weight.

Dispersive Efficiency.—The dispersive efficiency of the spectroscope is its ability to separate and distinguish spectral lines whose indices of refraction differ but slightly; it is closely analogous to the *dividing power* of a telescope in dealing with double stars. It depends* not only upon the train of prisms, but also upon the focal lengths of the telescope and collimator, the width of the slit, and the magnifying power of the eye-piece.

As has been said before, each bright line is an image of the



slit whose *magnitude*, referred to the limit of distinct vision, depends upon the telescope and collimator, but is independent of the prism train. The *distance* between the centres of two neighbouring lines, on the other hand, depends upon the number and character of the prisms, the focal length of the telescope, and the magnifying power of its eye-piece, but is totally independent of the collimator.

In order that two lines may be divided, it is necessary that the *edges* of their spectral images should be separated by a certain small distance—a *minimum visibile*, whose precise value is of no particular importance to our present purpose, but which I suppose to be about $\frac{1}{500}$ of an inch.

* After the appearance of the article referred to, I found that Mr. Lockyer had anticipated me by some months, not only in respect to the method of making the rays traverse the prism train twice, but also in the use of a half prism at the beginning of the train, and the employment of an elastic spring in the adjustment for minimum deviation. In all essential particulars his instrument is the same as mine, though in some matters of detail there are differences which have proved to be of practical importance in favour of my own.

Mr. Lockyer has, however, never printed an account of his instrument, and at the time of my publication I knew only the fact (which I then mentioned), that he intended to send the light twice through the prism train by a total reflection.

The beautiful instrument recently constructed for Dr. Huggins by Mr. Grubb differs mainly in using compound prisms, and in producing the adjustment for minimum deviation by an arrangement of link work, which, though not theoretically exact, is practically accurate.

* It is very common to describe the dispersive power of a spectroscope as being equivalent to a certain number of prisms, or a certain number of degrees from A to H. But either method fails entirely to convey an idea of the appearance of the spectrum in the instrument, and it is much better to name the closest double line which it will divide, or else to give the distance between the two D lines, either linear (referred of course to the limit of distinct vision), or angular. If we know, for example, that the D lines are separated 1', or, what comes to the same thing, appear to be one-sixth of an inch apart, we have a definite idea of the power of the instrument.

From these principles it is easy to deduce a formula which will express the dispersive efficiency of a given instrument, and enable us to judge of the effect of variations in the proportion and arrangement of the parts.

Let f be the focal length of the collimator.

f^1 ,, ,, ,, telescope.

m the magnifying power of the eye piece (which is found by dividing the limit of distinct vision by the equivalent focal length of the eye-piece and adding unity to the quotient).

n the number of prisms in the train.

w the width of the slit.

k the *minimum visibile* above alluded to.

d μ, the difference between the indices of refraction for two adjacent lines; and finally

δ, the co-efficient of dispersion for each prism (which, r being the refracting angle of the prism, is given by the equation

$$\delta = \frac{\sin \frac{1}{2} r}{\sqrt{1 - \mu^2 \sin^2 \frac{1}{2} r}}\Big)$$

If, now, we put D for the distance between the centres of the two lines, and b for their breadth, we shall have

$$D = m\ n\ s\ \delta f^1 .\ d\ \mu, \text{ and}$$
$$b = m\ w f^1 \div f.$$

But the distance between the edges of the lines equals $D - b$; and this, for two lines as close as the instrument will divide, must equal k.

Hence $k = m\ n\ \delta f^1 .\ d\ \mu - \frac{m\ w f^1}{f}$. Finding from this the value of d μ, taking its reciprocal as a measure of the dispersive efficiency of the instrument, and calling it E, we get

$$E = m\ n \delta\ \frac{f f^1}{k f + m w f^1} \qquad (1)$$

This formula, in which m, n, and δ appear as simple factors, of course supposes that the perfection of workmanship and intensity of the light are such that there is no limit to the magnifying power and number of prisms which may be employed.

My special object, however, in working it out has been to exhibit clearly what is evident from its last term, the dependence of the dispersive efficiency upon the focal lengths of collimator and telescope.

Differentiating equation (1) with respect to f and f^1, we obtain

$$d\ E = m.\ n.\ \delta\ \left\{\frac{k f^2 (d f^1) + m w f^1 (d f)}{(k f + m w f^1)^2}\right\} \qquad (2)$$

which shows that any increase in either f or f^1 adds to the dispersion. If f increases, both D and b increase in the same proportion, and so, of course, does the width of the interval between the adjacent lines; while every augmentation of f^1 decreases the width of the spectral images without in the least affecting the distance between their centres.

This principle seems to have been often overlooked, and collimators and telescopes of short focus employed when longer ones would have been far better.

In spectroscopes designed to be used for astronomical purposes, at the principal focus of a telescope, there is, of course, no advantage in making the angle of aperture of the collimator much greater than that of the equatorial itself; accordingly a collimator of one inch aperture ought to have a focal length of 10 or 12 inches, or, if special reasons determine a focal length of only 6 inches, then it is needless to make the collimator and view telescope much over half an inch in diameter, and the prisms may be correspondingly small.

If, on the other hand, the focus of telescope or collimator is lengthened for the purpose of securing increased dispersion, object glasses and prisms must also be correspondingly enlarged, in order to transmit the same amount of light.

It is, perhaps, worth noting that when f and f^1 are equal, formula (1) becomes simply

$$E = \frac{m.\ n.\ \delta .\ f}{k + m w}. \qquad (3)$$

Luminous Efficiency.—The extreme faintness of many spectra greatly embarrasses their study, so that it becomes a matter of interest to examine how the different dimensions and proportions of a given instrument stand related to the brightness of the spectrum produced.

It appears to be necessary, for this purpose, to distinguish two classes of spectra, those composed of narrow and well defined *bright lines*, and those which are not, the light being spread out more or less evenly and continuously.

The brightness of a spectrum of the latter kind is evidently directly proportional to the amount of light admitted, diminished by its subsequent losses, and inversely to the area over which it is distributed; similar considerations apply in the first case, only as the lines are exceedingly narrow images of the slit, their brightness, being independent of their distance from each other, is inversely proportional to the length of the lines simply—*i.e.*, to the *width* of the spectrum, having nothing to do with its *length*.

Using the same notation as before, merely adding

i = intensity of source of light.

l = length of the slit.

a = linear aperture of the collimator object glass;

and supposing the prisms and view telescope of a size to take in the whole beam transmitted by the collimator, and that the angular magnitude of the luminous object, as seen from the slit, is sufficient to furnish a pencil large enough to fill the collimator object glass, we shall then have for the quantity of light transmitted to the prisms the expression

$$i\ l\ w \frac{a^2}{f^2}.$$

This is afterwards diminished in passing through the prism train and telescope.

To estimate the precise amount of this loss is very difficult, and the algebraic expression for it is of so complicated a character that it would be of little use to attempt to introduce it into our formula. Calling it S, however (which of course is a function of the number and refracting angle of the prisms, as well as of the optical character of the glass), we may write for the quantity of light effective in forming the spectrum,

$Q = i\ l\ w \frac{a^2}{f^2} - S$. And this expression applies to both kinds of spectra—bright line and continuous.

In the continuous spectrum this light is spread out over an area whose length is the angular dispersion of the train * $\triangle$, multiplied by the magnifying power of the eye-piece and by the focal length of the view telescope, and whose breadth is the width of the spectrum. Putting A for this area, we have

$$A = \frac{l\ m^2\ n.\ \triangle .\ f_1{}^2}{f}.$$

And for the intensity of light in the continuous spectrum, which equals $Q \div A$, we get finally

$$L = \frac{i\ l\ w\ a^2 - f^2\ S}{l\ m^2\ n\ \triangle\ f_1{}^2 f}. \qquad (4)$$

If we neglect the loss of light in transmission, and take $f = f^1$, the formula simplifies itself to

$$L^1 = \frac{i\ w\ a_1^2}{m^2\ n\ \triangle\ f^3}. \qquad (5)$$

Either of these formulæ shows how rapidly the light is cut down by any increase of the dispersive power, whether by adding to the prism train or by enlargement of the linear dimensions of the apparatus.

Our only resource in dealing with spectra of this kind, when the limit of visibility on account of faintness is nearly attained, seems to be either to increase i or a. If the luminous object be a point (like a star) we can do the former by concentrating its light on the slit with a lens; if it be diffuse, like the light of the sky, I know no means for producing the desired concentration, and we can only gain our end by increasing the angular aperture of the collimator.

For the discontinuous bright-line spectrum, the case is quite different. Q, *i.e.* the quantity of light which goes to form the spectrum, remains unchanged, but instead of A the whole area covered by the spectrum we have only to consider its width, *i.e.* the length of the lines.†

* $\triangle = n\left(\mathrm{Sin}^{-1} (\mu_H \times \mathrm{Sin}\ \frac{1}{2}\ r) - \mathrm{Sin}^{-1} (\mu_A\ \mathrm{Sin}\ \frac{1}{2}\ r)\right)$ where μ_A and μ_H are respectively the indices of refraction for the lines A and H; the prisms being supposed to be so mounted as to maintain the position of minimum deviation.

† So long as the opening of the slit is small enough to secure accurate definition of the lines, it is not necessary to take into account either this or the magnifying power as diminishing the brightness of the lines by increasing their breadth, since irradiation alone gives them a sensible width sufficient to render the effect of other causes comparatively unimportant.

We then have $A^1 = \frac{lmf^1}{f}$;

and for the brilliance of the bright line spectrum, we get

$$\Lambda = \frac{Q}{A'} = \frac{ilwa^2 - f^2 S}{lmff^1} \qquad (6)$$

If we neglect S, the loss of light in transmission through the apparatus, and suppose $f = f^1$, this becomes

$$\Lambda' = \frac{iwa}{mf^2} \qquad (7)$$

These formulæ show that with a spectrum of this kind we may, without diminishing the brightness of the lines, increase the dispersive power of our instrument to any extent by increasing its linear dimensions; if we increase the dispersive power by adding to the prism train, the case is different, since S is a function of n, the number of prisms.

New form of Spectroscope.—I close the article with the suggestion of a new form for a chemical spectroscope, which seems to present some advantages in the saving of material and labour as well as of light.

The figure (Fig. 4) sufficiently illustrates it, except that it may be necessary to add that I have not represented any of the many possible convenient arrangements for reading off the positions of lines observed. The centre of motion for the telescope is at c, the collimator remaining fixed.

The half prisms of heavy flint-glass are concave at the rear surface, and directly cemented to the single crown glass lenses, which form the object-glasses of telescope and collimator. There is thus a saving of two surfaces over the common form; and, what is more important, the prisms to fit telescopes of a given aperture are considerably smaller on the face, and can be made from plates of glass of less than half the thickness required by the ordinary construction, a circumstance which greatly reduces the difficulty of obtaining suitable material.

NOTES

WE learn by British-Indian cable that the English Government Eclipse Expedition arrived at Galle on Monday last; all well. The authorities in India and in Ceylon are doing everything they can to assist the party. M. Janssen has gone to the Neilgherries. Mr. Lockyer is in communication with Colonel Tennant. The weather was at that time fine.

PROFESSOR JOHN YOUNG has written to the *North British Daily Mail*, detailing the reasons for the notice of motion which he gave in April last to the General Council of the University of Glasgow, relative to the division of the chair of Natural History in that University. The duties of the chair would render it incumbent on its occupant to teach, if required to do so, Zoology, Comparative Anatomy and Physiology, Geology and Palæontology, Mineralogy, Mining, Metallurgy, and possibly Meteorology. Actually, Professor Young gives instruction in Comparative Anatomy and Geology. He is naturally extremely anxious that he should no longer be called upon to teach subjects which, in the present state of science, it is impossible can be efficiently combined. It is to be hoped that, before long, the University will see the necessity of instituting a separate chair of Geology, as has recently been done at Edinburgh; but where will be found a Sir Roderick Murchison to endow it in so munificent a manner?

AT the second M.B. Examination for Honours at the University of London, Mr. William Henry Allchin, of University College, has taken the Scholarship and gold medal, and Mr. Henry Edward Southee, of Guy's Hospital, the gold medal in Medicine; Mr. Richard Clement Lucas, of Guy's Hospital, the gold medal in Obstetric Medicine, and Mr. Ernest Alfred Elkington, of the General Hospital, Birmingham, the gold medal in Forensic Medicine. At the second B.A. and second B.Sc. Examination, Mr. Thomas Olver Harding, of Trinity College, Cambridge, obtained the Scholarship in Mathematics and Natural Philosophy. No gold medals were awarded in Animal Physiology, Chemistry, Geology and Palæontology, or Zoology.

MR. LAZARUS FLETCHER, of the Manchester Grammar School, was on Saturday last elected to the vacant scholarship at Balliol College, on the foundation of Miss H. Brakenbury, for the encouragement of the study of Natural Science. Mr. Hainsworth, of the same school, and Mr. Greswell, of Louth School, were also mentioned by the examiners as worthy of commendation. The scholarship is worth 70*l*. a year, and is tenable for three years.

WITH reference to the destruction of the Museum at Chicago, we learn that Dr. Stimpson's own collection of North American shells formed part of the Smithsonian Museum; and that the collection made by Professor Agassiz and Count Pourtales, in their deep-sea explorations of the Gulf of Mexico, belonged to the Cambridge Museum. Many of Dr. Stimpson's MSS. and drawings have been published. Mr. Gwyn Jeffreys was, as our readers are aware, fortunately the means of saving some of the shells from the Gulf of Mexico, which he is now engaged in working out before returning. Many valuable specimens which Mr. Jeffreys took to Chicago of course shared the fate of the remainder; some of them, however, he hopes to be able to replace. Professor Agassiz has offered Dr. Stimpson a place at Cambridge, Mass., and to give him the means of again carrying on dredging operations in the Gulf of Mexico.

A FINE young pair of the Grey seal (*Halichœrus grypus*) has just been added to the Zoological Society's living collection. This species, although not uncommon on some parts of the British coast, has never previously been received alive by the Society. The present specimens were obtained near St. David's in South Wales, where this seal is said to be of not unfrequent occurrence. Besides this seal, the Society's collection also contains examples of three other Phocidæ—namely, the sea-lion (*Otaria jubata*), the Cape eared seal (*Otaria pusilla*), and the common seal (*Phoca vitulina*).

IN the Northern United States the richest marine fauna is to be found in the vicinity of Eastport, Maine, the adjacent region of the Bay of Fundy having become classic ground through the labours of Stimpson, Verrill, Packard, Morse, Webster, Hyatt, &c. It is rumoured, according to *Harper's Weekly*, that Mr. J. E. Gavit, of New York, president of the American Bank-note Company, and at the same time an eminent microscopist, has it in contemplation with some friends to erect a building at Eastport, to be suitably endowed and maintained for the use of any naturalists who may wish to avail themselves of the facilities it may afford. We can only hope that so excellent an idea may be realised at an early day.

THE latest advices from Captain Hall's expedition were dated at Upernavik, September 5, being somewhat later than the information brought back by the *Congress*. After parting with the *Congress* at Disco, Captain Hall sailed nearly north until he reached the harbour of Proven, where he landed and endeavoured to obtain dogs. In this, however, he was not very successful, procuring only eighteen, most of which were not well fitted for service. From Proven the *Polaris* proceeded to Upernavik, arriving there on the 30th of August. He left that port on the 5th of September, and continued on his polar journey.

AMONG the movements of naturalists abroad, we understand that Mr. J. Matthew Jones, F.L.S., President of the Nova Scotian Institute of Natural Science, intends spending the winter months in the Bermudas, for the purpose of more thoroughly investigating the marine zoology of the group.

MESSRS. WESTERMANN, of Brunswick, announce for early publication, in two volumes, a rendering into German, by Herr Schellen, of the French translation of P. Secchi's "Le Soleil."

THE *Feuille des Jeunes Naturalistes*, to which we called attention some time ago, has entered on its second year of existence in a somewhat enlarged form. Aiming at the development of an intelligent love of nature amongst French schoolboys, it claims the sympathy of all those amongst ourselves who, by means of school museums and natural history societies, are labouring in the same field. The editor solicits contributions from English boys, on any subject connected with natural science, which he promises carefully to translate and publish.

ON the 5th of January, 1872, will be published, in Bombay the first number of a monthly journal, the *Indian Antiquary*, intended as a medium of communication between Oriental scholars in India, Europe, and America, and a repertory for information on the Antiquities, History, Geography, Literature, Religion, Mythology, Natural History, Ethnography, and Folklore of India and adjoining countries, and thus embracing a similar variety of subjects to the English *Notes and Queries*, the plan of whieh the *Indian Antiquary* will, to some extent, follow. The most eminent Orientalists in India, Europe, and America, it is expected, will become contributors to the pages of this journal, and it will be edited by Mr. J. Burgess, M.R.A.S., F.R.G.S. The London agents will be Messrs. Trübner and Co.

WE have received the first number of "The Garden," a weekly newspaper, edited by Mr. W. Robinson, F.L.S. It contains original articles by the editor and other correspondents on gardening topics, illustrated by wood-cuts, instructions for gardeners suited to the time of the year, descriptions of new plants, &c.

MR. W. F. DENNING, the Honorary Secretary of the Observing Astronomical Society, publishes "Astronomical Phenomena in 1872," a complete guide to the astronomer for the more important phenomena to be looked for during the year.

MR. ROTHSCHILD, of the Rue des Saints Pères, Paris, has commenced publishing, in large folio numbers, a magnificent work upon the Trajan Column at Rome. A complete series of mouldings was executed in 1862, by order of the Emperor, for the Louvre Museum. A cast was taken of these mouldings in galvano, by the Procédé Oudry, and from these casts phototypographic plates have been done. There will also be many woodcuts interspersed through the work. The letterpress will be by M. W. Frochnor, the conservator of the Louvre Museum. It will be finished in 1873.

MR. CUTHBERT COLLINGWOOD, M.A. and B.M., Oxon, F.L.S., &c., author of "Rambles of a Naturalist on the Shores and Waters of the China Seas," &c., announces, as in the press, "A Vision of Creation," a poem, with an introduction, geological, and critical.

PROF. HUXLEY, in his address at the distribution of prizes at the Oxford Local Examination at Manchester, spoke as follows: "He believed that he was speaking entirely within measure when he said now that there was nowhere in the world a more efficient or better school, so far as it went, for teaching the great branches of physical science than was at the present time to be found in the University of Oxford. He thought it right that he should here state what had come to his knowledge as a member of the Royal Commission connected with these matters. That noble University had within the last ten or fifteen years devoted no less than about 100,000*l.* to the endowment and equipment of physical science and physical science only.

M. JOLY, a distinguished member of the French Academy of Medicine, has recently read a paper before that learned society, in which he attributes the enervation of the nation, as evinced during the late war, to the combined effect of alcohol and nicotine upon the national character. "Tobacco," says Dr. Joly, "although of only recent introduction, has gained upon its older rival. Imitativeness and 'moral contagion' have done their work, until the use of this poison has penetrated everywhere—has enslaved the nation, caused personal and racial degeneracy, enervated the entire army, and made it slow to fight and powerless in action. The use both of spirits and tobacco has frightfully increased, and human depravity could scarcely devise a worse compound than the mixture of brandy and tobacco, which is the latest liquid novelty patronised by Parisian sensualists. The French consume more tobacco than any other nation."

THE *Gardener's Chronicle* states that a series of photographs devoted to the illustration of Linnean relics has been recently issued in Sweden, and copies are to be procured in London. They consist of photographs of Linné's statue in the Botanical Garden at Upsala, of the Botanical Garden itself, the monument in Upsala Cathedral, his country seat and museum at Hammarby, a portrait, one of his letters, and other objects of interest in connection with the great naturalist.

AN interesting contribution to the supposed "Serpent Worship" in Scotland is stated to have been lately discovered near the shores of Loch Fell, near Oban, where the form of a monstrous serpent three hundred feet in length has been disinterred. From the accounts which have been published it would appear that the figure of the serpent was excavated in the rocks above the lake, and had become overgrown.

SOME interesting experiments have lately been tried at the Crystal Palace to improve the illuminating power of ordinary gas. The inventor, by mixing a certain proportion of oxygen with the gas as it issues from the burner, claims to have found both a more economical and a more wholesome method of burning gas. There is no doubt that the light is much more brilliant, the only question is whether it is not too expensive. The oxygen is generated by passing supersaturated steam over red manganate of soda previously heated in dry air. The steam absorbs the oxygen from the manganate, and on being condensed the oxygen passes over alone and is mixed with the gas at the burner.

THE *Indian Medical Gazette* says that a report furnished by the Inspector of Police to the Bengal Government shows that of 939 cases of snake bites in which ammonia was administered by the police 702 are reported to have recovered, and the average length of time between the bite and the application of the ammonia is said to have been in fatal cases 4h. 12m. 13s, and in cases of recovery 3h. 28m. 14s.

ON the 29th of September a slight shock of earthquake was felt at Memoodabad in the Ahmedabad Collectorate, Bombay.

IT is stated that an aerolite weighing 127lb. fell lately near Montereau (Seine-et-Marne) in France It appears to have come from the east, and burst with a loud explosion, giving a bright blue light. It is of an irregular spheroid shape and black, and is to be sent to the Academy of Sciences.

A VERY violent typhoon raged at Hongkong on the 2nd of September, doing an immense amount of damage both on land and sea.

ON October 16th a terrific hurricane swept over Halifax, New Brunswick, and caused a large amount of damage. It was accompanied by an extraordinary high tide, which was unexampled in the history of the city for damage and violence. On the same and the following day, very heavy storms were experienced on Lake Superior and Lake Huron, which caused the destruction of many vessels and the loss of numerous lives.

IN Ecuador there have been discovered in the forests of Santa Helena the trees yielding the red guinea bark.

AN earthquake took place in the beginning of October on the Isthmus of Chiriqui near Panama.

DR. ROBERT BROWN, in a communication on the "Interior of Greenland," states that all the results of the attempted explorations of the interior serve to show that this is one huge *mer de glace*, of which the outlets and overflow are the comparatively small glaciers on the coast, though when compared with the glacier system of the Alps, they are of gigantic size. The outskirting land is, to all intents and purposes, merely a circlet of islands of greater or less extent. There are, in all probability, no mountains in the interior—only a high plateau, from which the unbroken ice is shed on either side to the east and west, the greater slope being toward the west. No mountains have been seen in the interior, the prospect being generally bounded by a dim, icy horizon. Dr. Brown considers Greenland susceptible of being crossed from side to side with dog or other sledges, provided the party start under experienced guides, and sufficiently early in the year.

OCCASIONAL glimpses of pre-historic times are afforded to us. One of the Indian papers records the deeds of a mad elephant, which made its way from the Rewah territory into the Mundla district. The first day it attacked the village of Tarraj, when the inhabitants took refuge on the roofs, but it killed a woman and child. The next night it went to the village of Mauzah and killed a boy. Two days after it killed a woman at Barbashore, and on the following night added to the number a man and woman at Kamaria. Thence it made its way to Donoria, and the villagers tried to escape, but two old women met their death, and another was trampled on and seriously injured. Its next stage was Manori, destroying a woman and two children, and so to Karbah. Here it snatched a baby from the mother's arms and killed it, and in the evening succeeded in killing a man in the same place. The next night a man was killed at Nigheri, and on that following another at Banu. On the 7th February it met with a check in passing the Ramgurgh Tahsil, where it was fired on, and retreated to Bijori, taking revenge by killing a man and a boy. On the 8th it surprised a party of villagers in the jungle, who had escaped from Nanda, again taking a woman's baby from her arms and killing it. The next slaughter was of a man at Belgaon and another at Belgara. It then visited Sayla, the villagers making their escape, except one boy, who was caught by it, but only rolled about for fun, but the elephant went into the village and pulled down several houses. By the 15th he was at Mohari, and injured a man and woman by rolling them about without killing them. On the 19th it killed one man and wounded another at Narainguuj. By this time a party was got together to resist it, about three weeks having elapsed, and the animal was driven across the river Nerbudda and into the jungle of a hill, but from which the force was inadequate to dislodge it. In three weeks it drove the people out of many villages, killing twenty-one persons, wounding others, and ravaging the country. It is alleged to have devoured five of its victims. The above recital of what took place in a relatively settled country, gives colour to the legends of Hercules and Theseus. In this case nothing is said of the destruction of crops which must have taken place.

AN improvement in the apparatus attached to fire-engines has been proposed by Mr. Prosser in the form of a spreading fire-nozzle, the object of which is, by means of a number of moveable as well as fixed fingers so to direct the jet of water that it shall divide it into a more or less fine spray. The water is thus economised, and instead of a large proportion running off after scarcely coming into contact with the burning material, every drop, falling in the form of a conical shower of rain, performs its part towards extinguishing the fire.

COLDING ON THE LAWS OF CURRENTS IN ORDINARY CONDUITS AND IN THE SEA

II.

FORCHHAMMER has filled up that gap by his researches upon the water of the ocean; for we can now, by the help of his results and of the temperatures, ascertain pretty exactly the specific weight of the water of the ocean in the principal seas of the globe. Calculation has proved the correctness of Maury's original notion, viz., that the density of the water of the ocean is least at the equator, and increases with tolerable regularity in proportion as we advance towards the north and towards the south. The water of the Atlantic seems to be of the greatest density at about 60° N. latitude to the south and south-east of Greenland. If we take this density as unity, the specific weight of the water of the sea will on an average be represented by the following numbers :—

NORTHERN HEMISPHERE		SOUTHERN HEMISPHERE	
Between 60° and 70° latitude in Davis Straits .	0·9980	Unknown	
About 60° latitude in the Atlantic	1·0300	Unknown	
Between 50° and 60° latitude in the Atlantic . .	0·9994	In the Cold Currents of Cape Horn	0·9990
Between 40° and 50° latitude in the Atlantic . .	0·9985	In the Atlantic	0·9984
Between 23° and 40° latitude in the Atlantic . .	0·9972	In the Atlantic	9·9970
Between 0° and 23° latitude in the Atlantic . .	0·9966	In the Atlantic	0·9966

Of these the former, those of the Northern Hemisphere, are most to be depended on, because the observations there have been most numerous.

It will be seen by this table that the density of the water of the ocean increases along with the latitude, and in almost the same proportion both north and south of the equator. But Forchhammer has also determined the saltness of the sea at various depths, and has found that it decreases in very slow proportion with the increase of the depth. If we start from this fact, taking account at the same time of the decrease of temperature in proportion to the depth, we find the result to be that, at 500 fathoms below the surface, the density of the water of the sea over the whole globe may be considered as equal to 1, the difference at any particular point being scarcely discernible. But since the density of the water of the ocean at a depth of 3,000 feet is everywhere equal to 1, and since at the surface it diminishes as we approach the equator, it is evident that the mass of water underneath cannot be in equilibrium; that if the surface of the sea is more elevated between the tropics than under the poles, and if we take the mean densities given above, at the surface, and at the bottom of this liquid mass, we find that the height of the surface of the sea above the level corresponding to the density of 1, ought to be nearly as follows :—

Height	between the	Equator and the Tropics		6·6 feet.
,,	,,	Tropics and 40° lat.	.	4·2 ,,
,,	,,	40° and 50°	,,	2·2 ,,
,,	,,	50° and 60°	,,	0·9 ,,
,,	at	60°	,,	0·0 ,,
,,	between	60° and 70°	,,	3·0 ,,

But a similar difference of level necessitates the formation of a double surface-current passing from the equator to the two poles, and that cannot take place without entailing a diminution of the height of the water under the tropics, unless, indeed, there be an equivalent afflux into the tropical seas. But if the level of the water between the tropics be lower, the equilibrium of the under strata will be destroyed, and there ought, consequently, to be a submarine current which comes both from the north and the south towards the equator. That there really exists a current in that direction is a result of the circumstance that the temperature of the sea decreases with the depth.

Supposing then that there were no other forces in action, the difference of level mentioned above, ought, as Maury at first admitted, to give rise to a surface-current from the equator to the poles, and an under-current from the poles to the equator. But these currents are enormously modified by the intervention of other forces. The north-east trade-winds react against this equatorial current of the northern hemisphere, exercising upon the surface of the sea an oblique pressure, of which the effect is greater than that of the difference of level. There results from this, reckoning from the 30° latitude, a rising of the water in a direction contrary to the liquid masses which the south-east trade-winds tend to draw from the south Atlantic; at the same time the north-east trade-winds force the waters of the surface, as Franklin supposed, to take a south-western direction towards the Carribean Sea. In this sea, and in the Gulf of Mexico, where the trade-winds exercise no influence, the water continues its course to the north by the Strait of Florida, and thus gives birth to the Gulf Stream. But in order to enable the Gulf Stream to advance from the Gulf of Mexico and the Strait of Florida as far as 30° N. latitude, a difference of level is necessitated, which can be calculated by the help of the general formulæ for the movement of water in currents; by this means we find that the level of the water in the Gulf of Mexico ought to be about 6 feet higher than at St. Augustine. If we then observe that in accordance with the density of the water at St. Augustine, the level of the sea ought to be found to be about $3\frac{1}{2}$ feet above the point marked zero, which corresponds to the mean density of 1, it follows that the level of the Gulf of Mexico is about $9\frac{1}{2}$ feet above that point, and that the trade-winds are the means of adding a height of 3 feet to the water of that gulf.

After this immense current—which, in the Strait of Bemini, may be compared to a river delivering at the rate of 1,600,000,000 cubic feet per second—has passed St. Augustine, it pursues its course to the north-east, as has been said above. In order to accomplish this long passage, it has at its disposal, at the most, an incline of $3\frac{1}{2}$ feet; but it is easy to see that the force which results from this is altogether insufficient to accomplish the work which this movement demands, and it evidently follows that the Gulf Stream ought, during all this course, to be subjected to the action of another force, to which hitherto our attention has not been drawn. But what is this force of which we have thus taken no notice? Singularly, it is an old acquaintance, whose function we have not sufficiently understood, although Kepler was the first to announce its importance. In fact, the force which impels the Gulf Stream towards the north is simply that which results from the rotation of the Earth; and it acts not only upon the Gulf Stream, but is, as we shall see, the chief cause of all currents, both atmospheric and marine. That the daily rotation of the earth should exercise an influence upon all currents which go from the equator to the poles and *vice versâ*, and that the direction of the trade winds are due to the same cause, are facts well known. But though it is agreed that this rotation acts upon the currents of the ocean, opinion has hitherto been very much divided as to the importance of the action; some maintaining that the rotation of the earth is the chief cause why the Gulf Stream and the polar currents follow respectively the directions north-east and south-west, while others hold that it cannot cause any change to speak of in the courses taken by the ocean currents, courses which they would continue to follow all the same were there no rotation of the earth. But although there is so much dispute as to this point, every one agrees in acknowledging that we know but little about the matter, and in any case nothing certain of the laws which regulate the movements of the ocean and atmosphere; for we are at present ignorant whether the atoms of water or air move without resistance, or whether they meet and are subject to the action of certain forces, and we know still less about the origin of these forces, their magnitude, &c. This ignorance on the subject of the influence which the rotation of the earth exercises upon the currents is evidently due to the imperfect knowledge which we have of the laws which regulate the movement of fluids in currents; for if we had been able to establish that such a force ought to be in play, we would, without doubt, soon have determined the true expression. The thing is, in fact, very simple; if we suppose that a section of element current flows from the equator in the direction of the meridian in a definite channel, that line will turn with the earth with a speed from west to east $= \frac{2\pi R}{86400} \cos \theta$, θ representing the latitude, and R the radius of the earth. After a time dt, during which the current in question will arise at latitude $\theta + d\theta$, it will act upon the sides of the canal as if it were subjected to a force which, in the time dt, had communicated to it an increase of speed $\frac{2\pi R}{86400}$ sin. $\theta\, d\theta$ from west to east, the line of current being supposed perfectly free. The force which results from the rotation of the earth could then be represented by

$$\psi = \frac{2\pi R}{86400} \text{sin. } \theta \left(\frac{d\theta}{dt}\right) = \frac{2\pi}{86400} \text{sin. } \theta\, v$$

v being the speed in the supposed channel. But the movement not being free, since the material section which we are considering is forced to move in a channel from south to north, it will exercise per unit of mass against the sides of the canal, a pressure ψ directed from west to east. If the section, as we have supposed, forms part of a current compelled to move circularly in a channel, it is evident that the surface of the water will rise from left to right; and if we designate the height by what it rises by h, for a breadth of channel $= l$, we shall have $-g\frac{h}{l} = \frac{\text{sin. } \theta\, v}{13750}$.

The trajectory being the same, it is clear that the surface of the current ought to present the same slope, whether it moves in a channel or flows freely in the middle of the sea. But it is no less evident that whatever be the situation of this trajectory on the surface of the globe, the section which in the time t is found at latitude θ, and after the infinitely small time dt, arrives at latitude $\theta + d\theta$, ought, under the influence of the rotation of the earth, to move in the same manner as if, the earth being immovable, it had been driven from west to east with a force

$$\psi = \frac{2\pi R}{86400} \text{sin. } \theta \frac{d\theta}{dt} = \frac{\text{sin. } \theta. \text{ sin. } w\, v}{13750}$$

where v still represents the speed of the section under consideration, and w the angle which the direction of the trajectory described makes with the eastern part of the circle of latitude. But we can, in consequence, put aside the rotation of the earth, and consider the latter as immovable if to the other forces which act upon the water, we add the force ψ acting from west to east. If we decompose this into two other rectangular forces, one of them following the direction of the current, which, let us suppose, has throughout its course a fall $\frac{du}{d\lambda}$, we find that its surface ought to present from left to right, and perpendicularly to the direction of the current, an elevation $\frac{h}{l}$, whose value is given by the equation

$$(1) \; . \; . \; . \; . \; . \; . \; g\frac{h}{l} = \frac{\text{sin. } \theta \text{ sin }^2 w.\, v}{13750}$$

and that the liquid mass is impelled forward by a force

$$\left[\frac{\text{sin. } \theta \text{ sin. } w \cos\, w.\, v}{13750} + \frac{du}{d\lambda} g\right]$$

which, in accordance with my theory, leads to the following equation of the movement of the current:—

$$(2) \quad u = \frac{V^2 - V_0^2}{2g} + \frac{0{\cdot}016}{3}\,\frac{V^2 + V.\,V_0 + V_0^2}{2g}\,\frac{\lambda}{H} + \frac{\text{sin.}\theta \text{ sin.} w \cos. w}{13750}.\,\frac{V + V_0}{2g}\,\lambda$$

where u is the fall of the current in the length λ. H its depth, V_0 its initial speed, and V its final speed after having run the course λ. In short, if, according to the theory, we place for the delivery of the current per second

$$(1) \; . \; . \; . \; . \; . \; 2 = 0{\cdot}82\, V.\, H.\, \lambda$$

we shall have the fundamental formulas which give the laws of the course of ocean currents over the whole surface of the globe; the angle θ, which is positive in the northern hemisphere and negative in the southern, having its values comprehended between 0 and 90°, while the angle w, following the direction of movement, may be found in the 1st, 2nd, 3rd, or 4th quadrant.

It follows from these three formulas that all the currents of the northern hemisphere, whatever be their direction, have a surface which goes on rising from left to right, and whose progress, the force resulting from the rotation of the earth, accelerates or retards according as they move in the 1st or 3rd, or in the 2nd or 4th quadrant: hence it follows that a movement in one of these later quadrants is possible only when the current possesses a sufficient fall, or an equivalent force, due, for example, to the action of the wind, the specific weight of the water of the sea, &c. When the current follows the meridian, the inclination of its surface, perpendicularly to its direction, is at the maximum; but besides this, the rotation exercises no influence upon its course. When the current flows at right angles to the meridian, the fall $\frac{h}{l} = 0$, and the rotation has, in short, no effect upon its course.

If, then, we consider the Gulf Stream from its exit from the Gulf of Mexico, we see that, in its passage from Bemini to St. Augustine by the Strait of Florida, where it runs directly north, the current is kept up by a difference of level which, as has been stated above, may, for that extent, be estimated at six feet. Throughout this course the current presents from west to east an elevation whose total value is about 1 3 feet.

From St. Augustine to the Bay of New York the Gulf Stream runs towards the north-east; in all this course it is impelled by the rotation of the earth with a force corresponding to a fall of from nine to ten feet, and rises from left to right about 1·2 feet.

From the Bay of New York the Gulf Stream runs eastward towards the shores of Europe, and, throughout the passage, obeys the impulse of the force of rotation, which raises it from left to right by a total elevation of about one foot. Having reached the neighbourhood of Europe, the current divides into two nearly equal branches, one of which, under the influence of the diminished force of the action of the earth's rotation, runs in a south-easterly direction towards the coast of Africa, with an elevation from left to right. The other branch, meanwhile, is forced to skirt the coasts of Great Britain, taking a more northerly direction on account of the resistance it meets with from the land, the action of the force of rotation causing it to advance in its northerly course with an elevation from left to right facing the land of one and-a-half feet. If we try to estimate the influence which the earth's rotation exercises upon the Gulf Stream from St. Augustine to the 60th degree of N. latitude, we find that the force is nearly the same as that which would act upon the current, if, between these two points, a distance of about 950 miles, the Atlantic showed a difference of level of twenty-five feet. When the Gulf Stream has passed the northern extremity of Scotland, the resistance which obliged it to take a more northerly direction disappears, and, from this time, the principal current inclines more to the east towards the coast of Norway, which it then skirts to the north-east, sloping towards the land on account of the earth's rotation. Another branch of the Gulf Stream is arrested by Iceland in its course to the north, and turned to the north-west, striving against the earth's rotation, which elevates it towards the south and south-west coast of the island just mentioned, it ought consequently to present a slope towards the north-west as far as the polar current.

(*To be continued.*)

SCIENCE IN GERMANY*

IN his address at the opening of the present University Session at Berlin, the out-going Rector quoted some interesting figures showing the effect of the recent war on the activity of the University. In October 1870 there matriculated in all the faculties 1,236 students, while the number of entries for the winter session of 1869 was 2,421. Of the 1,236 students who entered their names in October, only 904 continued their attendance throughout the winter. The actual number of medical students last winter was 173, while in the previous winter session they amounted to 550. The falling off in numbers extended about equally to all the four faculties; but it appears that none of the theological students who entered at the beginning of the session were required to break off their studies. The courses of lectures, public and private, that were announced amounted to 366, and of these 271 actually came off. Forty students took their degrees—8 in jurisprudence, 19 in medicine, and 13 in philosophy. The number of deaths, so far as was ascertained, amounted to 32. The University seems now to have returned to its full activity, to judge from the crowded state of many of the class-rooms. A few of the students are to be seen wearing the ribbon of the Iron Cross.

Two ladies from America have applied to the Berlin University authorities for permission to attend the medical classes. One lady, a Russian, is studying chemistry in Prof. Hofmann's laboratory. An American lady has been studying medicine at Breslau, and has sent to an American newspaper a glowing account of her friendly reception at the Silesian University. Another pioneer of the same sex is studying engineering at the Polytechnic School of Aix-la-Chapelle; and two ladies recently joined the University of Prague, where they are studying under the professor of history. During the past summer a solitary American lady, M.D., attended the clinics at the Vienna General Hospital, and appeared to suffer, to the full extent, the inconveniences of being in so considerable a minority.

The autumn season on the Continent, as in England, is marked by the occurrence of various scientific gatherings. At several of these, Prof. Virchow has been receiving invitations, which the Berlin newspapers have chronicled from time to time. At the Assembly of German Naturalists and Physicians, held at Rostock, his speech was the great event of the meeting. During the Bologna Conference of Archæologists, he was entertained at a banquet by the Italian dignitaries and men of science; and at a scientific assembly held in Rome, the audience rose to their feet to welcome the celebrated Berlin professor, who made them a speech in French. In his address to the Rostock Conference, Virchow made some remarks upon the nature of annual scientific gatherings, of which he himself is an assiduous frequenter. "It was a matter of encouragement to me," he said, "when I read in the proceedings of the recent meeting of the British Association, in the opening address of its renowned President, Sir William Thomson, that Brewster, in his letter by which he called the Association into existence, expressly stated that he was led to this step from considering the great and beneficent results that the German Naturalists' Association (*Naturforscherversammlung*) had achieved during its nine years' previous activity. We were the first to advance among all nations; the English followed, and the number of these associations has gradually increased. They have, by degrees, extended into every possible province of human activity, and we have thereby become accustomed, by the co-operation of the many, to define more clearly the common objects at which the whole has to aim." And again, speaking of the results of these meetings, he says: "Not only the pleasures of fellowship, which are inseparable from a great congress of individuals; not only the amenities of personal acquaintance, which cannot be too highly valued; the forming of friendly ties, where perhaps, under other circumstances, harsh and even bitter opposition would have sprung up; the reconciling of many controversial antagonisms through personal intercourse—all this is the smaller result. There is yet a greater—the communication of knowledge, the explanation of methods, the clearing up of the directions in which research should be undertaken—and these are things which can be nowise better told than by word of mouth." The main subject of Professor Virchow's address was the part that science would have to play in the new national life of Germany. Their work, he held, was to introduce into the popular life of the nation the great and all-pervading idea of evolution. Space will not permit even to give an abstract of his views.

Among the books that have issued from the German press within the last month or two are—the new edition of Virchow's "Cellular Pathology," much improved and enlarged; Professor Traube's "Contributions to Physiology and Pathology," in two bulky volumes, one containing experimental and the other clinical researches; a new instalment (the fifth) of Stricker's "Handbuch;" a treatise on Leuchæmia, by Professor Mosler of Greifswald; and an elaborate work with plates, by Barkow of Breslau, on "Dilatations and Tortuosities of the Blood-vessels," with special reference to aneurism of the aorta in its various sites.

* From a Correspondent of the *British Medical Journal.*

SCIENTIFIC SERIALS

THE fourth number of the *Zeitschrift für Ethnologie* for the present year begins with Dr. A. Erman's concluding part of his "Ethnological Observations on the coasts of Behring's Sea." He draws attention to the bold and often successful surgical treatment which was found to have been practised by the Aleutians when they were first visited by Europeans. The influence exerted by the Russians on these primitive people has tended to make them conceal, or even gradually relinquish the practice of many of their old national habits, and, amongst other usages, they have almost wholly given up their heroic surgical operations. Dr. Erman met, however, with one skilled Aleutian operator, from whom he learned many particulars in regard to the native practice of his art. It would appear that their variously-sized lancets are formed of finely-polished and sharply-edged flakes of obsidian. With these instruments bleeding in the leg as well as the arm is performed, and incisions made in various parts of the body, including the thoracic walls, for the purpose of removing blood or pus, in cases of their effusion into the cavity of the pleura, or in pulmonary disease. But although we are told that this practice is not found to be attended with any dangerous results, we are not informed how the injurious effect of any possible admission of air into the chest is guarded against. The Aleutians exhibit great dexterity in removing various parts of the bodies of whales, and of sea-lions and other seals which they have killed, as, for instance, the mucous membrane of the neck, without in any way injuring the contiguous parts. And they show wonderful skill in fabricating from such membranes thoroughly water-proof and highly elastic coverings for the feet and legs, as well as those invaluable rowing dresses known as "Kamlejkes," which, when drawn over the head and upper part of the body and fastened down to the rowing seat, enable the Aleutian in his one-holed *baidurka* to bid defiance to the fiercest storm and roughest sea. Unlike their neighbours, the Kamtschadales, who, in their aversion to come in contact with a corpse, throw their dead to their dogs to be devoured and removed from sight, the Aleutians devote much time and care to the preservation of the body after death. This they do so effectually that they can keep the corpse in their dwellings for more than a fortnight without causing injury or annoyance to the living, while long after death the features and external appearance of the deceased remain unchanged. Dr. Erman supplies us with many valuable additions to our knowledge of the social habits, taste for ornamentation, traditional lore, language, &c., of the Aleutians. In counting the Aleutian employs 20 as his highest numeral, making all larger quantities dependent upon that number; thus, 40, 60, &c., are respectively 2, 3, &c., twenties.—In the second paper of the *Zeitschrift*, Dr. Robert Hartmann continues his careful summary of the remains of Swiss Lacustrine dwellings, passing in review the principal mammals represented in the deposits, and entering fully into the often-discussed question whether the diluvial Cave bear (*Ursus spelæus*), is identical in species with our common bear (*U. arctos*) or whether and to what extent it differs from it. Dr. Hartmann seems disposed in this inquiry to regard the question of identity as possessing strong claims to probability, although there may not be sufficient ground at present to answer it affirmatively—"The Nirvana and Buddhistic Morality" forms the title of a very comprehensive paper by A. Bastian, which treats very fully of the principles on which the faith of Buddha is based, the ideas underlying the various forms which it has assumed, and the special phases of human thoughts and feelings to which it more particularly addresses itself.—In a paper by G. Rohlfs, entitled "Henry Noël, of Bagermi," the writer gives an account of the kingdom of Bagermi, which is situated on the N.E. of Lake Tsad, in Central Africa. The Bagermi people are a pure Ethiopian race, who, in point of moral and intellectual capacity, may be said to form the link between the most highly-developed negro kingdoms, and the numerous small negro states, lying to the S. of them, of which we do not even know the names. The King and Court of Bagermi, after a temporary adhesion to Islamism, have relapsed into their old Fetish worship, in which trees appear to form the principal objects of adoration. The practice of taking sisters and daughters in marriage prevails in the reigning family; but, while the rich indulge extensively in polygamy, poor men take only one wife.—Dr. Behrnauer, of Dresden, gives a *résumé* of an official paper by the Assistant-Resident, Herr J. Riedel, of Batavia, on the geographical, topographical, and geological character of the districts of Holontalo, Limœto, Bone, Boalemo, and Kattingola or Andagile in the Celebean Isthmus of the Eastern Archipelago. To this is appended much useful information in regard to the statistical, historical, and social condition of these countries, from which, however, we are not led to form a favourable opinion of the character, either of the Aborigines or of the Chinese and other foreign settlers. There are different grades of nobility, and till lately slavery and the slave-trade were allowed. Opium is undermining the health and vigour of the upper classes, and the poor are sunk in misery in the midst of an abundant vegetation, and with numerous sources of wealth around them; the mountains and river beds being rich in minerals. On the banks of the river Lonœo lumps of gold have from time to time been found as large as a hen's egg.—The last paper in this number of the *Zeitschrift* that we can notice is one by Herr Neumayer on the intellectual and moral qualities of the native Australians.

THE *American Journal of Science and Arts* for October. The first paper in this number is "On the Connecticut River Valley Glacier, and other examples of Glacier Movement along the Valleys of New England," by James D. Dana. In former papers by the author he has pointed out the existence of a Connecticut valley glacier in the glacial era, understanding by this expression that the under part of the great continental glacier, lying in the Connecticut valley, moved in the same direction. In the present paper the evidence with regard to this movement is gone into more fully, and further evidence is given to show that other large valleys of Central and Western New England had, in the same sense, their valley glaciers, that is the valleys determined the direction of the ice that lay within them.—Mr. R. Pumpelly follows with a second contribution "On the Paragenesis and Derivation of Copper and its Associates on Lake Superior." He gives a number of observations as to the minerals occurring with copper in various mines. In many of the cases in which calcite crystals are found enclosing copper, it is difficult to distinguish as to the relative ages of the two. The author has, however, conclusive proof that each of the following cases occur:—(1) that the copper was present before the calcite began to form and became enclosed in the growing crystal; (2) the crystal of calcite was partly formed, then became incrusted with copper, and was finished by a new growth of calcite over the metallic film; and (3) the copper has entered the calcite crystal since its growth was finished.—A valuable paper follows, "On photographing Histological Preparations by Sunlight," by J. J. Woodward. The arrangement which is found most suitable is to place the microscope at the window of the dark room, the body being horizontal, the achromatic condenser is then illuminated by a solar pencil, which is reflected from a heliostat on to a movable mirror. Between this mirror and the achromatic condenser there is placed a 2-inch lens of ten inches focal length, at such a distance that the solar rays are brought to a focus, and begin again to diverge before they reach the achromatic condenser. When a photograph is to be taken, a cell containing ammonia-sulphate of copper is placed between the lens and condenser, working with a power of 500 diameters; the time of exposure was but a fraction of a second. By allowing the solar rays to come to a focus before reaching the achromatic condenser, the heat rays may be separated from the light rays by so adjusting the condenser as to bring the light rays to a focus, while the heat-rays, after passing the second lens, became parallel, or even divergent according to the position of the achromatic condenser. The author finds that a right-angled prism may be used instead of the heliostat, and in working with low powers a piece of plain unsilvered plate-glass is sufficient instead of the mirror.—The concluding original paper in this number is "On the Discovery of a New Planet," by Dr. Peters, which will probably receive the number 116 of the asteroid group. The elements of the 114th asteroid have been computed, and are given, which show that this planet is not so small as was supposed. It is found to be now in the remotepart of its orbit, near its aphelion.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, November 16.—General Sir Edward Sabine, K.C.B., president, in the chair.

"Contributions to the History of the Opium Alkaloids.—Part III." By C. R. A. Wright, D.Sc.

"On a Periodic Change of the Elements of the Force of Terrestrial Magnetism discovered by Prof. Hornstein."

"Corrections and Additions to the Memoir on the Theory of Reciprocal Surfaces, Phil. Trans. vol. clix. (1869)." By Prof. Cayley, F.R.S.

"Corrections to the Computed Lengths of Waves of Light published in the Philosophical Transactions of the year 1868." By George B. Airy, C.B., Astronomer Royal. The author, after adverting to the process by which in a former paper he had attempted the computation of the lengths of waves of light, for the entire series measured in the solar spectrum by Kirchhoff, from a limited number of measured wave-lengths, and to the discordances between the results of these computations and the actual measure of numerous wave-lengths to which he subsequently had access, calls attention to his remark that means existed for giving accuracy to the whole. The object of the present paper is so to use these means as to produce a table of corrections applicable through the entire range of Kirchhoff's lines, and actually to apply the corrections to those computed wave-lengths which relate to spectral lines produced by the atmosphere and by many metals. Adopting as foundation the comparisons with Angström's and Ditscheiner's measures given in the former paper, and laying these down graphically, the author remarks that in some parts of the spectrum the agreement of the two experimenters is very close, that in some parts they are irreconcilable, and that in one part (where they agree) there is a peculiarity which leads to the supposition that some important change was made in Kirchhoff's adjustments. He then explains the considerations on which he has drawn a correction-curve, whose ordinates are to give the corrections applicable to his former computed numbers. A general table of corrections is then given, and this is followed by tables of the lengths of the light-waves for the air and metals as corrected by the quantities deduced from that general table. The author remarks that he has not yet succeeded in discovering any relation among the wave-lengths for the various lines given by any one metal, &c., which can suggest any mechanical explanation of their origin.

Zoological Society, November 21.—Prof. Flower, F.R.S., V.P., in the chair. Mr. Sclater exhibited and made remarks on a fine skin of *Ateles variegatus* Wagner (*A. bartletti* Gray) which had been received in a collection from Oyapok, on the eastern limits of Cayenne, being a new locality for this species.—A communication was read from Prof. Owen, F.R.S., containing the third of a series of memoirs on the osteology of the Marsupials. In this memoir Prof. Owen entered at full length into the modifications observable in the cranium of the three known species of Wombats (*Phascolomys*).—Dr. Günther, F.R.S., read a report on several important collection of Fishes which had been recently obtained for the British Museum collection. Amongst them were many new forms from the Pacific, obtained through the agency of the Museum Godefroianum of Hamburgh; several novelties from Celebes, collected by Dr. B. Meyer; and some interesting fishes from Tasmania, transmitted by Mr. Morton Allport. Dr. Günther called special attention to the occurrence of many well-known European forms of fishes in the Australian seas, and in explanation of this fact, suggested that these might also occur as deep-sea fishes in the intermediate seas of the tropics.—A paper by Mr. A. Anderson was read, containing notes on the Raptorial Birds of North Western India.—A communication was read from Messrs. G. Stewardson Brady and David Robertson, giving descriptions of two new species of British *Holothuroidea*.—Mr. P. L. Sclater exhibited and described, under the name *Turtur aldebranus*, a specimen of a new species of Dove of the genus *Turtur*, from the coral reef of Aldabra, north of Madagascar. This specimen had been lately living in the Society's Gardens, having been presented by Mr E. Newton—A paper by Mr. John Brazier, of Sydney, N.S.W., was read, giving descriptions of seven new species of the genus *Helix*, and of two Fluviatile Mollusks from Tasmania. A second paper, by Mr. Brazier, contained notes on the specific names of certain Land Shells from the South Sea Islands.—A communication was read from Count Thomas Salvadori, containing a note on *Ceriornis caboti*.—A communication was read from Mr. W. T. Blanford giving a description of a new Himalayan Finch, proposed to be called *Procarduelis pubescens*, from Sikim.

Anthropological Institute, November 20.—Sir John Lubbock, Bart., M.P., president, in the chair.—Captain R. T. Burton, late H.M.'s Consul, Damascus, read a paper on "Anthropological Collections from the Holy Land." Captain Burton having unexpectedly returned to England, under the peculiar circumstances now publicly known, travelled to Palmyra from Damascus between April 5 and April 20, 1870, and has brought home specimens of the Palmyrene mummies, the first which have seen the light in England, statuettes, beads, coins, and other articles calculated to throw light upon a subject hitherto left in the gloom of antiquity. On some of the figures described were emblems illustrative of the Phallic and other mysteries, and according with similar reliques found at Nineveh.—Dr. Carter Blake read a long note on the human remains discovered by Captain Burton at Palmyra. These indicated an entirely different race from that which inhabited modern Syria, and the skulls afforded many points of resemblance to the ancient Phœnicians which have been described by other anthropologists. The men were of large stature, in one case reaching probably about 6 feet 4 inches. There were among these remains not one which could be confidently referred to the Hebrew race, a fact on which the author laid stress, without offering any comment. Minute descriptions and measurements of all the specimens were given. Captain Burton will read further papers before the Anthropological Institute, and describe, with topographical notes, the various objects of silex and others which he collected during his 22 months of service in Syria and Palestine.

Entomological Society, November 20.—Mr. A. R. Wallace, president, in the chair. The following gentlemen were elected: Mr. C. V. Riley, State Entomologist for Missouri, as foreign member; Lieutenant B. Lowsley, R.E., and Mr. F. Raine, as ordinary members; and Mr. W. H. Miskin as a subscriber.—With reference to Prof. Westwood's exhibition of *Formica herculanea* (at the last meeting), found in the crop of a great black woodpecker said to have been shot near Oxford, Mr. Dunning remarked that, according to information received, several examples of this bird (presumably of foreign origin) were exposed for sale in the London markets at the precise time of its supposed occurrence near Oxford. Prof. Westwood had information from Messrs. Robertson and Jackson that it occurred in Devon; the former gentleman affirming that he had repeatedly seen it at Clovelly. Mr. F. Smith was informed that thirty examples had been recorded as British, and that one in particular had been shot by the grandfather of the present Lord Derby. Mr. Jenner Weir reiterated his belief in the species not being British, and Mr. Bond said that every recorded instance had been traced and found to be erroneous, save Lord Derby's example, concerning which doubt existed. Mr. E. Sheppard could not reconcile the occurrence of a gigantic ant, not hitherto known as British, in the crop of a bird, the origin of which was open to doubt, with the idea of the former being an addition to the British Fauna. Mr. McLachlan suggested that Prof. Westwood should visit the locality in which the bird was said to have been shot, and search for the ant. The discussion ended by Prof. Westwood promising to furnish further evidence.—Mr. Bond exhibited small pale examples of *Lasiocampa trifolii*, which appeared to form a distinct race; also females of *Clisiocampa castrensis*, with the wings on one side assuming male characters, without any evidence of gynandromorphism.—Mr. Stainton exhibited a variety of *Agrotis comes* (*Triphæna orbona* of collections), captured near Exeter by Mr. Dorville.—Mr. Smith exhibited the cocoons of the American *Tiphia tarda*; these were double, consisting of a flimsy outer casing, and a hard inner cocoon. He expressed his belief that the larvæ of the *Tiphia* devoured those of *Aphodius*. Mr. McLachlan brought before the notice of the meeting an instance of mimetic resemblance between two common North American *Libellulidæ*. The insects in question were *Libellula pulchella* Drury, and *Plathemis trimaculata* De Geer. In the former the sexes were nearly similar in appearance; in the latter very dissimilar, and the female almost precisely resembled that of *Libellula pulchella*. During the discussion which followed, the question was raised as to the liability or non-liability of dragon-flies to the attacks of birds. Mr. F. Smith had seen swallows devouring *Agrions*, and Mr. Briggs had observed a contest between a sparrow and a large dragon-fly in the streets of London, in which the former was obliged to retreat. It was recommended that American entomologists should observe the habits of these two species, and suggest a reason for the close mimicry existing between them.—Mr. Müller related that he had found the larvæ of a *Thrips* to be destructive to peas, by eating the outside of the green pods.—Mr. McLachlan read notes on the confusion existing in the nomenclature of the common European *Myrmeleonidæ*, in consequence of Linnæus having confounded them in his descriptions.—The publication of a further portion of the proposed general Catalogue of British Insects (*Hymenoptera Aculeata*, by Mr. F. Smith) was announced.

Linnean Society, November 16.—Mr. G. Bentham, president, in the chair.—"On the Floral Structure of *Impatiens fulva*," by A. W. Bennett, F.L.S. The author described the closed "cleistogenous" flowers of this plant, which are far more numerous than the well-known conspicuous flowers, and which produce nearly all the seed-vessels, being abundantly self-fertilised. He suggested that the "cap" formed by the calyx and corolla in these closed flowers is thrown off by the elasticity of the stamens, which are entirely different in structure from those in the conspicuous flowers, the anthers never dehiscing, but the pollen putting out its pollen-tubes while still in the anther, and piercing the wall in order to come into contact with the stigma. In the conspicuous flowers there is a peculiar arrangement in the form of a membrane attached to the stamen-tube, which prevents the access of pollen to the stigma, and as they do not appear to be visited by insects they seldom produce seed-vessels.—"Floræ Hongkongensis Supplementum," by H. F. Hance, Ph.D. In this paper a large number of new species are described, increasing the number included in Bentham's "Flora Hongkongensis" by about one-seventh.

GLASGOW

Geological Society, November 2.—Mr. John Young, vice-president, in the chair. Mr. James Thomson, F.G.S., laid before the meeting some portions of curiously-spotted clay which he had obtained during the recent excavations to the east of the old College of Glasgow. He stated that the occurrence of white spherical spots in the Old Red sandstone, particularly in the neighbourhood of Dumbarton, had often been remarked by the members, and various opinions had been expressed as to the probable cause of the discolouration. Having observed similar spherical markings in a bed of dull red clay which was being excavated near the old College, he secured several portions of it, which, after drying, split freely and exposed both discoloured spots and lenticular patches similar to those found in the Old Red sandstone referred to. On examination, he observed in the centre of each discoloured spot faint indications of some foreign body, which, on closer scrutiny, proved to be decayed vegetable matter; and on further breaking up the clay, he found the matrix around this vegetable matter always more or less discoloured, while the fibrous or woody matter itself was nearly black. He suggested that the phenomenon was due to the chemical affinity of the oxide of iron in the clay for the constituents of the vegetable matter, and that the discoloured spots in the red sandstone might be due to a similar cause, though no trace remained of the organism by which they were occasioned.

Dublin

Royal Geological Society, November 8.—Edward Hull, M.A., F.R.S., Director of the Geological Survey of Ireland, in the chair. G. H. Kinahan, M.R.I.A., read a paper on the Coal Measures of Ireland. This paper was in reply to some statements made by Mr. Hull at a former meeting of the Society in regard to the work of the late Mr. J. B. Jukes and his colleagues. Mr. Hull had stated that, while true Coal Measures existed in Connaught, there were none in either Leinster or Munster. The author argued that this assertion was quite erroneous, and that the Coal Measures of these three provinces were identical. Mr. Hull, in reply, seemed to argue that the lower Measures in Munster and Leinster were very similar to the so-called Yoredale rock and millstone grits of England, but acknowledged the general correctness of the maps published under Mr. Jukes' direction. Rev. Dr. Haughton moved that Mr. Kinahan's paper be published, and expressed his belief that all such subjects were much better discussed on published data.—Rev. Dr. Haughton F.R.S., read a note from Mr. Richardson, secretary to G. R. Graves, M.P., of Liverpool, informing him that the *Neptune*, Captain Edwards, had just put in from Quebec, and that the Captain reported that on the 12th October, at sea, in lat. 46° N., long. 35° W., at about 4 P.M., blowing strong from W., he observed a dense cloud of fog arise on the western horizon, which gradually came up with and surrounded the vessel, and so continued until midnight. From first coming up with the ship until clearing off, there prevailed a very strong smell of burning wood, both the Captain and crew felt their eyes much irritated by the smoke, and the decks were strewn with fine dust. At the time the ship was more than 2,000 miles from Chicago.—Prof. Macalister exhibited for the President, Lord Enniskillen, the skull of *Ursus ferox* found in the County of Monaghan.

Royal Irish Academy, November 13.—The President, Rev. Dr. Jellett, in the chair.—Dr. Whitley Stokes read a paper "On the Félire of Oengus." This ancient Irish MS., of which Dr. Stokes presented a translation to the Academy, although it, he said, was of but little literary merit, possessed from the purity of its vocabulary considerable value to the student of comparative philology, revealing very fully the position which the Celtic occupied in the great Aryan family of languages. Dr. Stokes illustrated his views by the comparison of many words with their cognate forms in Greek, Latin, Sanscrit, &c. He also explained the structure of the metre in the poem, and mentioned the several copies of the MS. in existence.—Prof. R. Ball read a paper, written by his brother, Valentine Ball, B.A., of the Geological Survey of India, "On the Andaman Islands," in which he gave a short account of a visit to the "Home" at Mount Augusta, which he made in company with Mr. Humfrey, who is the superintendent of the Home, and Dr. Curran. — Prof. Ball read a paper "On a Geometrical Study of the Kinematics, Equilibrium, and small Oscillations of a Rigid Body."—G. H. Kinahan read a paper "On the Granitic and other Ingenite Rocks of the Mountainous track of Country west of Loughs Mask and Corrib." The term Ingenite he adopted from David Forbes.—

Paris

Academy of Sciences, November 13.—M. Dumas noticed the loss which the Academy had sustained in the death of its foreign associate, Sir Roderick Murchison, of whom he spoke in high terms.—M. F. du Moncel read a note on the most economical arrangement of voltaic piles with respect to their polar electrodes, in continuation of a former note.—M. Faye presented a note on the spectroscopic measurement of the rotation of the sun by means of Dr. Zöllner's reversion spectroscope, in which he stated that Dr. Vogel of Bothkamp, near Kiel, had succeeded in effecting this measurement, and ascertained a velocity of rotation of 2,497 metres per second.—M. Faye also communicated a memoir on the law of rotation of the sun, in reply to a reclamation by Father Secchi, and a memoir by Dr. Zöllner; in this he indicated the reasons which led him to the belief that the sun is a gaseous body.—M. Le Verrier announced that but few meteors had been observed in France on the night of the 12–13th November.—M. Phillips read a paper on the governing spiral of chronometers.—M. H. Resal presented a note on the movement of a material system referred to three rectangular axes capable of moving around their origin.—General Morin communicated a memoir by M. Tresca on the results of experiments of flexion made upon steel and iron rails beyond the limit of elasticity.—A note by M. W. de Fonvielle was read relating to an observation made by M. Janssen on the stoppage of the rotation of the car of a balloon.—MM. Becquerel presented a memoir on the temperature of soils covered with low vegetation or denuded. The observations were made at various depths below the surface, from five to sixty centimetres, and showed that the mean temperature during the months of August, September, and October is lower under a denuded surface than under one covered with herbage.—M. C. Sainte-Claire Deville noticed the observation of faint aurora borealis in France on the evening of the 9th November.—A memoir, entitled "Thermic Investigation on Crystalline Dissociation," by MM. P. A. Favre and C. A. Valson, was read. The authors remarked upon the variety of phenomena involved in the solution of a crystalline salt in water, which they proposed to study from the thermo-chemical point of view, and tabulated and discussed the results of the solution of a long series of crystalline salts, chiefly sulphates.—M. E. P. Bérard presented a note on the *salant*, or saline crust, which is formed on the shores of the Mediterranean upon certain unproductive soils. Common salt is the chief ingredient in this crust.—M. Berthelot communicated a continuation of his memoir on the formation of precipitates, in which he discussed the thermal phenomena associated with the separation of the acid of salts from the base.—M. Maumené presented a note calling attention to the fact that he had some years ago indicated the possibility of the slow transformation of cane sugar into glucose.—M. J. Decaisne communicated some observations on the Pomaceæ, the chief object of which was to indicate the characters by which this important group of plants may be divided into good natural genera.—M. Bossin and M. Baudet communicated suggestions for the destruction of *Phylloxera vastatrix*.—M. Claude Bernard presented a note by M. Ranvier, on the Histology and Physiology of the Peripheral Nerves.—M. Milne-Edwards presented a note on *Oncidium celticum*, by M. L. Vallant, in which the author described the anatomy of that curious gasteropod, and expressed the opinion that although rightly placed among the Pulmonata, it presents certain affinities with the Opisthobranchiate mollusca.—M. de Quatrefages communicated a note by M. E. Perrier on *Eudrilus*, a new genus of Lumbricina from the West Indies.—M. Mène presented some investigations on the fat of domestic animals.

November 20.—A paper was read by M. de Saint-Venant on the mechanics of ductile bodies.—M. H. Resal presented a memoir on the movement of a point subjected to the action of a periodical cause, which experiences a constant resistance directed in the inverse direction of the velocity; M. C. Rozé a note on the asymmetry of the terminal curves of the spiral spring of chronometers; and M. de Saint-Venant a memoir by M. J. Boussinesq on the theory of the undulations and movements which are propagated along a rectangular horizontal canal when there is communicated to the liquid contained in this canal like velocities from the surface to the bottom.—M. Yvon Villarceau communicated extracts from a letter from Mr. Gould relating to the establishment of an Observatory at Cordoba in the Argentine Republic.—M. Le Verrier communicated a note giving the results of observations of meteors made in France on the 12th, 13th, and 14th November. Those observed on the 12th and 13th issued from a point in the neighbourhood of the constellation Auriga; the "Leonides" or meteors issuing from Leo were most numerous on the night of the 14th. M. Faye made some remarks on this communication, and to these M. Le Verrier replied.—M. Chapelas also presented a note on the meteors of November 1871.—M. Le Verrier presented a note by M. de Gasparis on the formulæ for calculating the orbits of double stars.—M. P. A. Favre read a continuation of his thermic investigations upon electrolysis, in which he discussed the thermic phenomena observed during the electrolysis of sulphate of copper, sulphate of zinc, nitrate of copper, and mixtures of neutral sulphates of zinc and copper with sulphate of hydrogen.—M. Elie de Beaumont made some remarks upon the Mont Cenis tunnel, and read a letter from Father Secchi on the pendulum experiments which it is proposed to make in the tunnel.—M. J. Bourget presented a paper on the velocity of sound in sonorous tubes.—M. Jamin communicated a note by M. E. Gripon on the transverse vibrations of wires and thin plates, and also a note by M. Alvergniat on a new phenomenon of phosphorescence produced by frictional electricity. According to the latter a small quantity of chloride or bromide of silicium hermetically sealed in a vacuum tube gives a bright luminosity when the tube is rubbed with a piece of silk. The chloride gives a rose colour, the bromide a greenish yellow.—M. Le Verrier presented a note on the history of the observations on the action of ecliptic conjunctions upon the elements of terrestrial magnetism, by M. Moise Lion.—M. Le Verrier also presented a note by M. Tarry, giving an account of an aurora borealis observed at Brest on the 9th November, in which the author noticed particularly the perturbations manifested by the apparatus employed in telegraphy.—M. Le Verrier also remarked that auroras had been observed in Piedmont on the nights of the 2nd, 9th, 10th, and 15th November, and referred to the coincidence between the occurrence of these phenomena and the November flight of meteors, which M. C. Sainte-Claire Deville supposed to exist.—M. Berthelot read the conclusion of his paper on the formation of precipitates. In this he discussed the changes which take place in the state of aggregation of precipitates, illustrating his views by the facts observed in the cases of the carbonates of strontia, baryta, lead, and silver, and of the oxalates.—M. Wurtz presented a note by M. E. Ritter on the transformation of albuminoid matters into urea by permanganate of potash. This note contained an experimental confirmation of M. Béchamp's statement.—M. de Quatrefages communicated an extract from a letter by M. E. S. Delidon, on the butts of Saint-Michel-en-l'Herm, and on the means by which their elevation above the sea, and other local elevations, may have been effected. He considers that local elevations may be due to infiltration of fresh and salt water.—Mr. E. Blanchard presented a note by M. S. Jourdain on the anatomy of the sunfish (*Orthagoriscus mola*)—An extract of a letter from M. A. Poey to M. Elie de Beaumont, on the influence of violet light upon the growth of the vine, pigs, and cattle, was read.—M. de Quatrefages presented a note by M F. Garrigou on lacustrine habitations in the Pyrenean region of the South of France. In this note the author describes the results obtained by him in the investigation of the deposits of ancient lakes at the foot of the Pyrenees.

BOOKS RECEIVED

ENGLISH.—Beeton's Medical Dictionary (Ward, Lock, and Tyler).

FOREIGN—Anales del Museo publico de Buenos Aires; Entrega octava: por German Burmeister (Paris, Savy). (Through Williams and Norgate)—Beiträge zur Parthenogenesis der Arthropoden: von Siebold.—Lehrbuch der chemischen u. physikalischen Geologie: G. Bischof.—Untersuchung des Weges eines Lichtstrahls durch eine beliebige Anzahl von brechenden sphärischen Oberflächen: P. A. Hansen—Die Arachniden Australiens nach der Natur beschrieben und abgebildet: Dr. L. Koch.

PAMPHLETS RECEIVED

ENGLISH—On the Formation of the Cirques of Brittany: Rev. T. G. Bonney.—Law of Husband and Wife: Philofamilias.—The Obstacles to Science Teaching in Schools: Rev. W. Tuckwell—Educational Hospital Reform: T. J. Boyd.—Report of Science and Art Department of the Committee of Council on Education, South Kensington—Directory, with Regulations for Establishing and Conducting Science Schools.—Flints, Fancies, and Facts; a Review: W. Robinson.—Cases of Diarrhœa: Dr. Chapman.—Apprenticeship to the Sea Service; a Circular of the Board of Trade.—Proceedings of the Bristol Naturalists' Society, Vol. iv., part 1.—Glaciation of the North-west of England: C. E. De Rance.—Cholera and Disinfection; Asiatic Cholera in Bristol in 1866: Dr. Budd.—Primary Schools and the Difficulty of Spelling: E. Jones—On the Effect of Small Variations of Temperature on Steel Magnets: Gordon and Newall.—Prize Medals of the Royal Geographical Society.—General Representation on a Complete Readjustment and Modification of Mr. Hare's Scheme: A. E. Dobbs—Reply to John Hampden's Charges against Mr. Wallace.—The Variations at Different Seasons of a *Hieracium*: Prof. Balfour.—Quarterly Journal of Education, October—Fifteenth Annual Report of the Medical Officer of Health of St. James's.—On the Pre-glacial Geography of Northern Cheshire: C. E. De Rance.—Man contemplated Physically, Morally, Intellectually, and Spiritually: J. W. Jackson.—Introductory Lecture on Experimental Physics: J. Clerk Maxwell.—On Ocean Currents: J. K. Laughton.

AMERICAN AND COLONIAL.—On the Constitution of the Solid Crust of the Earth: Archdeacon Pratt.—On the Direction and Force of the Wind: F. C. Loomis.—Influence of Temperature: F. C. Loomis.—Thoughts on the Higher Education of Women: Principal Dawson.—Papers and Proceedings of the Royal Society of Tasmania, 1871.—Monthly Notices of the Royal Society of Tasmania, 1871.—Reports of the Mining Surveyors and Registrars, Victoria, June 30, 1871.—Victoria; Patents and Patentees, Vol. iv.: W. H. Archer.—Victoria: Seventh Report of the Board of Visitors to the Observatory.—Lessons on Population, suggested by Grecian and Roman History: Dr. N. Allen.—An Address delivered at the Annual Exhibition of the Farmers' Club, Princeton: Dr. N. Allen.—On the Intermarriage of Relations: Dr. N. Allen.—Remarks on the Relations of Anomia: E S. Morse.—Correspondence on the subject of Atmospheric Electricity: Seth Boyden.—Spectroscopic Notes: Prof. C. A. Young.

FOREIGN.—Medizinische Jahrbücher, 1870: S. Stricker.—On the Diurnal Variation of the Inclination of the Magnet at Batavia: P. A. Bergmann.—On the Lunar Atmospheric Tide at Batavia: P. A. Bergmann.—Bulletin de la Société d'Anthropologie de France, Tome v.—Studi sopra un lignaggio anemofilo delle composte ossia sopra il gruppo delle Artemisiacee: Delpino.—Zeitschrift des oesterreichischen Gesellschaft für Meteorologie, Band vi., Nos. 14–22.

DIARY

THURSDAY, NOVEMBER 30.

ROYAL SOCIETY, at 4.—Anniversary Meeting.
SOCIETY OF ANTIQUARIES, at 8.30.—Notes on an Example of Alamannic Phalaræ: W. M. Wylie, F.S.A.—On an Early French Deed (A.D. 1397) Relating to the Knights of Saint John of Jerusalem: C. K. Watson, M.A.

FRIDAY, DECEMBER 1.

GEOLOGISTS' ASSOCIATION, at 8.—On the Glacial Drifts of North London: H. Walker.
ARCHÆOLOGICAL INSTITUTE, at 4.

SUNDAY, DECEMBER 3.

SUNDAY LECTURE SOCIETY, at 4.—The Coast Line and its Teachings: Dr. T. Spencer Cobbold, F.R.S.

MONDAY, DECEMBER 4.

ENTOMOLOGICAL SOCIETY, at 7.
ANTHROPOLOGICAL INSTITUTE, at 8.—Anthropological Collections from the Holy Land. No II.: Captain Richard F. Burton, F.R.G.S—On a Collection of Flint Implements from the Cape of Good Hope: Prof. Busk, F.R.S., and Rev. Mr. Dale
VICTORIA INSTITUTE, at 8.—The Serpent Myths of Ancient Egypt: W. R. Cooper.
LONDON INSTITUTION, at 4—The Physiology of Bodily Motion and Consciousness (VI.): Prof. Huxley, F.R.S.
ROYAL INSTITUTION at 2.—General Monthly Meeting.

TUESDAY, DECEMBER 5.

ZOOLOGICAL SOCIETY, at 9.—On the Freshwater Siluroids of India and Burmah: Surgeon Francis Day, F.Z.S.—On a Small Collection of Butterflies from Angola: A. G. Butler.—Description of a New Genus of Lepidoptera, allied to *Apatura*: A G. Butler.
SOCIETY OF BIBLICAL ARCHÆOLOGY, at 8.30.

WEDNESDAY, DECEMBER 6.

GEOLOGICAL SOCIETY, at 8.
SOCIETY OF ARTS, at 8.—On Sewage as a Fertiliser of Land, and Land as a Purifier of Sewage: J Bailey Denton.
MICROSCOPICAL SOCIETY, at 8.—On Microscopic Uredines: M. C. Cooke, M.A.

THURSDAY, DECEMBER 7.

ROYAL SOCIETY, at 8.30.
SOCIETY OF ANTIQUARIES, at 8.30.
CHEMICAL SOCIETY, at 8.
LINNEAN SOCIETY, at 8.—Botany of the Grant and Speke Expedition: Lieut.-Col. Grant, C.B, C.S.I.—On a hybrid *Vaccinium* between the Bilberry and Crowberry: R. Garner, F.L.S.—On the Formation of British Pearls, and their possible improvement: R. Garner, F.L.S.

CONTENTS

ERRATUM.—P. 63, col. 1, lines 19, 18, 16 from bottom, for "lineau" read "linear."

THURSDAY, DECEMBER 7, 1871

THE CHAIRS OF SCIENCE IN THE SCOTTISH UNIVERSITIES

THE biographer of a Scottish Professor says (we fear boastfully) that his friend had lectured on anatomy, chemistry, physiology, pathology, medical jurisprudence, and medicine, and that he was well qualified also to lecture on botany, mineralogy, and geology. There were giants then surely, but their day is past; for the Professor of Natural History in Glasgow University is just now trying to procure the erection of a new Chair, on the ground that geology or comparative anatomy is, either of them, as much as he can effectively teach. Perhaps no better indication of the enormous progress of Science during the last half century could be found than the facts we have mentioned. The earlier professor found his multifarious duties possible because the subjects were very limited, and, in physiology, chemistry, mineralogy, and geology, the means of investigation were few. Now geology has outgrown the dimensions of anatomy, as a teaching subject. The Chairs of Natural History in Scotland, now only two in number, those in Glasgow and Aberdeen (for Science is only provided for temporarily in St. Andrews at present at the cost of Civil History), are remarkable foundations. There is no clear notion what the Professors may not teach. Custom has settled that geology and zoology shall be expected of them, and the Ordinances of the University Commissioners act upon this tradition. But it is doubtful if successful restraint could be put upon an eccentric Professor who selected ethnology and meteorology as his topics. He would lose class fees; but as he holds from the Crown, and the Crown has not defined his duties, he would be legally safe. Fortunately there has been no attempt hitherto to act independently of University needs; on the contrary, there have been from time to time voluntary modifications of the class work, both as regards the length of the courses and the subjects, so as to meet the needs of students. But this very complaisance has been injurious; for, to take the case of Glasgow, the Universities (Scotland) Act made zoology a compulsory subject for medical students, and the Court and Senate at a later date resolved to grant a degree in Engineering Science (modestly calling it a certificate), requiring geology as one of the subjects of examination. Complete systematic courses were therefore indispensable, and the attempt to provide these has demonstrated their impossibility; hence the present attempt to procure a change.

While sympathising with the Glasgow Professor, and with his colleagues in Aberdeen and in Queen's College, Ireland, we decline to discuss the question as one of individual hardship, or even as detracting from the efficiency of one or several Universities. The existence of lectureships which profess to be scientific, but which can only be popular if the work is equally divided between the different subjects, is an evil which demands a remedy, and Scotland cannot be indulged in her fancy for multiple Chairs, as anatomy and botany, logic and rhetoric, moral philosophy and political economy, civil and Scots law. If the teacher has a strong bias in favour of either subject he will throw himself into that and neglect the other, even though it forms part of that curriculum for which a degree is granted. Now, apart from the degradation of a scientific honour, the lowering of the standard of scientific teaching is especially to be guarded against at the present time. There are too few inducements for young men to devote themselves to Science as a life profession, still less encouragement do they receive to devote their energies to one branch exclusively. If our Universities continue to sanction average teaching, it will be a substantial injury to education throughout the country, and will put an end to that scientific work upon which the progress of science and the reputation of the country ought to rest: for it cannot be expected that a man whose ideas are frittered away by desultory work can have either the inclination or the time for patient continuous research. It is to be regretted that the Scottish Universities are too poor to help themselves in this matter. Private liberality has placed Edinburgh in a right position; geology and zoology being respectively the entire occupation of Geikie and Wyville Thomson. In Newcastle the new college has started wisely with one subject, geology; but it is to be hoped that zoology will ere long be added as a separate professorship. In the London colleges separate provision, such as it is, is made for these two branches of Science, and even in the Universities which flippant so-called Radicals are wont to denounce as effete, and to contrast unfavourably with their Scottish sisters, there is provision for teaching as well as for the teacher.

It is in the interest of these and other bodies that we urge the necessity of reforming Scottish Universities in the matter of Science teaching. If they are permitted to continue as at present, the good done by their better equipped rivals will be diminished. It is a mistake to suppose that one college is better off if the teaching in another is defective; that may happen for a year or two, but in the end all suffer for the fault of one, all are lowered in tone though they may not be brought equally low. To maintain English teaching, Scottish teaching must be raised. But as no funds exist on which a just claim may be established for this purpose, private generosity or the State purse are the only appeals. Cabinet Ministers have been known to talk of Science as having condemned itself if it is not self-supporting, and in London there is a current opinion that Science is too largely subsidised, comparatively speaking, north of the Tweed. But it must be remembered that in Glasgow and Aberdeen, even in Edinburgh, it is impossible, save in the exceptional case of the director of the Scottish Survey, to find a man qualified for the post, and at the same time deriving an adequate income from other sources; for the time is past when Science was the pursuit only of the wealthy. It may not be sound in principle, but it is a practical necessity for the State to endow Science in the provinces; failing that and failing local effort, it would be best, in the interests of sound education, to suppress the starved chair altogether. But in the particular cases at present under consideration there is a strong claim on the State; the chairs of Natural History are creations of the Crown, and as circumstances have altered greatly since their creation, it behoves the Crown to secure that its intentions are fulfilled by making corresponding alterations.

Of course this is the final resort after it is clear that Scotsmen decline to supply the money needed; but in Glasgow at least it is not to be believed that the examples of Manchester, Birmingham, and Durham will be without effect. All that has been said is equally true of Ireland; but the practical treatment of the difficulty involves other considerations upon which we cannot at present enter.

JUKES'S LETTERS

Letters and Extracts from the Addresses and Occasional Writings of J. Beete Jukes, M.A., F.R.S., F.G.S. Edited, with connecting Memorial Notes, by his Sister, with a Portrait. (London: Chapman and Hall, 1871.)

HOW few among us—when his glass is run—would care to have the story of his life from year to year, even from his boyish days, writ down and published to the world—indeed, how very few would be found worthy of more record than "born, lived, died." Now and again, however, one meets with a man whose career in life is not only lifted above the monotonous hum-drum existence of ordinary mortals, but who, both by his life and writings, attracts our admiration and regard.

Such a man was Joseph Beete Jukes, a sketch of whose life and writings, together with some two hundred letters, edited by his sister, Mrs. A. H. Browne, form the substance of this volume.

Blest not only with a goodly person and stature but with a noble and generous nature, which won to his side both the ignorant and the educated, Mr. Jukes was also a man of high mental endowments, and both as a speaker and a writer had the knack to command attention. But in his leisure hours no one entered more keenly than he into all the enjoyments of the country, being fond of hard riding, and a keen sportsman and good shot. Nor was he less fond of a good joke, as his letters often testify.

Educated at Cambridge during Sedgwick's palmy days,* no wonder that he caught some of the fire from "Old Adam," as his students lovingly nicknamed him, and instead of entering the Church, as his mother fondly hoped, inaugurated a career for himself by walking through Derbyshire, Staffordshire, Cheshire, Shropshire, Yorkshire, and many other parts of England, geologising and lecturing wherever he could get a class to attend. And very successful Jukes seems to have been. Writing from Nottingham in June 1838, he says, "I have had a very good class here, never less than two or three hundred, and frequently four or five hundred" (p. 26).

Having about 1838 made himself acquainted with practical surveying, he was in 1839 offered the appointment of Geological Surveyor of Newfoundland, a post he gladly accepted, and which occupied his time until the close of 1840. Into all the hardships of this work he entered with his accustomed good-will and spirits. Mr. Jukes contrasts his own easier lot with that of the hardy naturalist Prof. Stuwitz, who "set off at the beginning of December in a boat with a little cuddy, to which (he says) my cabin is a palace, to see the winter fishing in Fortune Bay, with the chance of being frozen up on his return, and having to get ashore and come through the woods and snow," and he adds, "don't talk of my hardships and privations and courage" (p. 91). But the Newfoundland survey ended in October 1840,* and early in 1842 Mr. Jukes had the satisfaction to find himself appointed to the office of Naturalist to the Expedition for surveying Torres Straits, New Guinea, &c., on board H.M. ship *Fly*, commanded by Captain F. P. Blackwood, R.N. This task, so congenial to him who loved no occupation so well as one requiring constant out-door exercise in the saddle, on foot, or on the water, occupied him until June, 1846, and during his four years' absence his letters and journals furnish abundant materials of interest to the reader; much of which, however, will necessarily also be found in Mr. Jukes's book entitled "Narrative of the Surveying Voyage of H.M.S. *Fly* (2 vols.), published in 1847.

His description of scenery in the interior of Java is very interesting:—"Rich plains covered with all kinds of tropical productions, watered in every direction by clear rocky brooks, surrounded by mountains, either in single cones or serrated ranges, from 5,000 to 11,000 feet in height; abundance of game whenever we choose to stop and shoot, jungle-fowl, peacocks, deer, wild pigs, tigers. We crossed one great range of mountains by a path that led us through the extinct crater of a volcano, five miles across and 7,000 feet above the sea, and in the centre of which was a small cone and crater still in action, though when we looked down into it it was only blowing out steam, with a roar as of a thousand blast-furnaces. Take a scene on the slope of these mountains, as they dip into the plain of Malang. Scene:—An open mountain valley, full of coffee plantations, with small scattered villages, into which opens a deep mountain glen, crowded with the rankest luxuriance of tropical vegetation, groups of tree ferns and great broad-leaved plants, so as to arch over and frequently hide altogether the full brook that comes flashing and roaring down the rocks in a succession of rapids, varied by waterfalls; the road, narrow, steep, and slippery, as it winds down the sides of the glen, expands into a broad green lane, with an exquisite carpet of turf as it opens on the more level lands" (pp. 238, 239).

Like every other man who is fond of the sea, we find him exclaiming, "I confess I am getting more and more enamoured of a sailor's life, and regret I did not know the navy early enough to enter it. I see it would have suited me exactly" (p. 251).

But Mr. Jukes was destined to be a geologist. On the return of the good ship *Fly*, in June 1846, he only allowed himself a few weeks at home before he had again "signed articles" to Sir H. T. de la Beche, then Director-General of the Geological Survey, and in October joined Profs. Ramsay and Forbes at Bala. These appear to have been his most intimate friends, as his letters to Ramsay abundantly attest. His letters to Forbes have, unfortunately, not been preserved. To those not connected with the Survey, this is the section of the book which it seems to us will be the least interesting, although here and there one comes upon a funny bit or a matter of public interest.

His fagging away at the geology of the rocks south of Conway forms the subject of many letters, and the solution of their puzzling structure is well given at p. 306. For

* He matriculated at St. John's in 1830, being then nineteen years of age.

* For an account of his Newfoundland experiences and travels, see also "Excursions in and about Newfoundland during the years 1839 and 1840," 2 vols. 8vo, London, 1842. See also "Report on the Geology of Newfoundland," folio, 1840.

comical bits, the story of a new fossil discovered (p. 314); the boundary of the Caradoc Sandstone at Pentre Voelas (p. 318); and "a strange and marvellous history of a temptation and what befel thereon" (p. 323), must be read and laughed over, as also must the account of Miss Moggore and Miss Bood, natives of Murray and Darnley Islands, who *would* walk arm-in-arm with Mr. Jukes (p. 252).

Besides a vast number of letters to Prof. Ramsay, all more or less relating to geology, there are letters to Dr. Ingleby and other relatives; one on Versification (p. 377), in which two of Mr. Jukes's own verses appear. The annexed is a sample, probably intended for the Old Annual Survey Dinner: *—

Free o'er the hills our feet shall roam,
We'll breathe the mountain air, sir;
Care shall not ever dare to come,
Nor grief pursue us there, sir.
Joyous in Nature's wildest scene,
Where rocks lie topsy-turvy,
And falling waters flash between,
We'll prosecute the Survey.
Oh, the Survey, the Geological Survey!
Health and good humour shall be queen
Of the Geological Survey!

We have religious beliefs considered (p. 375); views on Providence (p. 386); creeds (p. 409); political opinions (p. 405), and many other matters discussed.

But we have said sufficient to recommend the book to all who are likely to be interested in it. We would especially direct geologists to it, as being the record of the life of a man who did very much for their science—indeed, who died in its service. To his friends, who are to be found scattered far and wide, the title of the book is sufficient to recommend it to them. To his relatives and intimate companions his memory will always be dear.

It seems strange that Prof. Jukes's life should be dedicated to Prof. Sedgwick, his early teacher; but so it is—the old oak, though decayed and feeble, still puts out its green leaves; but the younger man, whom he bid God speed thirty years ago, has already rested from his labours.

H. W.

* Alas! that this time-honoured institution of meeting "all hands" once a year should have fallen into disuse. It was a very bond of union.

OUR BOOK SHELF

The Science of Arithmetic. By James Cornwell, Ph.D., and Joshua G. Fitch, M.A. Thirteenth Edition. (Simpkin, Marshall, and Co., 1870.)

The School Arithmetic. By the same authors. Eleventh Edition. (Simpkin, Marshall, and Co., 1871.)

THESE books are too well known to mathematical teachers to need detailed notice from us. Both are very good, and stand in the first rank among the scores of arithmetics published in England. The explanations, arrangement and examples, especially in the former book, are generally very good. We will venture, however, to suggest two or three changes to the authors, which we think would render the book better still, and which our experience would make us wish to see universally adopted. The rule for multiplication of decimals given in these books is the old one of counting the decimal places. We think this becomes a rule of thumb. The method ought to be the same as that in multiplication of integers; and it is at once seen by the pupil that as in multiplying by tens and hundreds, the figures are shifted to the left; so in multiplying by tenths and hundredths, they are shifted to the right. The decimal point is brought down straight, and each line in the working has its meaning; as in the example, multiply 712·35 by 15·807:—

```
  712·35
   15·807
 --------
 3561·75
 7123·5
  569·880
    4·98645
 -----------
11260·11645
```

This is more certain to be understood *every time it is done* than the old counting rule, and each line means something. Again, in that schoolmaster's *crux*, the division of decimals, we have in the books before us, the old Case 1, Case 2, and Case 3; and everybody knows the result in an examination. A better method is this, which we indicate briefly. Explain first that you cannot divide until the quantities are of the same kind, and of the same denomination. You cannot divide 2*l.* by 3 pence, till you have reduced the pounds to pence. Nor can you divide tenths by thousandths, till you have reduced the tenths to thousandths. Hence, to divide 1·375 by ·0025, the dividend must first be expressed in the same denomination as the divisor, namely as ten thousandths; this amounts to marking off as many decimal places in the dividend as there are in the divisor, which is best done by drawing a line after the figure, and then dividing. It is plain that the result is integral until the figures on the right of the line are brought down. It is worth while, perhaps, to give examples of the different cases; the explanation is obvious from what has been already said—

Divide 7·9 by 4·308—

```
4·308) 7·900|00 (1·83 . .
       4·308
       -----
       35920
       34464
       -----
        14560
```

Divide 34·79628 by 2·5—

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2·5) 34·7|9628 (13·91 . . . .
     25
     --
     97
     75
     --
     229
     225
     ---
      46
```

Lastly, the methods of summation by differences and interpolation are essentially arithmetical, and of considerable interest, and we think might be introduced with advantage in the larger work.

The miscellaneous questions at the end of the larger book are not particularly good. They are often tedious, and not sufficiently varied, suggestive, or difficult. Nevertheless, the books are very good, and will teach teachers as well as learners.

J. M. W.

Skandinaviens Coleoptera, synoptiskt bearbetade af G. C. Thomson. Tom. X. 8vo. (Lund, 1868. London: Williams and Norgate.)

THERE are few investigations of more interest to the student of British Natural History than the comparison of our native productions with those of the Scandinavian peninsula, and no descriptive works published on the Continent, a knowledge of which is of greater importance to him, than those of the acute and laborious naturalists of Scandinavia and Denmark. The work done by these men is usually of the highest quality, both for carefulness of investigation and clearness of statement; and the great similarity which exists between the faunas and floras of our islands and of the Scandinavian region, enables their work to be used to a certain extent as handbooks by British Naturalists. May their study lead the latter to imitate the Scandinavian mode of work! We are led to these remarks by the receipt of the tenth and concluding volume of Prof. Thomson's descriptive work on the Scandinavian Coleoptera, although this consists almost entirely of corrections, emendations, and additions to the contents of the nine previous volumes, in which the systematic description of those insects was completed. Prof. Thomson's work will be found of the highest value to the British entomologist, inasmuch as a very large proportion of the insects described in it are inhabitants of these islands, and many of the others will probably be discovered hereafter in the north of Scotland. The whole descriptive portion of the book is written in Latin, the characters, although often brief, are admirably drawn up, and the determination of the species is greatly facilitated by the excellent tables both of genera and species given throughout the work. Amended tables, introducing all new forms discovered during the progress of the book, are given in the second part of the ninth and in the tenth volumes. Although it appears under a Swedish title, the only portions of the work written in that language are the notices of localities of occurrence and critical remarks on genera and species, the former, at any rate, requiring little knowledge of Swedish for their comprehension. W. S. D.

Ichneumonologia Suecica, auctore Aug. Emil Holmgren. Tom. II. (Stockholm, 1871. London: Williams and Norgate.)

THIS is a second most important Swedish work, which illustrates in a striking manner the remarks which we made in noticing M. Thomson's "Skandinaviens Coleoptera." In this the author has commenced a monographic revision of the Swedish members of one of the most difficult families of insects, the Ichneumonidæ, which he here treats in an almost exhaustive fashion. We cannot venture to say how far he is correct in his synonymies, or in the reference of supposed species to others as varieties; but he has spared no pains in the preparation of his descriptions, and the student of his book will find no difficulty in understanding precisely what he means. This work, when completed, will be an invaluable aid to the few entomologists who venture upon the study of the Ichneumonidæ. 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 Planet Venus

THIS beautiful planet being now very favourably situated for examination, it may interest many of your astronomical readers if I give a brief description of the markings which have recently been seen on her surface. That these markings are exceedingly difficult objects to detect, even with a powerful telescope and under favourable atmospheric conditions, there is no doubt, and many observers have consequently failed to see them. The late Rev. W. R. Dawes, although possessed of very excellent vision, could never make them out, and it seems that the fact of their existence is doubted at the present time by some observers. At the meeting of the Royal Astronomical Society on November 10 last, "the Astronomer Royal mentioned that Venus was very favourably situated for observation, especially for noticing spots if any existed on her surface, his own experience being that there were no certain markings thereon, which the President corroborated." The opinions of such eminent astronomers should always be carefully considered, and the matter in dispute thoroughly investigated, before a contrary opinion is entertained. In the present case, however, I think that there is a sufficiency of evidence to prove that markings of various forms exist on the surface of the planet. I am the more particularly induced to say this by having before me upwards of sixty sketches of their appearance, made by experienced observers, who in the making of observations employ telescopes of great power and excellent definition. No doubt the faint cloudlike markings can only be made out after attentive gazing, and then are scarcely visible, though they have been distinctly seen by many observers. It is difficult to account for the fact that Mr. Dawes could not distinguish them, but perhaps the reason may be apparent, if we consider that an observer who is the most successful in the observation of faint companions to double stars, cannot satisfactorily observe the faint markings with which the planet's disc is diversified. Many observations of the spots were made at Rome in 1839—1841, and of six observers those were the most successful who experienced the greatest difficulty in detecting minute companions to large stars.*

With respect to the spots and markings which have recently been examined, it may be said that they are of various forms and degrees of visibility. Some of them are only just perceptible after a long and careful scrutiny of the planet's disc, while others are much more apparent, and distinguishable with less difficulty. Whether or not they are permanent in their form remains to be determined from a comparison of the whole of the observations. Some of the representations of the cloudy spots taken at different dates seem to be somewhat similar in their principal features. Several times that position of the planet's surface immediately adjacent to the terminator has been seen to be interspersed with small bright circular spots, which seem to be analogous to lunar formations. These bright spots have been seen by several of those who have critically examined the planet's appearance. They were seen by Mr. T. H. Buffham on May 4 and May 6, 1868, and Dr. Huggins at the last meeting of the R.A.S. said that "he had occasionally seen dusky spots, but he considers them as very uncertain or illusive. When, however, the crescent was thin and the planet near the earth, he had seen minute points of light on the terminator, which by most observers was described as irregularly indented. He had also noticed that when definition was very good, appearances analogous to those of lunar craters had been seen. Dr. De la Rue had often seen markings on Venus similar in character to those observed on Mars." An observation made by Mr. F. Worthington, with a 13-inch reflector, on June 11 last, confirms the statement made by Dr. Huggins in reference to the bright markings on Venus being similar to objects on the surface of our satellite. He writes, "Definition extremely good. The markings were very clearly seen, and bore a very remarkable resemblance to the craters and inequalities of the moon as seen with a low power, say an opera glass." From the foregoing it would appear to be beyond a doubt that, when the planet is in a crescent form, small bright markings, resembling lunar craters, are perceptible. These objects should be persistently looked for, and when observed the details of their appearance and position duly registered.

That the dark, cloud-like markings are similar to those on the surface of Mars, as stated by Dr. De la Rue, seems also an established fact. Mr. Henry Ormesher saw several irregular spots on Venus on May 10 last, and he says they were "clear and well-defined, and reminded me very much of those on the planet Mars, as they had much the same appearance." Of course the markings on Mars are much more conspicuous than those visible on Venus, but in their appearance there is no doubt that they are not altogether unlike.

In many of the drawings which I have before me the outlines of the cloudy patches do not terminate abruptly as in the case of the penumbræ to solar spots (*maculæ*) but seem to fade away into the general brilliancy of the disc. In some of the sketches, however, the boundary of the spots appears to have a well-marked outline. In regard to the terminator, it seems to have a very serrated edge, but in some of the drawings this is not depicted.

Referring again to the coincidence in the appearance of the bright spots of Venus and the craters of the moon, I would draw the attention of your readers to the Rev. T. W. Webb's "Celestial Objects," second edition, p. 51, in which there is an observation of interest recorded. WILLIAM F. DENNING

Hollywood Lodge, Cotham Park, Bristol, Nov. 28

* See Webb's "Celestial Objects," p. 50. It is there stated that "a very sensitive eye which would detect the spots more readily would be easily overpowered by the light of a brilliant star, so as to miss a very minute one in its neighbourhood."

The Flight of Butterflies

IN the 103rd number of NATURE there are two notices of remarkable butterfly flights in America, and it is asked "Where the yellow butterflies are going?" Mr. R. Spruce, in "Notes on some Insect and other Migrations observed in Equatorial America" (published in the Journal of the Linnean Society, vol. ix. No. 38, read June 6, 1867), has the following curious account of similar flights, which, he says, have also been described by Messrs. Edwards, Wallace, and Bates: "The first time that I fell in with such a migration was in November 1849, near the mouth of the Xingú, when I was travelling up the Amazon from Pará to Santareon. . . . We saw a vast multitude of butterflies flying across the Amazon from the northern to the southern side in a direction from about N.N.W. to S.S.E. They were evidently in the last stage of fatigue. They were all of common white and orange yellow species, such as are bred in cultivated and waste grounds, and having found no matrix whereon to deposit their eggs to the northward of the river (the leaves proper for their purpose having probably been already destroyed or at least occupied by caterpillars) were going in quest of it elsewhere. The very little wind there was, blew from between E. and N.E., therefore the butterflies steered their course at right angles to it; and this was the case in subsequent flights I saw across the Amazon. . . . But the most notable circumstance is that the movement is always southward. . . . Since my return to England I have read Mr. Bates's graphic description of a flight of butterflies across the Amazon, below Obidos, lasting for two days without intermission during daylight. These also all crossed in one direction, from north to south. Nearly all were species of Callidryas, the males of which species are wont to resort to beaches, while the females hover on the borders of the forest and deposit their eggs on low-growing, shade-loving Mimosas. He adds, 'the migrating hordes, so far as I could ascertain, are composed only of males.' It is possible, therefore, that in the flights witnessed by myself the individuals were all males in which case the flights should probably be looked upon, not as migrations, but dispersions, analogous to those of male ants and bees when their occupation is done, and they are doomed by the workers to banishment, which means death. In the case I am about to describe, however, the swarms certainly comprised both sexes, although I know not in what proportion; and their movements were more evidently dependent on the failure of their food.

"In the year 1862 I spent some months at Chandsey, a small village on the desert coast of the Pacific northward of Guayaquil, where one or two smart showers are usually all the rain that falls in a year; but that was an exceptional year, such as there had not been for seventeen years before—with heavy rains all through the month of March, which brought out a vigorous herbaceous vegetation where almost unbroken sterility had previously prevailed. In April swarms of butterflies and moths appeared coming from the East, sucking the sweets of the newly-opened flowers, and depositing their eggs on the leaves, especially of a Boerhaavia and of a curious Amaranth, until the caterpillars swarmed on every plant. New legions continued to pour in from the East, and finding the field already occupied, launched boldly out over the Pacific Ocean, as Magalhaens had done before them, there to find a fate not unlike that of the adventurous navigator. No better luck attended most of the offspring of their predecessors, especially those who fed on the Boerhaavia. The shoal of caterpillars advanced, continually westward, eating up whatever to them was eatable, until, on nearing the sea shore and the limit of vegetation, I used to see them writhing over the burning sand in convulsive haste to reach the food and shelter of some Boerhaavia which had haply escaped the jaws of preceding emigrants. The explanation of this continual westward movement is not difficult. A few leagues inland, instead of the sandy coast-desert with here and there a tree, we find woods, not very dense or lofty, but where there is sufficient moisture to keep alive a few remnants of the above-mentioned herbs all the year round, and doubtless also of the insects that feed on them. There are also cattle farms. When the rains come on, therefore, they cause as it were a unilateral development of the vegetation from the forest across the open ground, and a corresponding expansion of the insect-life which breeds and feeds upon it."

The whole paper is very interesting, but I have copied only such portions as bear on the question "Where are the yellow butterflies going?"

T. S-M.

The Origin of Insects

IN an article by Dr. Beale, in your number for Nov. 23, on "One of the Greatest Difficulties of Darwinism," a most extraordinary misconception is stated to be a difficulty. That the pupa state is a modification of the ordinary process of skin-shedding in the Insecta is proved by so many facts, that one cannot understand for a moment how it can possibly be denied, much less how its denial can be made use of as an argument against the doctrine of evolution. Sir John Lubbock pointed out long ago that, in the development of the Insecta, every grade of modification exists between those insects which are gradually developed, each successive ecdysis producing only the slightest possible modifications, and those which undergo a change so complete that it may be likened to the process of metagenesis, as it has been called, which takes place in the Echinodermata.

It is an utter mistake to suppose that any insect is redeveloped during the pupa state. The most perfect instance of metamorphosis is that of the flies (some Diptera). In these the materials out of which the perfect insect is developed are supplied by the breaking up of the muscular system and fat bodies of the larva; but the cellular structures known as the Imaginal discs of Weismann are formed in the egg, and persist all through the life of the larva. These, it is true, only form a skin or case in which the fly is developed; but they are really nothing more than a larva skin, formed on the inside of the larva skin in the egg, and detached from it by the subsequent modifications of the larva.

The nervous system undergoes extensive modification in the development of the fly, but it never undergoes degeneration. The mouth organs of the imago, it is true, are not the mouth organs of the larva, nor are they formed by their modification, but they are foreshadowed in the egg before the mouth organs of the larva are formed. It is the mouth organs of the larva which are new formations, not those of the imago. In this most extreme case, the pupa skin is derived directly from the inner layers of the first larval skin, about twelve hours before the creature emerges from the egg. The imaginal skin is likewise derived from cells laid down in contact with the imaginal discs. There is absolutely only a difference in the time at which the successive skins are formed in this and in ordinary ecdysis.

A cimex which undergoes no change of form develops each successive skins from cells laid down within the last integument, and the same process is followed in the development of the fly.

The alimentary canal is likewise undoubtedly formed in a similar manner around that of the larva, and the sexual organs are gradually developed, even from the time when the embryo is enclosed in the egg.

Fritz Müller in his "Facts for Darwin," has shown very conclusively that the larval forms of insects are probably derived from imaginal forms; such seems, without doubt, to be the case with the flies (*Musca*). Every day the difficulties presented by the development of the Insecta to the doctrine of evolution are vanishing. It is extremely probable that insects first emerged from the water with fully formed wings. We have still relics of an aquatic winged insect fauna in the hymenopterous genus discovered by Sir J. Lubbock. We may readily believe the larval forms now existing on the earth are modified forms of originally perfect insects; we know that the larvæ are subject to far greater changes of life and far greater struggle for existence than the perfect insects. They are all probably embryonic forms, brought from the egg in a modified state before their perfect development is attained. The same thing is seen in several crustaceans, which are hatched as *Nauplius* forms, whilst all their allies attain the *Zœa* stage in the egg. The existence of mandibulate larvæ in insects which in the perfect state have suctorial mouths, is an additional argument in favour of this view. It appears to be either a reversion in the larva to an anterior type, for the earlier types of the Insecta were undoubtedly mandibulate, or it may by an embryonic character, which has never been lost in the egg, modified by reversion or circumstances. This view may appear fanciful, but the aortic arches of a fish undoubtedly exist in the mammalian embryo, and no one can say what changes might take place by reversion in those arches under altered conditions. Teratological embryology goes far to show that the embryo may revert to long anterior types in its development.

I should, however, transgress too far on your valuable space in giving proofs of all that has been put forward. I trust, however, that even this little may do some good, for it does seem hard, when the labours of men like Fritz Müller, Weismann, and Lubbock, are throwing light on this intricate subject, that darkness should return in the form of manifest misconceptions of well-known phenomena. B. T. LOWNE

99, Guilford Street, W.C.

Aspect

MR. LAUGHTON'S *aspect* is not only a felicitous word in relation to a plane, but it is susceptible of a wider application than that which he proposes for it, since it expresses a fundamental idea in the theory of surfaces. Every *surface* has at every point an *aspect*, which is the direction of a normal at that point. This may be regarded as the first property of surfaces, for if we define a surface as that form of extension which has at every part two and only two dimensions, we virtually say that, among all the directions in space that radiate from any point of the surface, there is one and only one perpendicular to all those (infinite in number) that lie within the surface at that point; in other words, that the surface has a normal at every point. A plane is then a continuous surface which has *the same aspect throughout*, the angle of two planes is the measure of *their difference in respect of aspect;* parallel planes (as Mr. Wilson points out) are those which have *the same aspect*, a plane tangent to a surface is one which contains a point of the surface, and has *the aspect of the surface at that point*, and a line tangent to a surface is one that contains a point of the surface, and has *a direction which lies within the surface* (or is perpendicular to the normal) *at that point.* Then *a straight line tangent to a plane lies wholly in the plane*, and if such a line, passing through any assumed point of a plane—rotate about that point—always remaining tangent to the plane, it must sweep *every point of the plane*, for it will generate a continuous and infinite surface coincident throughout its extent with the plane, and the plane, being continuous, can have no points without this surface. Therefore, *a straight line which joins two points of a plane lies wholly in the plane*, whence the propositions that *a plane is determined by three points*, and that *the intersection of two planes is a straight line*, together with the other elementary theorems of the geometry of space, are readily derived.

The use of *aspect* in the sense now proposed is not absolutely new, as Mr. Proctor (NATURE for October 26) seems to argue. It has the high authority of Sir W. R. Hamilton in his "Lectures on Quaternions" (1853). Thus we read on page 92 (the italics and capitals of the original are preserved):—"A biradial has also a PLANE and an ASPECT, depending on the *star* or *region* of infinite space, towards which its plane may be conceived to FACE. . . . When two bi-radials have, in the sense just now explained, the *same aspect*, their planes both facing at the same moment the *same star*, they may be said to be CONDIRECTIONAL BIRADIALS. When, on the other hand, they face in exactly *contrary* ways, and, therefore, have OPPOSITE ASPECTS, they may be called CONTRADIRECTIONAL. . . . Both these two latter classes may be included under the common name of PARALLEL BIRADIALS, so that the PLANES of any two *parallel biradials* are either coincident or parallel."

Vaguely, indeed, *aspect of a plane* may be used in the sense Mr. Proctor would assign it, as well as in several other senses. But if we could give it an exact and technical signification, that which is proposed by Mr. Laughton seems to issue directly from the proper meaning of the word; and it is a signification which no other word yet suggested will so easily bear. At present, therefore, it ought to be accepted as the very word that is needed in the re-construction of geometry.

As for *position*, it is pertinent to ask whether anyone would say that parallel planes have *the same position.* The attribute of planes, for which a word is demanded, is precisely that *element* of position in which parallel planes agree; and the *position* of a plane requires for its determination not that element only, but also some other element whereby the plane shall be distinguished from its parallels.

Permit me, by way of appendix to my too long note, to call the attention of those who are interested in the *early* teaching of Geometry, which has lately been discussed in your columns, to Dr. Thomas Hill's "First Lessons in Geometry. Facts before Reasoning." (Boston, 1856.)

J. M. PEIRCE

Cambridge, Massachusetts, Nov. 15

Cause of Low Barometric Pressure

IN the number of NATURE for July 20, 1871, I find a paper by Ferrel, "On the Cause of Low Barometer in the Polar Regions," &c. The author says that the law which deflects a body to the right in the northern hemisphere and to the left in the southern is not understood by meteorologists, and that it is admitted only when the movement is north and south.

I believe this law is now admitted by almost all meteorologists. The proof of it is the general acceptance of Buys Ballot's law of winds, which states that the wind will always blow towards a barometrical depression, and be deflected to the right in the northern hemisphere.

The most important meteorological works of the last years are based on this principle, as, for example, Buchan's "Mean Pressure and Prevailing Winds," and Mohn's "Storm Atlas." Mr. Mohn states the error which was committed in former times, and gives the expression of the deflecting force (page 17).—15°. sin L. (latitude) per hour. As to Mr. Ferrel's explanation of the low barometer at the poles, I must first state that it is not lowest near the poles. In the northern hemisphere, the lowest pressures are near Iceland and near the Aleutian islands, but northwards they are higher, as the observations of Greenland have shown, as is seen also in the prevalence of N.E. winds in winter at Stykkissholm (Northern Iceland); this would indicate that the pressure to the north and north-west of the last place is higher.

The great barometric depressions which so often visit Iceland cannot exist at temperatures of some degrees below freezing point. This explains why the barometer cannot be lower at the Arctic Pole than near Iceland in winter; the temperature there must be certainly much lower, even if the pole be surrounded by open water.

It is the low temperature also that explains the course of the Atlantic storms across European Russia (from N.W. to S.E.), as the winter temperature of Siberia is too low to admit the storms. This was already stated by Mr. Mohn, and I can but confirm his opinion.* In southern latitudes the barometrical depression seems to increase towards the pole, but do we know enough of these regions to say that the lowest barometer will be at the pole? In the highest southern latitudes attained by Sir James Ross the barometer was a little higher than northward. All that we know about the origin and propagation of barometrical depressions gives us the right to say that pressure cannot be lowest at the south pole, but that, as in the northern latitude, the greatest depression will be found at some distance from the pole, perhaps as far as the Antarctic Circle.

St. Petersburg, November 28 A. WOJEIKOFER

Symbols of Acceleration

I WISH to direct the attention of the reviewer of the "New Works on Mechanics," in No. 107 of NATURE, to the following statements which he makes while speaking of Wernicke's book:—"The symbol j is here and throughout the work used to denote an acceleration; for example $j\,x$ (*sic*) is the acceleration parallel to the axis of x. This notation (unfamiliar to English readers) has obvious advantages when the more appropriate language of the differential calculus cannot be employed."

Now I cannot see how the notation is "unfamiliar to English readers," when we have in common use α to denote an acceleration, and α_x an acceleration parallel to the axis of x. Again, though I agree with the reviewer that j_x (or the English α_x) "has obvious advantages when the more appropriate language of the Differential Calculus cannot be employed," yet it should be remembered that there is a more appropriate notation still, viz., that of Newton's Fluxions, recalled to its proper position in mixed mathematics by Sir W. Thomson (see Thomson's and Tate's "Nat. Phil.") and beginning to spread, in which $\frac{d^2 x}{d t^2}$ or an acceleration parallel to the axis of x is denoted by $\ddot{x}$. This notation can be employed at all stages of the student's progress, for it is as easy for him to learn that acceleration parallel to the axis of z, actual acceleration in the path, &c., are denoted by $\ddot{z}$, $\ddot{s}$, &c., as to make himself acquainted with Wernicke's symbols. Afterwards, when studying the Differential Calculus, he may be told the name of the notation, and have his knowledge of it enlarged, but he will never need to unlearn it; on the contrary, he will

* See also my paper "On Barometrical Amplitudes," in the *Journal of the Austrian Meteorological Society*, 1871, No. 10.

find its service increase in importance as he makes his way into the highest parts of the subject.

Of course no attempt is here made to attack D-ism, but to state that it and Dot-ism have their proper spheres, the latter generally, with more or less appropriateness, throughout the whole realm of functions, the former in the realm of motion, where the functions are functions of *t*—the sway over which realm was originally given to it by Newton, and acknowledged, as I have been told, by the D-ist Lagrange.

Glasgow College THOMAS MUIR

Occurrence of the Eagle Ray

A DOUBLE-SPINED specimen of the eagle ray (*Myliobatis aquila*), taken in Torbay on the 1st Nov., has been presented to this museum by Mr. Frank Gosden, fish dealer, High Street, Exeter. Its dimensions are as follows :—Breadth across the fins, 2ft. $3\frac{1}{2}$in. ; length from snout to the base of the spines, 1ft. $7\frac{3}{4}$in. ; total length from snout to extremity of the tail, 3ft. $6\frac{1}{2}$in. W. S. M. D'URBAN, Curator

Albert Memorial Museum, Exeter

Deep Sea Dredging

WHILE winter allows of time for complete arrangements to be made in anticipation of dredging weather, will you permit me to raise the question of the conditions under which our knowledge of the natural history of the sea may be most readily extended ?

As a rule, yacht owners object to the fatigue and dirt of dredging, but as we have the successful example of the *Norna*, may we not hope that other yachts may further the cause of science, if assistance in the way of instruction or apparatus be afforded to them by those having the necessary experience and means ?

The idea of now urging the question is not mine alone, but is entertained by many ardent naturalists who are much in favour of a skilful search of our seas at home, as well as of the Mediterranean and other distant and almost untried seas.

Your pages have often borne witness to the interest and importance attaching to marine zoology, and if men of practical experience, such as Carpenter, W. Thomson, Marshall Hall, &c., will indicate the best localities for search and the best measures to adopt, we may hope that others may follow in their steps, and that the large aquaria now built and building will be supplied, as only private zeal and enterprise can compass, with new and rare specimens from deep waters.

T. H. HENNAH

Milton House, Clarence Street, Brighton, Dec. 5

The Solar Halo

THE solar halo of the morning of the 13th ult. described in your last number as seen near, and at about thirty miles from, Durham, and which Prof. A. S. Herschel conjectures may have been seen from more distant stations, was visible here.

I first saw it at about 8 A.M., when it appeared as the arc of a circle, with a very short portion of an inverted arc touching it at the vertex—the sun itself being hidden by a bank of cloud, from behind which issued several radiating spikes. Shortly after half-past nine this halo had disappeared, except a small portion at the point of contact of the two arcs, vertically over the sun, which appeared like a bright elongated patch, forked at each end, and projected not on mist, but on blue sky, and tinged with dull prismatic colours, which were most strongly marked in the inverted arc, in which the red or orange was downwards, or on the outside of the circle. I then suddenly caught sight of a second halo, of much greater radius than the first—visible through perhaps 130° or 140° of arc, above, and to the right of, the sun, projected on the clear blue sky, but so faintly that it might easily have been missed. This outer circle exhibited the prismatic colours with a purity and delicacy that I have never before seen in halos, and which was quite different to the ordinary dull, muddy colours. In fact, it had just the appearance of a very faint and *narrow* rainbow, the red being inside, and the blue outside the circle. I was shortly after able to borrow a sextant, and measured the distance from the sun to the bright patch and the outer circle, which appeared respectively 21° 40′ and 43° 20′ ; but they were already growing so faint that I was unable to do this with much precision. Except the bright patch before named, I did not observe any appearance of "mock-sun."

Cardiff, Dec. 4 GEO. C. THOMPSON

ON THE ZIPHIOID WHALES

THE peculiar division of Cetaceans to which the term "Ziphioid" is now commonly applied, from one of the earliest known forms, *Ziphius* of Cuvier,* is in many respects one of the most interesting of the order. They form a very compact group, united closely together by the common possession of very definite structural characters, and as distinctly separated from all other groups by equally definite characters.

With the singular exception of *Hyperoodon rostratus* (the structure and habits of which species are as well known, perhaps, as those of any other cetacean), no specimen of the group had ever come under the notice of any naturalist up to the commencement of the present century. Since that time, however, at irregular intervals, in various and most distant parts of the world, solitary individuals have been caught or stranded, now amounting to nearly thirty, these being by some naturalists referred to upwards of a dozen distinct species and to very nearly as many genera. No case is recorded of more than one of these animals having been observed at one place at a time, and their habits are almost absolutely unknown. Their very presence in the ocean seems to pass unnoticed and unsuspected by voyagers, and even by those whose special occupation is the pursuit and capture of various better known and more abundant cetaceans, until one of the accidental occurrences just alluded to reveals the existence of forms of animal life of considerable magnitude, and at least sufficiently numerous to maintain the continuity of the race.

This comparative rarity at the present epoch contrasts greatly with what at one time obtained on the earth, especially in the period of the crag formations, and leads to the belief that the existing ziphioids are the survivors of an ancient family which once played a far more important part than now among the cetacean inhabitants of the ocean, but which have been gradually replaced by other forms, and are themselves probably destined ere long to share the fate of their once numerous allies or progenitors.

The Ziphioid whales belong to the great primary division or sub-order of the Odontocetes or Toothed whales, as distinguished from the Whalebone whales. They are allied on the one hand to the Cachalots or Sperm whales, and on the other to the true Dolphins and Porpoises, but more nearly to the former than the latter. They are animals varying between fifteen and thirty feet in length, and in external characters very closely resemble each other, all having small pointed snouts or "beaks," small rounded or oval pectoral fins or "flippers," a comparatively small triangular dorsal fin, situated considerably behind the middle of the back, and a single "blowhole" of concentric form, situated in the middle of the top of the head One of their most obvious characteristics, distinguishing them from the true dolphin, is the complete absence of teeth (except occasionally a few mere rudiments concealed in the gum) in the upper jaw, while in the lower jaw there is usually but a single pair, which in some species may be greatly developed and project like tusks from the mouth, though sometimes even these are rudimentary and covered up by the gum, so that the animal is practically toothless. In addition to these external and easily-recognised characters, there are others connected with the skeleton and internal organs which separate them still more trenchantly from the other members of the order. Their food appears

* "J'appliquerai au genre dont elle (a skull found on the shore of the Mediterranean) devient le premier type, le nom de Ziphius, employé par quelques auteurs du moyen age (Voyez Gesner I., p 209) pour un cétacé qu'ils n ont point déterminé" (Cuvier, "Ossemens fossiles"). According to strict rules of priority "Hyperodontoid" would be the more correct term, as Hyperoodon was the first genus of the group distinctly characterised; but as the name is erroneous in its signification, it will be better to keep to the more generally adopted and less objectionable term of "Ziphioid," first applied by Gervais. The group is equivalent to Eschricht's "Rhynchoceti."

to consist almost exclusively of cephalopods, or cuttlefish-like animals.

One of the greatest obstacles to acquiring a more accurate knowledge of this group is the excessively confused state of the nomenclature of the different animals of which it is composed. Nearly every single specimen that has been met with has been described under a different name, and before their characters and affinities were understood they were bandied about from one genus to another, even different individuals of the same species having been placed by systematists in different genera, until it has become almost impossible to write or speak of any of them, without the fear of inadvertently adding to the perplexity of those that come after, by adopting and perpetuating some ill-chosen or incorrect term.

In a valuable recent memoir on the subject by Prof. Owen,* the difficulty is disposed of in a very summary manner by uniting all the known forms, both recent and extinct (with the exception of *Hyperoodon*), under the generic name of *Ziphius*. This proceeding, at all events, has the merit of running no risk of adding to the confusion of nomenclature, caused by hasty or ill-defined generic subdivisions, founded on imperfect or fragmentary knowledge of the animal described. But, however great our admiration may be for this strong-handed resistance to the passion for name-coining, which is fast rendering the study of zoology almost an impossibility, it must not lead us to overlook well-marked structural characteristics by which certain small groups of species are allied together, and differentiated from others, whether we call them genera or by any other term.

In a paper recently presented to the Zoological Society (read Nov. 7), I have given reasons for my belief that the species of ziphioids at present known (I refer only to those now existing, not to the extinct forms), may be naturally arranged by certain structural characters, especially the conformation of the skull and teeth, into four groups; and as, so far as is yet known, these are not united by intermediate forms, they may, I think, be considered as generic, though of course this is a subject upon which the judgment of different zoologists may differ. This arrangement does not differ from that adopted by several other zoologists, who have specially studied the animals of this group, but the characteristics of each section or genus have not hitherto been clearly defined.

It is not my present purpose to enter into the details of these characteristics, for which I must refer to the above-mentioned communication, but to give a short summary of the known zoological facts relating to the different animals of which each is composed, so that a general idea may be gained of our present state of knowledge of the whole group.

I. Genus *Hyperoodon*, Lacépède.—This genus differs from the rest in having a very prominent convex "forehead" as it appears externally, though really corresponding to the lower part of the face of other animals, supported by strong bony crests on the maxilla, and below which the small pointed snout projects, something like the neck of a bottle from its shoulder, hence the name "Bottle-nose" often applied to these animals, in common with various other cetaceans. The common Hyperoodon (*H. rostratus*) is, as before mentioned, one of the best known of cetaceans, being a regular visitor to our coasts, and having been frequently described and figured by naturalists who have had opportunities of observing it in a fresh state. Skeletons, moreover, are to be seen in nearly every considerable osteological museum. The first really good description and figure is that of John Hunter, founded on an individual which was caught in the Thames near London Bridge, in the year 1783, and the skeleton of which still hangs in the great hall of the Museum of the Royal College of Surgeons. The figure of the animal appears in the Philosophical Transactions for 1787. Among the numerous subsequent contributions to the knowledge of the structure and natural history of this species, the monographs of Vrolik and of Eschricht are of especial importance.

The common Hyperoodon attains the length of twenty to twenty-five feet. It has no functional teeth, the only two which it possesses are quite small and buried in the gum at the front end of the lower jaw, but the palate is beset with numerous minute horny points. As in many other whales in which the teeth are either absent or very rudimentary when adult, it possesses a complete set at a very early period of its growth, but the majority of these disappear even before birth. Judging by the contents of the stomach of the captured specimens, their food consists of several kinds of squid and cuttlefish, and not of true fish; they are, therefore, not the enemies to fishermen that some have supposed them, but rather the reverse, for the cuttles, of which they destroy great quantities, are themselves voracious fish-eaters. In geographical range this species is limited to the North Atlantic, having been found both on the American and European coasts, extending as far north as Greenland, but its southern limit has not been accurately determined; it has, however, never been known to enter the Mediterranean. Within this range it is migratory, spending the summer in the Polar seas and the winter in the Atlantic, and it is chiefly on its passage northwards in the spring and southwards in the autumn that it visits our shores. It happens almost every year that in the last-named season one or more are stranded on some part of the extensive coastline of the British Isles; usually a female accompanied by a young one, seeking probably for food in too shallow water, are cut off by the retreating tide from their chance of regaining the open sea. In these cases it appears that it is the less experienced younger animal which gets into danger, and is then rarely abandoned by the old one, who thus falls a victim to the strength of the maternal instinct so largely developed in the cetacea. The old males are apparently more wary, and rarely approach the shore near enough to be taken. They are never seen in herds or "schools" like so many of their congeners, but always either singly or in pairs.

Another animal, allied to *Hyperoodon rostratus* but of larger size, being fully thirty feet in length, and of heavier proportions, has been occasionally met with in the North Seas, and is generally supposed to be another species of the same genus (*H. latifrons*), though some naturalists have maintained that it is nothing more than the old male of the former.

II. Genus *Ziphius*.—The type of this genus is *Z. cavirostris* of Cuvier, founded on an imperfect skull picked up in 1804 on the Mediterranean coast of France, near Fos, Bouches-du-Rhône, and described and figured in the "Ossemens Fossiles." It was at first supposed to be a fossil, but has since been proved to belong to a species still living in the Mediterranean, and there is no evidence that the skull is of ancient date.

2. An animal of the same species was afterwards taken on the coast of Corsica; its external characters are described and figured by Doumet in the *Revue Zoologique*, v. 1842, p 208, and its skeleton is preserved at Cette. 3. A third specimen was stranded near Aresquiers, Hérault, South France, in 1850; the skull, which is now in the Museum at Paris, has been described by Gervais and Duvernoy (*Annales des Sciences Naturelles*, 3 series, 1850 and 1851). 4 In the Museum of Arcachon is a skull found on the beach at Lanton, Gironde, West France, in 1864, and described and figured by Fischer, in the *Nouvelles Archives du Muséum*, tome 3, 1867. 5. A complete skeleton of an adult animal is mounted in the Anatomical Museum of the University of Jena. This was obtained at Villa Franca in 1867 by Prof. Haeckel, but has not yet been described. 6. In the Museum of

* British Fossil Cetacea from the Crag. Palæontological Society, vol. xxiii., 1870.

the University of Louvain is a skull of an animal of this genus, brought from the Cape of Good Hope, of which a description has been published by Prof. Van Beneden, under the name of *Ziphius indicus* (Mem. de l'Acad. Roy. de Belgique, coll. in 8vo, 1863). 7. A very similar skull in the British Museum, also from the Cape of Good Hope, has been described by Gray (Proc. Zool. Soc. 1865, p. 524) by the name of *Petrorhyncus capensis*. 8. A complete specimen of a young male, thirteen feet long, was taken near Buenos Ayres in 1865, and is the subject of an elaborate memoir by Burmeister (Annales de Museo Publico de Buenos Aires, Vol. i. p. 312, 1869), accompanied by detailed figures of external characters, skeleton, and some of the viscera. The specimen was first named in a preliminary notice *Ziphiorhynchus cryptodon*, but subsequently described as *Epiodon australis*.

Such are the materials upon which our knowledge of the genus *Ziphius* is based. For the present it is impossible to determine whether the differences that have been noticed in the above-mentioned specimens are the result of age, sex, or individual peculiarity, or whether they denote specific distinctions. The remains that are preserved indicate in every case an animal of rather smaller size than the Hyperoodon.

III.—Genus *Mesoplodon*, Gervais. It is not without some hesitation that I assign this designation to the present well-marked section, as it is extremely difficult to determine which of the numerous names which have been given to it by various authors should have the preference. The type-species of the group, Sowerby's whale, has had no less than eleven generic appellations given to it since its discovery in 1804, viz., *Physeter*, *Delphinus*, *Heterodon*, *Dioplon*, *Aodon*, *Nodus*, *Delphinorhynchus*, *Micropteron*, *Mesoplodon*, *Mesodiodon*, and *Ziphius!* Many of these names had to be abandoned almost as soon as they were bestowed, as their authors had overlooked the fact that they had been previously appropriated to other members of the animal kingdom. To give a full account of the entangled literary history of the genus would occupy too much space for the present communication, so I will content myself with enumerating the specimens which are referable to it, as far as they are known to me, existing in various museums, from which some idea of the frequency of occurrence and of the geographical distribution of the animals will be obtained. They are rather more numerous than those of *Ziphius*.

1. An imperfect skull in the University Museum, Oxford, from an animal (a male) sixteen feet long, obtained on the coast of Elginshire, figured and described by Sowerby (*British Miscellany*, p. 1, 1804) under the name of *Physeter bidens*, but to which the specific name of *Sowerbyi* has since been generally attached. (This is *Delphinus* (*Heterodon*) *Sowerbensis* of De Blainville, Nouv. Dict. d'Hist. Nat., t. ix., 1817, Second edition; *D. Sowerbyi* Desmarest, Mammalogie, 1822.) 2. A skull in the Paris Museum from a female specimen fifteen feet long, stranded at Havre, Sept. 9, 1825, described by De Blainville (Nouv. Bulletin. Sc. t. iv., 1825) as the "Dauphin du Dale," by Cuvier as *Delphinus* (*Delphinorhynchus*) *micropterus*, and afterwards by a variety of other names, but now generally considered to be specifically identical with the first mentioned. 3. A complete skeleton in the Brussels Museum from a young specimen stranded at Ostend, August 31, 1835. 4. A skull and part of skeleton in the Museum at Caen from Sallenelles, Calvados, North France, 1825. 5. Mutilated skull in the Museum of the Royal Dublin Society, from an animal fifteen feet long, stranded in 1864 in Bandon Bay, Kerry, Ireland. 6. Another skull and some bones in the same museum from a second specimen from the same locality, in 1770. 7. A lower jaw in the Christiana Museum, from the Coast of Norway. 8. A skull in the University Museum, Edinburgh, of unknown origin. (I am indebted to Prof. Van Beneden for information about this specimen, which has not hitherto been recorded.) All these appear to belong to one species. The adult males have a single triangular compressed tooth on each side, rather in front of the middle of the lower jaw, which projects beyond the lip like a tusk, working against a hard callous pad in the upper jaw. In the specimen from Calvados, a group of barnacles had attached themselves to the outer surface of the tooth. 9. In the British Museum is a skull received from the Cape of Good Hope, with teeth in a similar situation, but developed to such an extent as to pass (curving upwards, backwards, and finally inwards) all round the upper jaw, and actually to meet above, preventing the mouth from opening beyond a very few inches at most. It is very difficult to imagine how the animal could have lived and obtained food in this condition, and it might well be supposed to be an individual deformity, but Mr. E. Layard has shown me a tooth of another individual having exactly the same conformation, and being upwards of a foot in length. To this species the name of *Layardii* has been applied by Dr. Gray. 10. An animal probably of the same species, but with the tooth much less developed (? a female), was very lately stranded at Little Bay, about six miles from Sydney, and its skeleton is now in the Australian Museum. 10. In the Museum at Caen there is another skull, from an animal caught in the entrance of the Channel about 1840, which appears to belong to a different species from those ordinarily found on our coasts, as the compressed tooth is placed nearer the apex of the jaw. 12. A skull in the Museum at Paris, remarkable for the peculiar form of the lower jaw, and of the heavy massive tooth which it supports, obtained from the Seychelle Islands, has received the specific name of *densirostris*, and very recently a complete skeleton of the same (13), obtained by Mr. Krefft from Lord Howe's Island, has been added to the Sydney Museum, already rich in skeletons of rare Cetaceans. Lastly (14), in the Museum at Wellington, New Zealand, is a skull and some bones of an animal, nine feet long, which was killed in Titai Bay, Cook's Strait, January 1866, and figured by Dr. Hector in the "Transactions of the New Zealand Institute," vol. iii., part xv., of which the conformation of the skull shows that it is a member of this group: but the single compressed tooth in the lower jaw is situated farther forwards than in any other known species, thus completing the series of different positions in the side of the ramus occupied by the developed teeth, and proving its small value as a generic character.

IV.—*Berardius*, Duvernoy. This genus was founded by Duvernoy upon a skull received at the Museum of Paris in 1846, having been obtained from an animal stranded in Akaroa Harbour, New Zealand. In the name of *Berardius Arnuxii* conferred upon it by Duvernoy, the captain of the French corvette, *Le Rhin*, Bérard, and the surgeon, Arnoux, who jointly presented the specimen, with some others of considerable interest to the Museum, are commemorated in zoological literature.

Only three other specimens of this animal have since been seen, and all on the coasts of New Zealand:—One in 1862, embayed in Porirua Harbour, was converted into oil, and can only be conjectured to have been a Berardius by its dimensions, and a slight description published by Mr. Knox. In January 1870 another was taken in Worser's Bay near the entrance to Port Nicholson, and its skull and some bones were preserved for the Wellington Museum; and, lastly, a specimen of this fine animal, which is thirty feet long, and, after *Hyperoodon latifrons*, the largest of the group, ran aground on the beach near New Brighton, Canterbury, on the 16th of December, 1868, where it fortunately came under the notice of Dr. Julius Haast, F.R.S., the energetic and able geologist, and Curator of the Museum at Christ Church. The details of its capture are given by Dr. Haast in the Proceedings of the Philosophical Institute of Canterbury, New Zealand, May 5, 1869, and also in the "Annals and Mag. Nat. Hist." October 1870.

The skeleton of this animal has been lately placed among the fine series of Cetaceans in the Museum of the Royal College of Surgeons, thanks to the extremely liberal desire of Dr. Haast that it should be made as available as possible for scientific examination, comparison, and description, and to the generosity of Mr. Erasmus Wilson, F.R.S., a member of the Council of the College, in providing the means of adding it to the collection without expense to the Institution. A detailed and fully illustrated description of this skeleton formed part of the communication to the Zoological Society alluded to above, and will appear shortly in the "Transactions." All the characters of the skeleton agree generally with those of the other Ziphioids, but it appears in some respects to be a less specialised form, approaching somewhat nearer to the true dolphins, while *Hyperoodon* is at the other extremity of the series, being modified in the direction of the sperm whales. It has two teeth on each side of the lower jaw, situated near the front end or symphysis, which show nearly the same characteristic and peculiar structure as that described by Mr. Ray Lankester in the teeth of *Mesoplodon Sowerbyi*. The skull is far more symmetrical than in any other member of the group, and wants the great maxillary crests of *Hyperoodon*, and the dense ossification of the rostrum found in so many of the others. The cervical region is comparatively long, with the majority of its vertebræ free, the dorsals and ribs are ten in number, the lumber and caudals thirty-one, making forty-eight in all. Viewing the skeleton as a whole, the most striking feature is the small size of the head compared with the great length of the vertebral column, and the massiveness of the individual bones, especially of the lumbar and anterior caudal vertebræ. It presents in this respect a remarkable contrast to the sperm whale, which hangs near it in the museum, though agreeing generally with the other Ziphioids. As before mentioned, it is thirty feet in length, and, as Dr. Haast was able to observe, it agrees with its congeners in the nature of its food, for its stomach was found to contain about half a bushel of the horny beaks of cephalopods. The colour of the whole animal when fresh was of a deep velvety black, with the exception of the lower portion of the belly, which was greyish.

Extinct Ziphioids.—To the circumstance of the extreme density of the rostral portion of the skull of certain Ziphioids, owing to the firm ossification of the mesethmoid cartilage and its coalescence with the surrounding bones (the maxilla, premaxilla, and vomer) our knowledge of many of the ancient members of this group of whales is due. When all other portions of the skeleton have yielded to the destructive influence of time, these rostra, generally in the form of elongated and somewhat flattened cylinders, worn and eroded by the action of water, gravel, and sand, occasionally come to light to attest the presence of a former world of oceanic life. A few teeth also have been found which would appear to be referable to these same animals. The localities in which these occur in England are the Red Crag deposits of Suffolk. They are still more abundant, and in a much more perfect condition in the beds of corresponding age in the neighbourhood of Antwerp, which have fortunately been laid bare by the excavations made in the defensive works of that city. A magnificent series of these fossils containing many new forms has recently been added to the Brussels Museum, but until M. le Vicomte Du Bus, the accomplished late Director of the Museum, has completed the great task he has undertaken of determining and describing them, they are as little available for zoological science as if they still lay

In the bottom of the deep
Where fathom line could never touch the ground.

W. H. FLOWER

CONTINUITY OF THE FLUID AND GASEOUS STATES OF MATTER *

WHEN we find a substance capable of existing in two fluid states different in density and other properties, while the temperature and pressure are the same in both; and when we find also that an introduction or abstraction of heat without change of temperature or of pressure will effect the change from the one state to the other; and also find that the change either way is perfectly reversible, we speak of the one state as being an ordinary gaseous and the other as being an ordinary liquid state of the same matter; and the ordinary transition from the one to the other we would designate by the terms boiling, or condensing; or occasionally by other terms nearly equivalent, such as evaporation, gasification, liquefaction from the gaseous state, &c. Cases of gasification from liquids, or of condensation from gases, when any chemical alteration accompanies the abrupt change of density, are not among the subjects proposed to be brought under consideration in the present paper. In such cases I presume there would be no perfect reversibility in the process; and if so, this would of itself be a criterion sufficing to separate them from the proper cases of boiling or condensing at present intended to be considered. If now the fluid substance, in the rarer of the two states—that is, in what is commonly called the gaseous state—be still further rarefied, by increase of temperature or diminution of pressure, or be changed considerably in other ways by alterations of temperature and pressure jointly, without its receiving any abrupt collapse in volume, it will still, in ordinary language and ordinary mode of thought, be regarded as being in a gaseous state. Remarks of quite a corresponding kind may be made in describing various conditions of the fluid (as to temperature, pressure, and volume), which would in ordinary language be regarded as belonging to the liquid state.

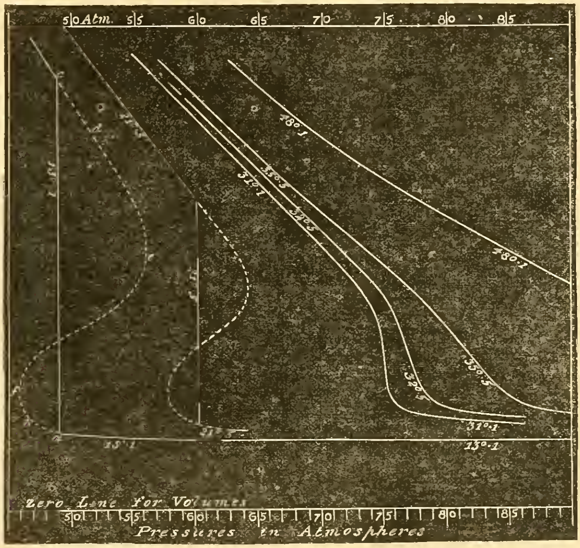
Dr. Andrews (Phil. Trans. 1869) has shown that the ordinary gaseous and ordinary liquid states are only widely separated forms of the same condition of matter, and may be made to pass into one another by a course of continuous physical changes presenting nowhere any interruption or breach of continuity. If we denote geometrically all possible points of pressure and temperature jointly by points spread continuously in a plane surface, each point in the plane being referred to two axes of rectangular coordinates, so that one of its ordinates shall represent the temperature, and the other the pressure denoted by that point; and if we mark all the successive boiling- or condensing-points of temperature and pressure as a continuous line on this plane; this line, which may be called the boiling line, will be a separating boundary between the regions of the plane corresponding to the ordinary liquid state and those corresponding to the ordinary gaseous state. But, by consideration of Dr. Andrews's experimental results, we may see that this separating boundary comes to an end at a point of pressure and temperature, which, in conformity with his language, may be called the critical point of pressure and temperature jointly; and we may see that, from any ordinary liquid state to any ordinary gaseous state, the transition may be effected gradually by an infinite variety of courses passing round outside the extreme end of the boiling line.

Now it will be my chief object in the present paper to state and support a view which has occurred to me, according to which it appears probable that, although there be a practical breach of continuity in crossing the line of boiling-points from liquid to gas or from gas to liquid, there may exist in the nature of things a theoretical continuity across this breach, having some real and true significance. This theoretical continuity, from the ordinary liquid state to the ordinary gaseous state, must be supposed to be such as to have its various courses passing through conditions of pressure, temperature, and volume in unstable equilibrium for any fluid matter theoretically conceived as homogeneously distributed while passing through the intermediate conditions. Such courses of transition, passing through unstable conditions, must be regarded as being impossible to be brought about throughout entire masses of fluids dealt with in any physical operations. Whether in an extremely thin lamina of gradual transition from a liquid to its own gas, in which it is to be noticed the substance would not be homogeneously distributed, conditions may exist in a stable state, having some kind of correspondence with the unstable conditions here theoretically conceived,

* "Considerations on the abrupt change at boiling or condensing in reference to the Continuity of the Fluid State of Matter," by Professor James Thomson, LL.D., Queen's College, Belfast, read before the Royal Society, Nov. 16, 1871.

will be a question suggested at the close of this paper in connection with some allied considerations.

It is first to be observed that the ordinary liquid state does not necessarily cease abruptly at the line of boiling-points, as it is well known that liquids may with due precautions be heated considerably beyond the boiling temperature for the pressure to which they are exposed. This condition is commonly manifested in the boiling of water in a glass vessel by a lamp placed below, when the temperature of the internal parts of the water, or, in other words, of the parts not exposed to contact with gaseous matter, rises considerably above the boiling-point for the pressure, and the water boils with bumping.* At this stage it becomes desirable to refer to Dr. Andrews's diagram of curves, showing his principal results for carbonic acid, and to consider carefully some of the remarkable features presented by those curves. In doing so, we have first, in the case of the two curves for 13°·1 and 21°·5 which pass through the boiling interruption of continuity, to guard against being led by the gradually bending transition from the curve representing obviously the liquid state into the line seen rapidly ascending towards the curve representing obviously the gaseous state, to suppose that this curved transition is in any way indicative of a gradual transition from the liquid towards the gaseous state. Dr. Andrews has clearly pointed out, in describing those experimental curves, that the slight bend at about the commencement of the rapid ascent from the liquid state is to be ascribed to a trace of air unavoidably present in the carbonic acid; and that if the carbonic acid had been absolutely pure, the ascent from the liquid to the gaseous state would doubtless have been quite abrupt, and would have shown itself in his diagram by a vertical straight line, when we regard the co-ordinate axes for pressures and volumes as being horizontal and vertical respectively. Now in the diagram here submitted, the continuous curves (that is to say, those which are not dotted) are obtained from Dr. Andrews's diagram with the slight alteration of substituting, in accordance with the explanations just given, an abrupt meeting instead of the curved transition between the curve for the liquid state and the upright line which shows the boiling stage. Looking to either of the given curves which pass through boiling, and, for instance, selecting the curve for 13°·1, we perceive, from what has been said as to



the conditions to which boiling by bumping is due, that for the temperature pertaining to this curve the liquid state does not necessarily end at the boiling pressure for this temperature; and that thus in the diagram the curve showing volumes for the liquid state must not cease at the foot of the upright line which marks the boiling stage of pressure, but must extend continuously, for some distance at least, into lower pressures in some such way as is shown by the dotted continuation from *a* to *b*. But now the question arises, Does this curve necessarily end at any particular point *b*? We know that the extent of this curve in the direction from *a* towards or past *b*, along which the liquid volume will continue to be represented before the explosive or bumping change to gas occurs, is very variable under different circumstances, being much affected by the presence of other fluids, even in small quantities, as impurities in the fluid experimented on, and by the nature of the surface of the containing vessels, &c.

The consideration of the subject may be facilitated, and aid towards the attainment of clear views of the mutual relations of temperature, pressure, and volume in a given mass of a fluid may be gained, by actually making, or conceiving there to be made, for carbonic acid, from the data supplied in Dr. Andrews' experimental results, a solid model consisting of a curved surface referred to three axes of rectangular co-ordinates, and formed so that the three co-ordinates of each point in the curved surface shall represent, for any given mass of carbonic acid, a temperature, a pressure, and a volume which can co-exist in that mass. It is to be noticed here that in his diagram of curves the results for each of the several temperatures experimented on are combined in the form of a plane-curved line referred to two axes of rectangular co-ordinates, one of each pair of co-ordinates representing a pressure, and the other representing the volume corresponding to that pressure at the temperature to which the curve belongs. Now to form a model such as I am here recommending, and have myself made, Dr. Andrews' curved lines are to be placed with their planes parallel to one another, and separated by intervals proportional to the differences of the temperatures to

* It has even been found by Dufour (Bibliothèque Universelle, Archives, year 1861, vol. xii. "Recherches sur l'Ebullition des Liquides") that globules of water floating immersed in oil, so as neither to be in contact with any solid nor with any gaseous body, may, under atmospheric pressure, be raised to various temperatures far above the ordinary boiling-point, and occasionally to so high a temperature as 178° C. without boiling. On this subject reference may also be made to the important researches of Donny, "Sur la Cohésion des Liquides et sur leur Adhérence aux Corps solides," Ann. de Chimie, year 1846, 3rd ser. vol xvi. p. 167,—July 28, 1871.

which the curves severally belong, and with the origins of co-ordinates of the curves situated in a straight line perpendicular to their planes, and with the axes of co-ordinates of all of them parallel in pairs to one another, and then the curved surface is to be formed so as to pass through those curved lines smoothly or evenly.* The curved surface so obtained exhibits in a very obvious way the remarkable phenomena of the voluminal conditions at and near the critical point of temperature and pressure, in comparison with the voluminal conditions throughout other parts of the range of gradually varying temperatures and pressures to which it extends, and even throughout a far wider range into which it can in imagination be conceived to be extended. It helps to afford a clear view of the nature and meaning of the continuity of the liquid and gaseous states of matter. It does so by its own obvious continuity throughout its expanse round the end of the range of points of pressure and temperature where an abrupt change of volume can occur by boiling or condensing. On the curved surface in the model Dr. Andrews' curves for the temperatures 13°·1, 21°·5, 31°·1, 32°·5, 35°·5 and 48°·1 Centigrade, which afford the data for its construction, may with advantage be all shown drawn in their proper places. The model admits of easily exhibiting in due relation to one another a second set of curves, in which each would be for a constant pressure, and in each of which the co-ordinates would represent temperatures and corresponding volumes. It may be used in various ways for affording quantitative relations interpolated among those more immediately given by the experiments.

We may now, aided by the conception of this model, return to the consideration of continuity or discontinuity in the curves in crossing the boiling stage. Let us suppose an indefinite number of curves, each for one constant temperature, to be drawn on the model, the several temperatures differing in succession by very small intervals, and the curves consequently being sections of the curved surface by numerous planes closely spaced parallel to one another and to the plane containing the pair of co-ordinate axes for pressure and volume. Now we can see that, as we pass from curve to curve in approaching towards the critical point from the higher temperatures, the tangent to the curve at the steepest point or point of inflection is rotating, so that its inclination to the plane of the co-ordinate axes for pressure and temperature, which we may regard as horizontal, increases till, at the critical point, it becomes a right angle. Then it appears very natural to suppose that in proceeding onwards past the critical point, to curves successively for lower and lower temperatures, the tangent at the point of inflection would continue its rotation, and the angle of its inclination, which before was acute, would now become obtuse. It seems much more natural to make such a supposition as this than to suppose that in passing the critical point from higher into lower temperatures the curved line, or the curved surface to which it belongs, should break itself asunder, and should come to have a part of its conceivable continuous course absolutely deficient. It thus seems natural to suppose that in some sense there is continuity in each of the successive curves by courses such as those drawn in the accompanying diagram as dotted curves uniting continuously the curves for the ordinary gaseous state with those for the ordinary liquid state.

The physical conditions corresponding to the extension of the curve from *a* to some point *b* we have seen are perfectly attainable in practice. Some extension of the gaseous curve into points of temperature and pressure below what I have called the boiling, or condensing line, as for instance some extension such as from *c* to *d* in the figure, I think we need not despair of practically realising in physical operations. As a likely mode in which to bring steam continuing gaseous to points of pressure and temperature at which it would collapse to liquid water if it had any particle of liquid water present along with it, or if other circumstances were present capable of affording some apparently *requisite conditions for enabling it to make a beginning of the change of state*,† I would suggest the admitting speedily of dry steam nearly at its condensing temperature for its pressure (or, to use a common expression, *nearly saturated*) into a vessel with a piston or plunger, all kept hotter than the steam, and then allowing the steam to expand till by its expansion it would be cooled below its condensing point for its pressure; and yet I would suppose that if this were done with very careful precautions the steam might not condense, on account of the cooled steam being surrounded entirely with a thin film of superheated steam close to the superheated containing vessel. The fact of its not condensing might perhaps best be ascertained by observations on its volume and pressure. Such an experiment as that sketched out here would not be easily made, and unless it were conducted with very great precautions, there could be no reasonable expectation of success in its attempt; and perhaps it might not be possible so completely to avoid the presence of dust or other dense particles in the steam as to make it prove successful. I mention it, however, as appearing to be founded on correct principles, and as tending to suggest desirable courses for experimental researches. The overhanging part of the curve from *e* to *f* seems to represent a state in which there would be some kind of unstable equilibrium; and so, although the curve there appears to have some important theoretical significance, yet the states represented by its various points would be unattainable throughout any ordinary mass of the fluid. It seems to represent conditions of co-existent temperature, pressure, and volume, in which, if all parts of a mass of fluid were placed, it would be in equilibrium, but out of which it would be led to rush, partly into the rarer state of gas, and partly into the denser state of liquid, by the slightest inequality of temperature or of density in any part relatively to other parts. I might proceed to state, in support of these views, several considerations founded on the ordinary statical theory of capillary or superficial phenomena of liquids, which is dependent on the supposition of an attraction acting very intensely for very small distances, and causing intense pressure in liquids over and above the pressure applied by the containing vessel and measureable by any pressure-gauge. That statical theory has fitted remarkably well to many observed phenomena, and has sometimes even led to the forecasting of new results in advance of experiment. Hence, although dynamic or kinetic theories of the constitution and pressure of fluids now seem likely to supersede any statical theory, yet phenomena may still be discussed according to the principles of statical theory; and there may be considerable likelihood that conditions explained or rendered probable under the statical theory would have some corresponding explanation or confirmation under any true theory by which the statical might come to be superseded. With a view to brevity, however, and to the avoidance of putting forward speculations perhaps partly rash, though, I think, not devoid of real significance, I shall not at present enter on details of these considerations, but shall leave them with merely the slight suggestion now offered, and with the suggestion mentioned in an earlier part of the present paper, of the question whether in an extremely thin lamina of gradual transition from a liquid to its own gas, at their visible face of demarcation, conditions may not exist in a stable state having a correspondence with the unstable conditions here theoretically conceived.

* For the practical execution of this, it is well to commence with a rectangular block of wood, and then carefully to pare it down, applying, from time to time, the various curves as templets to it; and proceeding according to the general methods followed in a shipbuilder's modelling room in cutting out small models of ships according to curves laid down on paper as cross sections of the required model at various places in its length.

† The principle that "the particles of a substance, when existing all in one state only, and in continuous contact with one another, or in contact only under special circumstances with other substances, experience *a difficulty of making a beginning of their change of state*, whether from liquid to solid, or from liquid to gaseous, or probably also from solid to liquid," was proposed by me, and, so far as I am aware, was first announced in a paper by me in the Proceedings of the Royal Society for November 24, 1859, and in a paper submitted to the British Association in the same year. In the present paper, at the place to which this note is annexed, I adduce the like further supposition that *a difficulty of making a beginning of change of state* from gaseous to liquid may also probably exist.

ALTERNATION OF GENERATIONS IN FUNGI

IT has long since been shown that certain fungi pass through an alternation of generations on the same plant. The Rev. M. J. Berkeley demonstrated that in the case of the common "bunt" at least four consecutive forms of reproductive cellules were produced. In the majority of Uredines there are two forms of fruit, but these can scarcely be regarded as an alternation of generations, since there is no evidence that the spores of *Trichobasis* by germination, or otherwise, produce the bilocular spores of *Puccinia*. In *Podisoma* and *Gymnosporangium* (if the two genera are really distinct) the bilocular spores germinate freely and produce unilocular secondary spores. Prof.

Oersted contends that if these secondary unilocular spores are sown upon young plants of the *Sorbus aucuparia*, they will germinate, and that the ends of the germinating filaments penetrating the tissues of the leaf of the sorb will in turn produce the spermagonia and peridia of *Ræstelia cornuta*. This is very similar to the deductions of Prof. de Bary that the spores of the *Æcidium* which flourishes on the berberry may be employed to inoculate young plants of wheat, and will produce as a result the wheat-mildew (*Puccinia graminis*), which he contends is another generation of the berberry fungus completed upon a different host. (*See* NATURE, vol. ii. p. 318)

Such experiments as those of Professors Oersted and De Bary must always prove unsatisfactory unless performed with extraordinary care, and until confirmed by other observers. One or two strong presumptions can always be urged against them, and require to be boldly faced. Wheat is very subject to the attacks of mildew (*Puccinia*), and the results claimed for certain experiments are that they have produced by inoculation with other spores the common *Puccinia* upon wheat plants, to which the wheat is particularly addicted all the world over. Admitting that the *Æcidium* spores sown on the leaves of young wheat plants germinate, and that the germinating filaments enter the tissues of the leaf, are we therefore justified in affirming, or admitting that the inoculating spores produce the *Puccinia* which ultimately exhibits itself? Is it not more feasible to believe that the germination of the foreign spores have only served to stimulate the latent germs of the *Puccinia* already present in the tissues of the wheat plant? What guarantee is afforded by those who have already experimented, that the wheat plants experimented upon would not ultimately, without inoculation, have developed precisely the same parasite as that supposed to have been produced by inoculation? Assuming also that the experiment was pursued in the opposite direction, and that the spores of the wheat mildew were sown upon young plants of the berberry, if the *Æcidium* should soon afterwards appear on the leaves, it is easy enough to jump to the conclusion that they were produced by inoculation, but assumption is insufficient since the berberry is very subject, year after year, to bear on some of its leaves the peridia of the *Æcidium*. What evidence could be given that the *Æcidium* would never have appeared but for the inoculation? It is manifest that no amount of care in cultivation under bell glasses or other exclusion from foreign influences is sufficient against a contingency which dates back to the seed of the nurse-plant.

If the sowing of the spores of *Æcidium* upon the leaves of wheat resulted in the production of an *Æcidium* identical with it, or if the inoculation of berberry with wheat mildew was succeeded by the development of a *Puccinia* of a very similar character, it would not be so difficult to believe in both cases that the resulting forms might have been caused by inoculation. When the fungi assumed to be produced by inoculation are those to which the nurse-plants are particularly and specially subject, the evidence should be very strong before it is affirmed that a very natural phenomenon had an unnatural * cause.

The evolution of *Ræstelia* on the leaves of the "mountain ash" by inoculation with *Podisoma* spores is quite analogous to the berberry and wheat fungi. It is common enough to find the *Podisoma* on junipers, and the *Ræstelia* on "mountain ash," and the presumption would be, if young plants of "mountain ash" were covered up ever so carefully with bell glasses, notwithstanding that the leaves had been sprinkled with the spores of a dozen other species of fungi, if *Ræstelia* made its appearance, that it bore no relation whatever to any of the foreign spores which had been sown upon it, but would have been there independent of inoculation, or bell glasses, or a dozen like contingencies.

In both cases to which allusion has been made above, there is need of the strongest evidence to show that the ultimate parasite would not have made its appearance but for the inoculation, or that the whole chain was completed which connected the inoculating spore with the parasite produced. It would be folly to contend against facts for the sake of theory, and absurd to combat conclusions fairly deduced from ascertained facts; but in this instance we are bound to contend, in honesty to our convictions, that in neither case has Oersted or De Bary shown to our satisfaction that they were justified in declaring for an alternation of generations of fungi in which the stages were passed on different nurse-plants. When the facts are confirmed and established will be time enough to inquire whether both stages are essential the one to the other, and, if so, how it is that mildewed wheat in such great profusion can be found in districts where berberry bushes are unknown, or why the *Ræstelia* on the leaves of pear trees should be so common in counties where scarcely a savin can be found.

I have been led to these observations partly because some writers have accepted the conclusions at once as if they were incontrovertible facts, and partly because I have personally been charged with ignoring (by silence, it is presumed) the results of De Bary and Oersted's experiments, whereas I only claim the privilege of doubting where I would not dare to deny.

M. C. COOKE

* The term "unnatural" is employed here in the sense that the presumed cause is one of which we have no experience, and which is contrary to the ordinary course of nature.

THE SCIENCE AND ART DEPARTMENT

THE following important Minute on the subject of Science instruction has recently been issued by the Committee of the Privy Council on Education:—

It appears desirable that the instruction of students in Science, after they have completed the course of the ordinary elementary school, should be carried on more methodically than is at present the case, and that they should not attempt to grapple with the more advanced forms of Science until they have received sound and practical instruction in those subjects which constitute the groundwork of all the physical sciences.

To this end the course of instruction specified below has been prepared as adapted both to secondary day schools and to night classes.

It will depend on circumstances, especially if the student can only attend night classes, how many subjects he can take up in one year. It must therefore be understood that the course should not only comprise the subjects named below, but also that they should be taken in the order in which they are stated.

The terminology used is that of the Science and Art Directories. The syllabus of subjects there given states precisely what is included under each head. And it is assumed that before commencing the following course, the student will have been made acquainted, in the elementary school, with the elements of arithmetic, and the primary conceptions of physical science.

COURSE OF INSTRUCTION.—*First Year.*—Mathematics (Subject V., First Stage); Freehand Drawing (2nd Grade Art); Practical Plane Geometry (2nd Grade Art); Elementary Mechanics, including the physical properties of liquids and gases (Subject VI., First Stage); Physics: Acoustics, Light and Heat (Subject VIII., First Stage). *Second Year.*—Chemistry, Inorganic (Subject X, First Stage), with practical work; Physics: Magnetism and Electricity, frictional and voltaic (Subject IX., First Stage); Mathematics (Second Stage and, if possible, Fourth Stage, Subject V.); Practical Geometry, Plane and Solid (Subject I., First Stage); Animal Physiology, if possible (Subject XIV., First Stage). The student should also, during the first and second year, work at mechanical drawing as provided for in the Art Directory, Stage 23*a*. *Third Year*.—The work of this year must depend so much on the student's aptitude, and the progress he has made in the preceding course, that it is impossible to lay down the subjects for the third year's course with any definiteness. It is essential that before continuing his course, or commencing new subjects, he should have a sound knowledge of the first stage of Mathematics, Elementary Mechanics, Physics, and Chemistry; that he should have such a knowledge of practical Geometry and Mechanical Drawing as to be able to draw and read simple plans, elevations, and sections with readiness, and that he should have sufficient facility in Freehand Drawing to make clear and neat explanatory diagrams.

When these subjects have been mastered, the student should, while continuing his studies in mathematics, take up the first stage of Animal Physiology, if he has not already done so. He will then be in a position to specialise his studies with advantage in one of the following groups, according to his requirements, taking up, for instance—1. Physics and Chemistry and Metallurgy; 2. Theoretical and Applied Mechanics, Steam, and Machine Construction and Drawing; 3. Theoretical and Applied Mechanics, and Building Construction and Drawing; 4. Biology; 5. Geology, Physical Geography, Mineralogy, and Mining. The student may also with advantage continue his freehand drawing and practical geometry.

The foregoing course is framed to lay the foundation of a thorough and systematic scientific training. It must, however, be understood that this course, though strongly recommended for all those who can devote sufficient time to go through it, in no way supersedes or does away with the power of holding special classes in different subjects for those who have not these opportunities, or diminishes the aid at present offered to such classes.

The fact of the course being intended as a systematic training will also explain the omission of certain subjects which are not to be considered unimportant because they find no place in the course. Thus systematic Botany will be found of very great use as a preliminary to the study of natural science. As such it may be taught in elementary schools before this course is commenced. But, further than that, it cannot be considered a step in a systematic course till the student takes it up as a portion of Biology in his third year. In the same way Physical Geography is a subject which may with great advantage be studied in all schools, and is especially adapted for students who cannot go through a systematic course. The first elements of Physical Geography, treating broadly the outlines of physical science and describing its objects, should, as stated above, be taught as an introduction to its systematic study. But Physical Geography in its general sense covers so wide a field, embracing to a greater or less degree so many branches of Science, that it does not fall into a systematic course of training in science, though as a means of imparting highly valuable general information, as distinct from a systematic training, it may be strongly recommended.

ARCTIC EXPLORATIONS

An excellent paper on the above subject appears in NATURE of Nov. 30, and it is to be hoped that it may have the desired effect of reanimating in our Government and among scientific men a fresh interest in the prosecution of a further survey of the unknown seas round the Pole.

Agreeing as I do with the writer as to the great importance of such an exploration as he recommends, I cannot so readily acknowledge the correctness of his opinion as to the advantages of the route by Smith Sound over that along the west shore and to the north of Spitzbergen, from which point Parry (the greatest and noblest of arctic explorers) attempted to reach the Pole with boat sledges in 1827.

Parry had, I think, on this occasion chosen the right route, but the wrong season of the year; for he attempted the journey in the month of July, instead of in March, April, May, and June.

At Spitzbergen a vessel can always get as far as 80° north, probably higher; for Mr. Lamont has, during the last two summers, on his pleasure cruises, readily reached the latitude named.

I had it from the great navigator Parry himself, that the ice he saw to the north of Spitzbergen would not have been difficult to travel over at the proper season of the year.

The farthest north point reached with much difficulty by ships in Smith Sound has been 78°40′, and we have not the least warrant or certainty that any future expedition may be able to winter its ship or ships nearer the Pole by this route.

From lat. 78°40′ the distance to the Pole is 680 geographical miles, making the journey there and back 1,360 miles in a straight line.

But surely no experienced Arctic traveller would be sanguine enough to believe that he could take a "bee line" in a sledge journey to the Pole; in fact, he would require to make an allowance of about one-fifth for obstructions by rough ice, probable contour of coast line, &c., so that the actual distance to be made would be $1,360 + 270 = 1,630$ geographical miles, a journey 200 or 300 miles longer than any that has yet been accomplished, even by that admirable Arctic traveller, the late Lieut. Mecham. Yet Mecham, in his two longest journeys of 1,200 or 1,300 miles each (I forget whether these are geographical or statute miles, but I think they are the latter), had advantages not likely to be found in a journey to the Pole. On the one occasion deer, musk-cattle, and other game were so abundant and so tame that he could and did easily kill as many as the party required, and could have killed many more. On the other occasion he was travelling along a known route, at several points of which depots of provisions had been placed by ships wintering there, or by other means, from which he was enabled to obtain supplies both on the outward and homeward march.

Mr. Markham says that a ship can always get so far north in Smith Sound that the Pole can be reached by a journey from it with sledges of 968 miles there and back.

By what powers of reasoning or rule of arithmetic this conclusion has been arrived at I am at a loss to know, unless there is always a certainty of ships getting into winter quarters in Smith Sound as far up as 82° latitude, yet Kane was stopped 200 miles south of this, and Hayes even at a greater distance.

The Spitzbergen route has never had a fair trial with sledges over ice either with or without the aid of dogs, and I believe that if the Pole is ever to be reached, it will be by it, and not by Smith Sound. The distance to be travelled will not probably be less than 1,400 geographical miles, possibly more, a journey practicable enough under favourable circumstances, but by no means easy of accomplishment.

JOHN RAE

NOTES

At the Anniversary Meeting of the Fellows of the Royal Society on Thursday last, Lieut.-General Sir Edward Sabine, R.A., K.C.B., resigned the office of president, which he has filled since 1861, and the Astronomer Royal was elected to fill the presidential chair. The following gentlemen were appointed officers and council for the ensuing year:—President: George Biddell Airy, M.A., D.C.L., LL.D., Astronomer Royal. Treasurer: William Spottiswoode, M.A. Secretaries: William Sharpey, M.D., LL.D. ; Prof. George Gabriel Stokes, M.A., D.C.L., LL.D. Foreign Secretary : Prof. William Miller, M.A., LL.D. Other Members of the Council : George J. Allman, M.D. ; John Ball, M.A. ; George Burrowes, M.D.; George Busk, P.R.C.S.; Prof. Robert B. Clifton, M.A. ; H. Debus, Ph.D. ; Prof. P. M. Duncan, M.B. ; Prof. G. Carey Foster, B.A. ; Francis Galton ; Thos. A. Hirst, Ph.D. ; Sir John Lubbock, Bart. ; Sir James Paget, Bart., D.C.L. ; The Earl of Rosse, D.C.L. ; General Sir E. Sabine, R.A., K.C.B. ; Isaac Todhunter, M.A. ; Sir Charles Wheatstone, D.C.L. The President's annual address was occupied by a *résumé* of the most important advances in science, mainly physical, during the year. After alluding to the loss sustained by the Society in the deaths of Sir John Herschel, Mr. Babbage, and Sir R. Murchison, General Sabine referred particularly to the munificence of Mr. J. P. Gassiot, by which the Kew Observatory has been transferred to the Royal Society in trust, with an income of 500*l*. per annum towards the cost of carrying on and continuing magnetical and meteorological observations with self-recording instruments, and any other physical investigations that may from time to time be found practicable and desirable in the present building at Kew belonging to the Government ; or, in the event of the Government at any time declining to continue to place that building at the disposal of the Royal Society, then in any other suitable building which the Council of the Royal Society may determine. The following papers and investigations were also specially named by the president :—"On the Dependence of the Earth's Magnetism on the Rotation of the Sun," by Prof. Hornstein, of Prague ; the Pendulum Experiments in India, by the late Captain Basevi, R.N. ; Mr. Ellery's report on the Great Melbourne Telescope ; the Investigations of the Lunar Atmospheric Tide, by M. Bergsma, of Batavia ; and the Memoir by Prof. Heer, of Zürich, on the Fossil Plants brought from Greenland by Prof. Nordenskiöld. The Copley and Royal medals were then awarded, as already noted.

WITH regard to the Australian arrangements for observing the Total Eclipse of Tuesday next, we learn that the Royal Society of Victoria (not of New South Wales, as had been previously reported) were up to the end of September making vigorous preparations for an Expedition, but that at that time they were afraid that their plans would be seriously frustrated by the failure of Government aid, which they had been led to expect would be liberally granted. Mr. Ellery, the president, and Mr. Rusden, the secretary of the Royal Society of Victoria, were exerting themselves to the utmost to secure the success of the Expedition, which was to start not later than November 22nd. By the most recent Melbourne papers of October 9 and 10, we learn that, notwithstanding the supineness displayed in the matter by the other Australian colonies, it was still hoped that the Government of Victoria would render such pecuniary assistance as would make it possible for the Expedition to set out with some chance of success in obtaining results of scientific value. The number of persons who had already agreed to join the expedition up to that date was twenty, of whom four or five were of Adelaide, three of Sydney, and one or two of Tasmania. No very certain information had been procured about the prevailing weather in the latitude where the eclipse will be visible. The destination of the steamer will be Cape Sidmouth, about midway between Cardwell and Cape York, where there is some risk of the weather being unfavourable, inasmuch as during December the N.W. winds frequently bring heavy rain. Probably the Expedition will be broken up into several observing parties, and two or more stationed at different points of the mainland, and one on a neighbouring island.

THE elevation of Mr. W. R. Grove, Q.C., to the judicial bench is a noteworthy event in the history of the *personnel* of Science. It is well known that the author of the "Correlation of Forces," and *quondam* President of the British Association, is an authority of no mean rank on some of the most abstruse questions of law.

THE Exhibition of Stone Implements (Neolithic and Savage) at the Apartments of the Society of Antiquaries in Somerset House will be open at the meeting of the Society this evening, and from the 8th to the 14th inclusive from eleven to four. Cards of admission may be obtained from the secretary.

WE learn from Prof. H. A. Newton, of New Haven, Conn., that between 11·20 P.M. on November 13, and 1·45 A.M. November 14, ninety-eight meteors were seen, though the sky was cloudy. Not more than one-tenth of them were, however, regarded as belonging to the meteor stream of November. Prof. Newton thinks that if the earth met the stream this year, it was either before or after the interval of observation.

AN application has been received by the Kew Committee of the Royal Society from Dr. Jelinek, Director of the "Central Anstalt für Meteorologie and Erdmagnetismus," to procure for that establishment a set of self-recording magnetographs similar to those at Kew. The request has been complied with ; and it is hoped that the apparatus will be ready for transmission to Vienna in March next, being the time named by Dr. Jelinek as that at which the new building in course of erection in that city is expected to be completed. The Committee has also been apprised by a letter from Mr. Stone, Astronomer Royal at the Cape of Good Hope, that he had at that date applied to the Admiralty for a set of magnetographs, similar to those at Kew, to be employed at the Cape. The Kew Committee hold themselves in readiness to supply the desired apparatus when they may receive directions to that effect from the Admiralty ; such directions, however, have not yet been received. If Mr. Stone's request is granted, the Cape Observatory will be the third in the British Colonial Dominions employing such instruments, the other two being the Colaba Observatory under Mr. Chambers at Bombay, and the Mauritius Observatory under Mr. Meldrum.

IT is reported that the French Government intends to establish two schools, one at Lyons and the other at Nancy, in place of the Strasburg medical school. The Strasburg professors are to go to Lyons ; and it is expected that that school will assume an important position in consequence of the large amount of hospital accommodation in the city. At Nancy, physics, chemistry and physiology will be more especially taught.

Harper's Weekly announces the death, in Boston, of the Rev. J. A. Swan, on October 31, at the age of forty-eight. Mr. Swan has been long known among his New England friends for his love of natural history and his skill in the use of the microscope ; and during his residence at Kennebunk, although a devoted pastor in that village, he found time to make numerous important explorations and observations in the natural history of the vicinity. Failing in health a few years ago, he visited Europe, and on his return was appointed to the responsible post of secretary of the Boston Society of Natural History, in connection with Prof. A. Hyatt, succeeding Mr. Scudder in charge of the business of the society. Apart from his scientific accomplishments, Mr. Swan was endeared to all his friends by personal qualifications of the rarest merit.

THE Society of the Friends of Science, in Posen, propose, on February 19, 1873, to celebrate the 400th birthday of the eminent astronomer, Nicholas Copernicus, at his birth-place, in the village of Thorn. In addition to the festivities of the occasion, they intend to publish an accurate biography of their countryman, and to prepare a monumental album, as also to strike an appropriate medal. A prize of 500 thalers is offered for the best biography that can be prepared before January 1, 1872, to be based only upon authentic documents.

We have received the first number of "The German Quarterly Magazine; a Series of Popular Essays on Science, History, and Art." The plan of the publication is to give in English such essays, selected from the "Sammlung gemeinverständlicher wissenschaftlicher Vorträge," edited by Profs. Virchow and Franz von Holtzendorff, as are likely to interest the English reading public, and also original contributions; the numbers presenting alternately selections from the departments of Science, History, and Art. The present number contains three papers:—"The Cranial Affinities of Man and the Ape," by R. Virchow; "Sight and the Visual Organs," by A. von Graefe; and "The Circulation of the Waters on the Surface of the Earth," by H. W. Dove; all papers of great interest and importance, but losing something to the English reader from the German phraseology in which the translations are clothed. They are illustrated by good woodcuts, and the subscription to the magazine is 10s. per annum.

Messrs. Longman & Co. are about to publish a volume by Mr. Serjeant Cox, entitled "Spiritualism answered by Science," in which he will detail the arguments that satisfied himself and the other scientific investigators that the phenomena of alleged "Spiritualism" are purely physical, and in no manner associated with spirits of the dead.

Dr. Bessels, the director of the scientific corps of Captain Hall's steamer *Polaris*, in a letter addressed to the president of the American National Academy of Sciences, dated Godaven, August 16, states that he had already made some important observations in regard to the physics of the northern seas, such as a peculiar coloration of the water and an unexpectedly high specific gravity, the maximum density noticed being 1·028. His experiences with his colleagues, Mr. Bryan, the astronomer, and Mr. Meyer, the meteorologist, have been very satisfactory; the former gentleman having made a number of successful azimuth observations, and the latter approving himself an excellent mathematician and an accomplished observer, and an honour to the Signal Service, from which he was detailed for duty with Captain Hall.

The recently published report of Commissioner R. W. Raymond upon statistics of mines and mining in the states and territories west of the Rocky Mountains for the year 1870, forms a stout volume of nearly 600 pages, illustrated by a number of plates and sections, embodying the result of a laborious personal examination, and that of several assistants. The report contains a detailed account of the present condition of the mining industry in California, Nevada, Oregon, Idaho, Montana, Utah, Arizona, New Mexico, Colorado, and Wyoming, together with interesting statements in regard to improved metallurgical processes, such as especially relate to the treatment of auriferous ores, the chlorination and smelting of silver ores, &c. There are also chapters on narrow-guage railways and their adaptation to mining regions, the mining law, the geographical distribution of mining districts, the origin of gold ingots and gold-dust, and the bullion product. The Commisioner congratulates the country upon an increased prosperity in the mining industry, as seen not only in an augmented bullion product, but an improved tone in the business itself, and relief from more or less of the irritating and burdensome questions that have hitherto been connected with the mining interest. Although the excitements which so frequently carry off the miners and settlers of one region into a new locality have been comparatively rare, yet there have been a few of special note. Among these mentioned by Mr. Raymond are those caused by the discovery of gold in Southern California, near San Diego; the discovery of silver in the Burro Mountains, and the rumours of rich placers on Peace River, far into the interior of British Columbia; the bars of Snake River; several localities in Nevada, and others in Utah; the silver mines in the Caribou district of Colorado, &c.

COLDING ON THE LAWS OF CURRENTS IN ORDINARY CONDUITS AND IN THE SEA

III.

Let us now direct our attention to the polar currents, and especially to that one which from Spitzbergen proceeds to the south-west along the coast of Greenland as far as Cape Farewell. It will be seen that this current has received an impulse from the force of rotation, and rises about one foot towards the west coast of Greenland, an effect which however ceases as soon as it has passed the southern point of that country. As soon as the resistance which compelled the current to follow the line of the coast in proceeding to the south-west disappears, it can no longer continue in the same course, but takes a westerly direction towards Labrador, partly in consequence of the rotation of the earth, partly because the level of the current is then higher than that of the waters of Davis Strait. After having advanced a little into the strait, the polar current encounters the currents coming from the north by Baffin's Bay, and joins them in their progress to the south-east along the coast of Labrador, towards which it slopes in virtue of the rotation of the earth. During this passage, and until its arrival in the neighbourhood of Newfoundland, this current is stemmed by the force of rotation, and ought, consequently, to present a slope all along Davis Strait and the east coast of Newfoundland as far as the Gulf Stream. During its course southwards along this course, the polar current is elevated towards the land by the earth's rotation; but as soon as it has passed Cape Race, this resistance suddenly disappears, and the same phenomenon is reproduced as at Cape Farewell. The current bends suddenly to the south-west, and follows the coast as far as Florida, while its breadth and the volume of its water continue to diminish.

From Newfoundland to Florida, a distance of about 500 miles, the Gulf Stream and the polar current flow constantly side by side, under the impulse of the earth's rotation, which raises the polar current towards the land and compels it to follow all the ins and outs of the coast. But what force is it that impels the Gulf Stream, which flows freely in the ocean, to keep by the side of the polar current in all its windings, instead of taking the more easterly direction, which the rotation of the earth tends to give it? It is, of course, gravity, to wit, the force resulting from the slope which the Gulf Stream presents from right to left perpendicular to its direction throughout its entire breadth, a slope which is 1·2 feet from the point where the current debouches into the Atlantic to New York, and about one foot from New York to the place where, after having approached the shores of Europe, it separates into two branches. And if it be asked why the Gulf Stream has this slope; the reason evidently is that the water of the polar current has a specific gravity greater than the water of the Atlantic, and ought consequently to have a lower level than that of the latter sea, since the water beneath is in equilibrium. That this is the real state of the matter is fully confirmed by the researches made in recent years in the Gulf Stream at the instigation of the American Government, and which leave no room to doubt that this current has not kept its place on account of the difference of density which exists between the waters of the polar current and those of the Atlantic. Under these circumstances it is easy to see that the Gulf Stream ought to follow all the sinuosities of the polar current as far as Newfoundland.

But while the Gulf Stream ought thus to be considered as presenting a uniform slope from the Atlantic towards the polar current, the researches undertaken by the American Government prove that the bottom of the Gulf Stream could be in equilibrium only if that current had an inclination directed away from the polar current towards the Atlantic, such that its maximum level would be nearly one-third of the distance from the polar current. Under the actual conditions, then, there is no equilibrium. The waters of the polar current exercise upon the Gulf Stream a pressure which increases with the depth, and causes a continual afflux of cold water, especially in its lower depth. In proportion as these cold waters penetrate into the Gulf Stream, it communicates to them its heat and its motion, and in proportion as it is raised under the influence of the pressure of the polar current driving away the water which it displaces, its breadth ought to go on increasing. But in order that the breadth of the Gulf Stream may increase, it is necessary that its level in the centre of the current be elevated above that which corresponds to the equilibrium of the surface, so that the force of rotation should acquire the preponderance necessary to produce an enlargement of breadth towards the east; and this elevation of the level gives birth at the same time—from the middle of the Gulf Stream to the polar current—to the surface current of warm water which has been ascertained to exist by the American Commission.

It follows then from what precedes, that on the one hand the polar current penetrates at all points into the Gulf Stream, nearly as far as its surface, which sends to the polar current a surface-current of warm water from twenty to fifty fathoms deep; and, on the other hand, that the Gulf Stream ought, throughout the whole of its depth, to exercise upon the waters of the Atlantic a pressure which forces them to give place to those which it receives from the polar current, and which it draws along with it.

The researches which have recently been made as to the Gulf Stream all appear to confirm these conclusions, so that if we suppose that the volume of the Gulf Stream is increased by all the water which the polar current loses in its course, it will follow that if we designate by Q the volume of the Gulf Stream at Bemini, and by q that of the polar current in any section between Newfoundland and Florida, the volume of the Gulf Stream, for the same section, will be equal to $Q + q$. After that, it is necessary that the polar current—which, from the east coast of Newfoundland, flows towards the Gulf Stream, and from Cape Race takes a south-westerly direction along the American coast —gives up in its passage towards Florida all its water to the Gulf Stream. If, then, we assume the speed of the polar current to the south of Newfoundland to be 1·8 feet per second, its breadth 50 miles, and its depth 900 feet, it will be found that its delivery per second is 1,600,000,000 cubic feet, which makes that of the Gulf Stream to the south of Newfoundland 3,200,000,000 cubic feet per second.

From the southern part of the North Atlantic, then, between the equator and 30° of latitude, it discharges at the rate of 1,600,000,000 cubic feet per second; but besides the loss which has been accounted for, there is another which is due to evaporation; the latter deprives the Gulf Stream of a quantity of water greater than that which falls into it in the form of rain, and which flows into it from the neighbouring lands. To calculate this difference, we can make use of the results of the researches which were made in 1860 at St. Helena by Lieut. Haughton. We thus find that the excess of evaporation in the Atlantic, between 0° and 30° of latitude, is equivalent to a mean height of water of 0·22″, which, after deducting one-tenth for the water which comes from rivers, gives a loss of 50,000,000 cubic feet per second. The total quantity of water, then, which passes from the Atlantic between 0° and 30° of N. latitude, can be stated as equal to 1,650,000,000 cubic feet per second.

If we then admit that two-thirds of all the surface of the lands situated to the north of the 30th degree of latitude send directly or indirectly their waters to the Atlantic, and if we estimate the quantity of rain which annually falls upon that surface, the north part of the Atlantic will receive per second an addition of 50,000,000 cubic feet of water, or, about the same quantity which is carried off by evaporation from the south part between 0° and 30° of N. latitude.

But it follows hence that since the southern branch of the Gulf Stream is formed by the water which flows from the south part of the North Atlantic, it ought to have a delivery of 1,650,000,000 cubic feet per second; and, as the delivery of the entire current, after having passed Newfoundland, may be stated at 3,250,000,000 cubic feet, it follows that that of the northern branch is 1,600,000,000 cubic feet, while the united polar currents ought to represent a volume of 1,650,000,000 cubic feet per second. At St. Augustine the depth of the Gulf Stream is about 300 fathoms, which goes on diminishing regularly, as far as Newfoundland, where it is 1,000 feet. From Newfoundland, where it has a breadth of eighty miles and a speed of two feet, the current proceeds E.N.E., with a decreasing speed and an increasing breadth; at the end of 300 miles it has a depth of 200 and odd fathoms, a speed of 0·6 feet, and a breadth of 200 miles. Moreover, during this part of its course it rises about 2 feet above its level at Newfoundland. Until it attains this height, the Gulf Stream forms only a single current maintained by the fall of 1 foot, which it presents from right to left; but as soon as it reaches that, its southern part presents a slope sufficient to give birth to a branch which proceeds to the south-east, towards the African coast, at a speed of 0·6 feet, and with a delivery of 1,650,000,000 cubic feet per second. When the latter current reaches the 30th degree of N. latitude, it meets the north-east trade-wind, which urges it towards the south.

But while the southern half of the Gulf Stream proceeds towards the south, its northern half, whose delivery per second is 1,600,000,000 cubic feet, pursues its course towards the north, along the shores of Great Britain, as far as the 60th degree of latitude in this passage, during which the current rises towards the land and gradually increases in breadth from 100 to 150 miles, while its speed diminishes from 0·6 to 0·3 of a foot per second, it is subjected to the impulse of the earth's rotation, and its western margin, which naturally blends with the surface of the Atlantic, is raised from 1⅓ foot through a course of 140 miles, so that at the 60th degree of latitude this side is 3⅓ feet above the level of the ocean at Newfoundland.

After the Gulf Stream, which throughout this course has a depth of from 200 to 300 fathoms, reaches the north coast of Scotland, about two-thirds of its waters proceed eastwards towards the Norwegian coast, while the other third runs against Iceland, and afterwards continues its course to the north-west to the polar current of Greenland. The latter branch, which the force of rotation raises towards the land, has a depth of 200 and odd fathoms, and a breadth of about 50 miles; in order to be able to advance towards the polar current with a speed of about 0·3 feet per second, a fall of nearly half a foot is necessary. If next we remark that the northern Gulf Stream, towards the north point of Scotland, presents an elevation of 1·5 foot towards the land, we shall easily see that the branch of the Gulf Stream, which proceeds to the north-west, has, along the Icelandic coast, a level which exceeds by half a foot the southern margin of the same current. From this it follows that the waters which skirt the coast of Iceland encounter the polar current on the west of that island at a level higher by 3⅓ feet than the surface of the Atlantic at Newfoundland. But while these waters advance towards the polar current in virtue of the above-mentioned fall, those of the southern margin of the Gulf Stream have precisely the same level as the polar current. The waters of the western side of the north branch of the Gulf Stream, which are forced to bend towards the west after having reached the 60th degree of north latitude, cannot then continue their course towards the polar current; they spread themselves over the surface of the Atlantic and take a southerly course towards Newfoundland, on account of the difference of level. With regard to those parts of the current situated between the north and south boundaries of this branch of the Gulf Stream, they are, according to their position, drawn for a shorter or longer time still towards the polar current, before taking their course towards the south; and it is thus evident that the warm current must spread itself over the whole surface of the Atlantic between the Northern branch of the Gulf Stream and the polar current which descends from Greenland.

If next we turn our attention to the progress of the polar current from the east coast of Greenland, starting from the following data, viz., that the eastern margin of this current, about 65° north latitude, on the west of Iceland, has a level of 3⅓ feet higher than that of the Atlantic at Newfoundland, and that it pursues a course to the south-west at the rate of ¾ of a foot per second—we see clearly that it is obedient to the impulse communicated by the rotation of the earth. Moreover, let us estimate, after Irminger, the breadth of the current at 40 miles, and suppose that the half of the water which the Gulf Stream carries into the icy sea, as well as the half of that which falls in the form of rain or snow, returns towards the south with the current, while the other half descends by Baffin's Bay; we then find that the force of rotation raises the polar current, whose depth may be estimated at 1,000 feet, one foot above its eastern margin, and, regarding the speed as constant as far as the south point of Greenland, we arrive at the result that, along its eastern side, which naturally blends with the Atlantic, its surface must continue to rise as far as Cape Farewell, from 3⅓ to 5 feet above the level of the ocean at Newfoundland. If, after having doubled Cape Farewell, the Gulf Stream descended straight towards Newfoundland, the water in Davis Strait ought to rise to a height sufficient to hinder the current from moving in a more westerly direction. But, as the water in Davis Strait cannot have a higher level than is necessary to impel towards the south the tributary bodies of water as rapidly as they join it, and, as for this purpose, at the 63rd degree of north latitude, an inclination of only 3½ feet above the level of the sea at Newfoundland is required, the polar current, on arriving at Cape Farewell, presents towards Davis Strait a slope of 2½ feet along the Greenland coast, and a foot and a half along its opposite margin, and in consequence of this slope proceeds several degrees into the Strait. But as Baffin's Bay and Davis Strait, as has been said before, are traversed by a polar current descending towards the south-east, it ought to have an inclination in that direction; and it is on this account that the current from the east coast of Greenland, after advancing for some time into Davis Strait, is forced to run westwards towards the coast of Labrador, along which it then flows southwards after joining the current from Baffin's Bay. The two united polar currents, whose delivery may be estimated at 1,200,000,000 cubic feet per second, have a breadth of fifty miles, a speed of $\frac{5}{6}$ of a foot per second, and a depth of about 250 fathoms. They flow to the south-east, under the influence of the earth's rotation, which raises them towards the coasts of Labrador and Newfoundland, and continue their course along the latter towards the Gulf Stream until they have doubled Cape Race, when they bend westward and make for Florida.

If now we return to the warm current which, from the Gulf Stream, curves round the south of Iceland, and then spreads itself gradually over the cold waters of the Atlantic, we see that on its arrival at the south point of Greenland, it rises from left to right, from the Gulf Stream to Cape Farewell, about $2\frac{1}{2}$ feet, which shows clearly that its course is really to the south. Moreover, this elevation from left to right enables us to give a more satisfactory account of the conditions of currents. In short, the western margin of the warm current accompanying the polar current, ought, along the latter, to have a depth of 1,000 feet and a speed of $\frac{3}{4}$ of a foot; and as the speed of the current diminishes regularly in approaching the Gulf Stream, and as all the parts of the current follow, as far as Cape Farewell, a direction nearly parallel, it follows that the speed along the Gulf Stream ought to be at the rate of about $\frac{1}{3}$ a foot per second. But if the returning branch of the Gulf Stream proceeds to the south-west with a fall of $\frac{1}{2}$ a foot on its west border, it follows that the depth of the current ought to be 76 feet. By determining in the same way the depth for a certain number of points of a transverse section, and by calculating according to these data the total delivery of the current, we find that it is raised to 410,000,000 cubic feet per second, which perfectly accords with the result which we ought to obtain. If next we inquire how the various parts of the warm surface current move under the united action of the slope and the earth's rotation, we ascertain that this current ought to follow the course of the polar current which gradually absorbs the waters that penetrate underneath, the water of the current being more dense than that of the polar current, and we find at the same time that in thus flowing towards the polar current the water ought to spread itself all over the Atlantic as far as Newfoundland.

After having thus shown that the preceding theory accounts in a tolerably complete manner for all the movements of the ocean currents, I shall add, in conclusion, that it is very possible, considering our imperfect knowledge of the progress of currents, that many details may be very different from those which have been expounded above; but, so far as the main question is concerned, I believe I am entitled to say with confidence that the laws of ocean currents are pretty much those which I have attempted to establish.

That these laws are equally applicable to the atmospheric currents is evident, and it is scarcely necessary to repeat, that in periods when the differences of temperature on the surface of the globe were greater than at present, all these currents were much stronger, and of a nature otherwise very energetic.

SCIENTIFIC SERIALS

THE *Quarterly Journal of Microscopical Science* for October, 1871. "The origin and distribution of Microzymes (Bacteria) in water, and the circumstances which determine their existence in the tissues and liquids of the living body," by Dr. Burdon Sanderson, F.R.S. This paper is occupied chiefly by details of experiments to determine the conditions which are fatal or favourable to the existence of microzymes in the liquid or gaseous fluids by which we are surrounded, in order to approach one degree nearer to an understanding of their influence on the processes which go on in the living body. After a definition of "microzymes" the author proceeds to their chemical composition and their relation to the media in which they grow. This portion is brief and incomplete. The remainder of the paper is occupied with the experiments, which are grouped under these three sections. (1) Experimental determination of the conditions which govern the development of microzymes in certain organic liquids to be used as tests. Having found in a number of cases that either contact with surfaces which had not been snperheated, or the admixture of water which had not been boiled, was the exclusive cause of the growth of microzymes in the experimental liquid, it was inferred that water is the primary source from whence the germinal particles of bacteria are derived whenever they seem to originate spontaneously in organic solutions. A number of experiments were made with different varieties of water in ordinary use, in order to confirm the observations already made, and to ascertain if all waters possess the properties in question in a like degree. These experiments are detailed under the second section (2) Distribution of the Germal Matter of Microzymes in ordinary Water. The results under this head were not deemed satisfactory. (3) Circumstances which determine the existence of microzymes in organic liquids and tissues, that is, whether the tissues and liquids of the living body participate in the zymotic property which exists in water and moist substances. The conclusion drawn from the facts is, that "it has appeared certain that there is no developmental connection between microzymes and torula cells, and that their apparent association is one of mere juxtaposition. Thus fungi are not developed, notwithstanding the presence of microzymes in the same liquid in which, microzymes being absent, but air having access, they appear with the greatest readiness." Finally, the writer is certain that, although air is the main source of what he calls fungus impregnation, as distinguished from impregnation with microzymes, yet the two acts may take place at the same moment, germs of torula being often contained in the same liquid media as the germ particles of microzymes.—"On the Colouring Matter of some Aphides," by H. C. Sorby, F.R.S.—"Observations and Experiments on the Red Blood Corpuscles, chiefly with regard to the Action of Gases and Vapours," by E. Ray Lankester.—"On Undulina, the type of a new group of Infusoria," by E. Ray Lankester.—"On the Circulation in the wings of *Blatta Orientalis* and other Insects, and on a new method of injecting the vessels of insects," by H. N. Moseley. After describing the method adopted for preparing and fixing the wings of insects for examination of the circulation, the writer proceeds to his experiences with the cockroach. The corpuscles in *Blatta* are so large that the circulation may readily be seen with a high power of a simple dissecting microscope. If an insect be carefully tied, the circulation may be observed in action for as long as twelve hours. Abundance of parasites were found in the blood vessels of *Blatta* and coleopterous insects. The method recommended for the injection of the circulatory system of insects is through the largest artery on the front border of the wing, and the injecting fluid is indigo carmine.—"On the production of Spores in the Radiolaria," by Prof. L. Cienkowski; translated from vol. vii., part 4, of the "Archiv. für Mikroskop. Anatomie." The observations on which this paper is based were mainly made upon Collosphæra and Collozoum. The capsule is the source of the zoospores. In the mature capsule the contents break up into a quantity of little spheroids.—"On the Peripheral Distribution of non-medullated Nerve-fibres," by E. Klein. The writer purposes treating of the nerves of the cornea, those of the nictitating membrane of the frog, of the canal in the tail of the rabbit, and of the mesentery. The present communication is confined to the nerves of the cornea, the remaining subjects are to be embodied in a second paper.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, Nov. 22.—The Rev. Thomas Wiltshire, M.A., in the chair. Mr. Samuel Baillie Coxon was elected a Fellow of the Society. The following communications were read:—1. "Notes on some Fossils from the Devonian Rocks of the Witzenberg Flats, Cape Colony." By Prof. T. Rupert Jones, F.G.S. In this paper the author noticed some Devonian fossils like those of the Bokkeveld, found on Mr. Louw's farm on the Witzenberg Flats, Tulbagh. *Orthoceras vittatum*, Sandberger, was added to the South African list of fossils. The fossils under notice were stated by the author to help to substantiate the late Dr. Rubidge's view, that the old schists termed "Silurian" by Bain are of Devonian age, and continuous across the colony. Their presence in the Witzenberg Flats was also shown to be conclusive against the idea of coal-measures being found there. Mr. Godwin-Austen remarked that the presumed Devonian species of South Africa appeared not to have been completely identified with those of European origin. Although, judging from the range of European marine mollusca, some of which were found of precisely the same species both in Europe and at the Cape, there was nothing surprising in the extension of any old deposit, yet it seemed unreasonable to suppose that the whole district over which the wide-spread Devonian rocks extend could have been submerged at the same time. He traced the original foundation of the Devonian system to the late Mr. Lonsdale, who, in the fossils found in the deposits of Devonshire, thought he traced sufficient grounds for a marked discrimination between those beds and those of Carboniferous age. Mr. Austen had, however, always regarded the Devonian system as merely an older member of the Carboniferous, holding much the same relation to it as the Neocomian to the Cretaceous; and he would be glad to see it recognised, not as an independent system, but merely as the introduction of that far more important system, the Carboniferous, during the deposit of which the globe was subject to the same physiographical conditions. Mr. Etheridge did not agree with Mr. Austen as to the suppression of the name of Devonian system, and commented on its wide-spread distribution, and on the peculiar facies of its fossils, and their importance as a group. He was rather doubtful as to specific determinations arrived at from casts. Though the species of many fossils of Queensland procured by Mr. Daintree did not correspond with those of European areas, yet some of the corals were identical with those of South and North Devon, as were also the lithological characters of the containing beds. Mr. Seeley objected to any attempt to supersede the arrangements of the South African rocks in accordance with the local phenomena, by correlating them too closely with any European series. The recognition of the correspondence in forms seemed to him more to prove a similarity of conditions of life than any absolute synchronism. As to the connection between the Devonian and Carboniferous systems, he agreed with Mr. Austen in regarding the one as merely constituting the natural base of the other. 2. "On the Geology of Fernando Noronha (S. lat. 3° 50′, W. long. 32° 50′)." By Alexander Rattray, M.D. (Edin.), Surgeon R.N. Communicated by Prof. Huxley, F.R.S. The author described the general geological structure of Fernando Noronha and the smaller islands which form a group with it. The surface-rock was described as a coarse conglomerate, composed of rounded basaltic boulders and pebbles, in a hard, dark red, clayey matrix. This overlies a hard, dark, fine-grained basalt, which forms the most striking of the bluffs, cliffs, and outlying rocks. The highest peaks in the group consist of a fine-grained, light grey granite. The author remarked upon the possible relation of the geology of these islands to that of the neighbouring continent of South America, and stated that there is evidence of the islands having been elevated to some extent at a comparatively recent period. 3. "Note on some Ichthyosaurian Remains from Kimmeridge Bay, Dorset." By Mr. J. W. Hulke, F.R.S. The author noticed some teeth found, with a portion of an Ichthyosaurian skull, in the Kimmeridge clay of Dorsetshire. The fragments of the snout were said to indicate that it was about three feet long and proportionally stout. The author indicated the character by which these teeth were distinguishable from those of various known species of *Ichthyosaurus*, and stated that they approached most closely to those of the Cretaceous *I. campylodon*. Mr. Seeley did not consider that, in the main, the teeth of Reptilia afforded any criteria for specific determination. In the Cambridge Greensand, though there were five species of *Ichthyosaurus*, possibly including a second genus, the teeth found were so closely similar that it would have been impossible, from them only, to identify more than one species. Mr. Boyd Dawkins recognised in the specimens exhibited by Mr. Hulke a form of tooth he had found in the Kimmeridge beds of Shotover, near Oxford, but which he had been hitherto unable to attribute to any recognised species. He could not fully agree with Mr. Seeley as to the absence of specific criteria in the teeth of Saurians, as, from his own experience, he was inclined to attribute some importance to their external sculpturing. 4. "Appendix to a 'Note on a New and Undescribed Wealden Vertebra,' read 9th February, 1870, and published in the Quarterly Journal for August in that year." By Mr. J. W. Hulke, F.R.S. The author generically identified this vertebra with *Ornithopsis*, Seeley, *Streptospondylus*, Owen, and *Cetiosaurus*, Owen, taking the last to be typified by the large species in the Oxford Museum. He remarked that if this be the type of *Cetiosaurus*, *C. brevis*, Owen, can hardly belong to it, as the trunk vertebræ are described as being of a totally different structure. Mr. Boyd Dawkins, who had recently visited Oxford, stated that he had there examined the remains referred to. There was, however, no tooth found with them of a character to show the nature of the food on which the animal subsisted. But one of his students had lately found in the same pit that had afforded the remains, a tooth corresponding in its principal characters with those of *Iguanodon*, with which, therefore, the *Cetiosaurus* seemed to be allied, so that it was probably a vegetable feeder. Mr. J. Parker had lately procured from the Kimmeridge clay a number of Saurian remains, and among them were some vertebræ of *Megalosaurus*, to which were articulated others presenting distinctly the characters of *Streptospondylus*. He thought that probably many of the supposed Streptospondylian vertebræ might prove to belong to the cervical region of Dinosaurians. Mr. Seeley disputed the attribution to *Cetiosaurus* of the vertebræ described, and questioned whether the remains at Oxford might not be assigned to *Streptospondylus* or *Ornithopsis*. The depressions in the vertebræ, which might be connected with the extension of the air-cells of the lungs, did not exist in *Cetiosaurus*, but were to be found in *Megalosaurus*. As to the premaxillary tooth mentioned by Mr. Dawkins, he was uncertain whether it should be referred to what he considered as *Cetiosaurus* proper, or to the Oxford reptile. Mr. Hulke replied, pointing out that, since the determination of the Oxford reptile as *Cetiosaurus*, numerous other remains of the same species had been discovered, which had added materially to the basis of classification.—The following specimens were exhibited to the meeting:—Devonian fossils from the Witzenberg; exhibited by Professor T. R. Jones, F.G.S., in illustration of his paper. Specimens of Silver Ores from South America; exhibited by Professor Tennant, F.G.S. Fragment of the Wolf Rock, near the Land's End, and section under polarised light; exhibited by Mr. Frank Clarkson, F.G.S.

Royal Geographical Society, November 27.—Major-Gen. Sir H. C. Rawlinson, K.C.B., president, in the chair.—The President read a letter from Dr. Kirk, of Zanzibar, to the late Sir Roderick Murchison, giving news of a serious outbreak in Unyanyembe, the country lying on the main route to Lake Tanganyika, which is likely to prevent communication with Dr. Livingstone for some time to come. The letter was dated September 25th, and stated that a native chief, having been attacked by a force of Arabs settled in Unyanyembe, had waited his assailants in ambush when returning with their plunder, and had killed many of the principal men. Mr. Stanley, an American gentleman, who was travelling to Lake Tanganyika, and who had charge of letters and stores for Dr. Livingstone, was in the fray, and had been deserted by the Arabs. He had also been ill of fever, and his future plans were uncertain. A report, to which Dr. Kirk attached little credence, had spread in Zanzibar, to the effect that Livingstone and the Arab Mohammed bin Gharib, with whom he had been living, were returning round the south end of Tanganyika, and out of the region of disturbances. Captain R. F. Burton, in commenting upon this letter, informed the meeting that similar affrays between Arab trading parties and the natives had occurred before, and that this unsettled state might continue for two or three years. He thought that Livingstone would find no difficulty in returning by the south of the lake, and that a fearless man like him, speaking the native languages, would be able to pass through the disturbed districts. He had not the slightest misgiving with regard to him.—Captain Burton then read a paper "On the Volcanic Region east of Damascus and the Cave of Umm Nirán." This was a narrative of a hazardous journey of fifteen days, which he had performed in May and June 1871, in company with Mr. C. F. Tyrwhitt Drake, through the Safá Region, the Oriental *Trachon* of the Greek geographers, a wide extent of ancient lava-fields, the hills of which, like little pyramids, dot the eastern horizon, as viewed from Damascus. The danger and difficulty of visiting the many interesting places in this district arose simply from certain petty tribes of Bedouin, descendants of the refractory robbers of the Trachonitis, who dwell in the highlands of the Hauran, under the patronage of the Druses. The worst are the Ghiyás and the Shtáyá, who although they have given hostages, were allowed, during the author's stay at Damascus, to ride the country within three hours of the walls, and to plunder the villages. During one of his excursions a skirmishing party of Ghiyás attacked his party, severely wounding one of his companions. During his journey 120 inscriptions were collected, including three in the Palmyrene dialect. The volcanic outbreak to which the district owes its singular character the author was inclined to attribute to the epoch when the Eastern Desert, a flat stoneless tract, extending from the Trachonitis to the Euphrates, was a mighty inlet of the Indian Ocean, having its northern limit in the range of limestones and sandstones, the furthest outliers of the Anti-Libanus, upon whose southern and eastern feet Palmyra is built, and which runs eastward to the actual valley of the great river. Mr. Drake took a continuous set of compass bearings during the journey, which had enabled him to draw an excellent map of the region. Mr. W. Giffard Palgrave spoke on the subject of the paper, stating that Captain Burton was the only European who had properly explored El Safá. He had himself explored about two-thirds of the distance, without, however, reaching the cavern of Umm Nírán. His own visit terminated at the southern part of the *El Leja*, the great volcanic district celebrated for the destruction of the Egyptian army in the time of Ibrahim Pacha, when they attacked the Druses in the basaltic labyrinth.—A second paper was read, "On the Geography of Southern Arabia," by the Baron Von Maltzan, which contained interesting elucidations of the physical configuration and tribal distribution of the region north of Aden, compiled by systematic interrogation of Arabs at Aden.

EDINBURGH

Naturalists' Field Club.—The annual business meeting of this club was held on Wednesday, the 29th ult., when Mr. Skerving was elected President and Mr. John Brown Honorary Secretary and Treasurer. A vote of thanks was accorded to Mr. Taylor, the retiring secretary. The club now numbers 87 members; and 13 excursions have been made to places of local interest during the summer months.

PARIS

Academy of Sciences, November 27.—M. Chasles presented a theorem concerning the harmonic axes of the geometrical curves, in which there are two series of points corresponding anharmonically on a unicursal curve.—M. P. A. Favre communicated the continuation of his thermic investigations upon electrolysis, in which he gave the results of experiments made especially with the voltameter with plates of copper immersed in sulphate of copper.—M. de Fonvielle presented a note on musical sounds produced at the opening of the valve in balloon ascents.—M. des Cloiseaux communicated some optical and crystallographical observations upon montebrasite and the amblygonite of Montebras, the former a new fluophosphate of alumina, soda, and lithia.—A letter was read from M. Moison describing the use of sea-water for making bread in the environs of Cancale.—M. H. Sainte-Claire Deville presented a note by M. T. Schlœsing on the separation of potash and soda. The author's process is founded upon that proposed by Serullas, in which perchloric acid is employed. He uses, instead of this acid, pure perchlorate of ammonia, treated with weak nitro-muriatic acid. The preparation of the perchlorate is described by the author.—M. Chabrier presented some further observations on the alternate predominance of nitrous and nitric acids in rain-water. The author finds that in calm weather nitrous acid is present in excess in rain-water, whilst nitric acid predominates in stormy weather.—M. Chevreul communicated a letter from M. Sacc on the properties of drying oils, with regard to which M. Thenard also made some observations.—A note by MM. Dusant and C. Bardy on the phenoles was presented by M. Cahours.—M. C. Bernard communicated a note by M. E. Faivre on the movements of the sap through the bark. The author describes a series of experiments made upon mulberry trees, and demonstrates that it is in the bark, and particularly in its liber, that the ascending and descending movements of the sap take place.—M. Joseph-Lafosse presented some observations on the germination of seeds submerged in 1870-71 during the inundation of the neighbourhood of Carenton for the defence of Cherbourg. He stated that after the retirement of the water many plants sprang up in unusual abundance and vigour, and suggested that experiments should be made upon the effects of long soaking upon the germination of the seeds of useful plants.—A letter from M. A. de la Rive on M. Marey's recent communications relating to the electrical discharge of the torpedo was read. The author considered the action of the nerves in causing muscular contraction to be electrical, and that the electrical effect produced by the apparatus of the torpedo was caused by the accumulation in it of the energy of the immense multitude of nervous filaments with which it is supplied.—M. C. Bernard presented a note by M. L. Reverdin on epidermic grafting, describing and discussing the phenomena produced by the transfer of portions of skin from one living animal to another. The author maintains that the adherence of these grafts is produced principally by the epidermis, the dermis having only a secondary action.—M. S. Meunier, in a note on meteoric metamorphism, described the transformation of aumalite into chantonnite by exposure for a quarter of an hour to a red heat, which confirms his conclusion that the latter is the eruptive form of the former.

BOOKS RECEIVED

ENGLISH.—The Young Collector's Handybook of Botany: Rev. H. N Dunster (Reeve and Co.).—Journal of the Iron and Steel Institute. Vol. II., No. 4.—Astronomical Pheaomena in 1872: W. F. Denning (Wyman and Son)

AMERICAN AND COLONIAL.—The Fossil Plants of the Devonian and Upper Silurian Formations of Canada, 21 plates: Principal Dawson.—Elements of Chemistry, Vol. II.: G. Hinrichs.

FOREIGN – Zeitschrift für Ethnologie; Supplement Band: Bastian and Hartmann. (Through Williams and Norgate.)—Die Sonne, von P. A Secchi, autorisirte Ausgabe von Dr H. Schellen, 1te Abtheilung.—Sitzungsberichte der Gesellschaft naturforschender Freunde zu Berlin, 1870.—Die ältesten Spuren Menschen in Europa: A. Müller.

DIARY

THURSDAY, DECEMBER 7.

ROYAL SOCIETY, at 8.30.—On the Fossil Mammals of Australia. Part VI. Genus Phascolomys: Prof Owen, F.R.S—On the Solvent Power of Liquid Cyanogen. On Fluoride of Silver. Part III.: G Gore, F.R.S.
SOCIETY OF ANTIQUARIES, at 8.30.—Exhibition of Stone Implements.
LINNEAN SOCIETY, at 8—Botany of the Grant and Speke Expedition: Lieut.-Col. Grant, C.B., C.S.I.—On a hybrid *Vaccinium* between the Bilberry and Crowberry: R Garner, F.L S.—On the Formation of British Pearls, and their possible improvement: R. Garner, F.L.S.
CHEMICAL SOCIETY, at 8.

FRIDAY, DECEMBER 8.

ASTRONOMICAL SOCIETY, at 8.
QUEKETT MICROSCOPICAL CLUB, at 8.

SUNDAY, DECEMBER 10.

SUNDAY LECTURE SOCIETY, at 4—On the Optical Construction of the Eye: Dr. R. E. Dudgeon.

MONDAY, DECEMBER 11.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.

TUESDAY, DECEMBER 12.

PHOTOGRAPHIC SOCIETY, at 8.

WEDNESDAY, DECEMBER 13.

SOCIETY OF ATS, at 8.—Observations on the Esparto Plant: Robert Johnston
ARCHÆOLOGICAL INSTITUTE, at 8.

THURSDAY, DECEMBER 14.

ROYAL SOCIETY, at 8.30.
SOCIETY OF ANTIQUARIES, at 8.30.
MATHEMATICAL SOCIETY, at 8.—On the Celebrated Theorem that any Arithmetical Progression, two of whose Terms have no Common Factor, contains an Infinitude of Prime Numbers: J. J. Sylvester, F.R.S.

CONTENTS PAGE

ERRATA.—Vol. v., p. 82, col. 2, line 9, for "150°" read "15°."—Vol. v. p. 95, col. 2, line 22 from bottom, for "inverse direction" read "inverse ratio."

THURSDAY, DECEMBER 14, 1871

THE COPLEY MEDALIST OF 1871

Dr. JULIUS ROBERT MAYER was educated for the medical profession. In the summer of 1840, as he himself informs us, he was at Java, and there observed that the venous blood of some of his patients had a singularly bright red colour. The observation riveted his attention; he reasoned upon it, and came to the conclusion that the brightness of the colour was due to the fact that a less amount of oxidation sufficed to keep up the temperature of the body in a hot climate than in a cold one. The darkness of the venous blood he regarded as the visible sign of the energy of the oxidation.

It would be trivial to remark that accidents such as this, appealing to minds prepared for them, have often led to great discoveries. Mayer's attention was thereby drawn to the whole question of animal heat. Lavoisier had ascribed this heat to the oxidation of the food. One great principle, says Mayer, of the physiological theory of combustion, is that under all circumstances the same amount of fuel yields by its perfect combustion the same amount of heat; that this law holds good for vital processes; and that hence the living body, notwithstanding all its enigmas and wonders, is incompetent to generate heat out of nothing.

But beyond the power of generating internal heat, the animal organism can also generate heat outside of itself. A blacksmith, for example, by hammering can heat a nail, and a savage by friction can warm wood to its point of ignition. Now unless we give up the physiological axiom that the living body cannot create heat out of nothing, "we are driven," says Mayer, "to the conclusion that it is the *total* heat generated within and *without* that is to be regarded as the true calorific effect of the matter oxidised in the body."

From this again he inferred that the heat generated externally must stand in a fixed relation to the work expended in its production. For, supposing the organic processes to remain the same; if it were possible, by the mere alteration of the apparatus, to generate different amounts of heat by the same amount of work, it would follow that the oxidation of the same amount of material would sometimes yield a less, sometimes a greater, quantity of heat. "Hence," says Mayer, "that a fixed relation subsists between heat and work, is a postulate of the physiological theory of combustion."

This is the simple and natural account given subsequently by Mayer himself of the course of thought started by his observation in Java. But the conviction once formed that an unalterable relation subsists between work and heat, it was inevitable that Mayer should seek to express it numerically. It was also inevitable that a mind like his, having raised itself to clearness on this important point, should push forward to consider the relationship of natural forces generally. At the beginning of 1842 his work had made considerable progress; but he had become physician to the town of Heilbronn, and the duties of his profession limited the time which he could devote to purely scientific inquiry. He thought it wise, therefore, to secure himself against accident, and in the spring of 1842 wrote to Liebig, asking him to publish in his "Annalen" a brief preliminary notice of the work then accomplished. Liebig did so, and Dr. Mayer's first paper is contained in the May number of the "Annalen" for 1842.

Mayer had reached his conclusions by reflecting on the complex processes of the living body; but his first step in public was to state definitely the physical principles on which his physiological deductions were to rest. He begins, therefore, with the forces of inorganic nature. He finds in the universe two systems of causes which are not mutually convertible;—the different kinds of matter, and the different forms of force. The first quality of both he affirms to be *indestructibility*. A force cannot become nothing, nor can it arise from nothing. Forces are convertible, but not destructible. In the terminology of his time, he then gives clear expression to the ideas of potential and dynamic energy, illustrating his point by a weight resting upon the earth, suspended at a height above the earth, and actually falling to the earth. He next fixes his attention on cases where motion is apparently destroyed without producing other motion; on the shock of inelastic bodies, for example. Under what form does the vanished motion maintain itself? Experiment alone, says Mayer, can help us here. He warms water by stirring it; he refers to the force expended in overcoming friction. Motion in both cases disappears, but heat is generated, and the quantity generated is the equivalent of the motion destroyed. Our locomotives, he observes with extraordinary sagacity, may be compared to distilling apparatus. The heat beneath the boiler passes into the motion of the train, and it is again deposited as heat in the axles and wheels.

A numerical solution of the relation between heat and work was what Mayer aimed at, and towards the end of his first paper he makes the attempt. It was known that a definite amount of air, in rising one degree in temperature, can take up two different amounts of heat. If its volume be kept constant, it takes up one amount; if its pressure be kept constant, it takes up a different amount. These two amounts are called the specific heat under constant volume and under constant pressure. The ratio of the first to the second is as 1 : 1·421. No man, to my knowledge, prior to Dr. Mayer, penetrated the significance of these two numbers. He first saw that the excess 0·421 was not, as then universally supposed, heat actually lodged in the gas, but heat which had been actually consumed by the gas in expanding against pressure. The amount of work here performed was accurately known, the amount of heat consumed was also accurately known, and from these data Mayer determined the mechanical equivalent of heat. Even in this first paper he is able to direct attention to the enormous discrepancy between the theoretic power of the fuel consumed in steam-engines and their useful effect.

Though this first paper contains but the germ of his further labours, I think it may be safely assumed that, as regards the mechanical theory of heat, this obscure Heilbronn physician in the year 1842 was in advance of all the scientific men of the time.

Having, by the publication of this paper, secured himself against what he calls "Eventualitäten," he devoted every hour of his spare time to his studies, and in 1845 published a memoir which far transcends his first one in weight and fulness, and, indeed, marks an epoch in the history of science. The title of Mayer's first paper was, "Remarks on the Forces of Inorganic Nature." The title of his second great essay was, "Organic Motion in its Connection with Nutrition." In it he expands and illustrates the physical principles laid down in his first brief paper. He goes fully through the calculation of the mechanical equivalent of heat. He calculates the performances of steam-engines, and finds that 100 lbs. of coal in a good working engine produce only the same amount of heat as 95 lbs. in an unworking one; the 5 lbs. disappearing having been converted into work. He determines the useful effect of gunpowder, and finds 9 per cent. of the force of the consumed charcoal invested on the moving ball. He records observations on the heat generated in water when agitated by a pulping engine of a paper manufactory, and calculates the equivalent of that heat in horse-power. He compares chemical combination with mechanical combination—the union of atoms with the union of falling bodies with the earth. He calculates the velocity with which a body starting at an infinite distance would strike the earth's surface, and finds that the heat generated by its collision would raise an equal weight of water 17,356° C. in temperature. He then determines the thermal effect which would be produced by the earth itself falling into the sun. So that here, in 1845, we have the germ of that meteoric theory of the sun's heat which Mayer developed with such extraordinary ability three years afterwards. He also points to the almost exclusive efficacy of the sun's heat in producing mechanical motions upon the earth, winding up with the profound remark, that the heat developed by friction on the wheels of our wind and water-mills comes from the sun in the form of vibratory motion; while the heat produced by mills driven by tidal action is generated at the expense of the earth's axial rotation.

Having thus with firm step passed through the powers of inorganic nature, his next object is to bring his principles to bear upon the phenomena of vegetable and animal life. Wood and coal can burn; whence come their heat, and the work producible by that heat? From the immeasurable reservoir of the sun. Nature has proposed to herself the task of storing up the light which streams earthward from the sun, and of casting into a permanent form the most fugitive of all powers. To this end she has overspread the earth with organisms which, while living, take in the solar light, and by its consumption generate forces of another kind. These organisms are plants. The vegetable world indeed constitutes the instrument whereby the wave-motion of the sun is changed into the rigid form of chemical tension, and thus prepared for future use. With this prevision, as shall subsequently be shown, the existence of the human race itself is inseparably connected. It is to be observed that Mayer's utterances are far from being anticipated by vague statements regarding the "stimulus" of light, or regarding coal as "bottled sunlight." He first saw the full meaning of De Saussure's observation of the reducing power of the solar rays, and gave that observation its proper place in the doctrine of conservation. In the leaves of a tree, the carbon and oxygen of carbonic acid, and the hydrogen and oxygen of water, are forced asunder at the expense of the sun, and the amount of power thus sacrificed is accurately restored by the combustion of the tree. The heat and work potential in our coal strata are so much strength withdrawn from the sun of former ages. Mayer lays the axe to the root of many notions regarding the vital force which were prevalent when he wrote. With the plain fact before us that plants cannot perform the work of reduction, or generate chemical tensions, in the absence of the solar rays, it is, he contends, incredible that these tensions should be caused by the mystic play of the vital force. Such an hypothesis would cut off all investigation; it would land us in a chaos of unbridled phantasy. "I count," he says, "therefore, upon assent when I state as an axiomatic truth that during vital processes the *conversion* only and never the *creation* of matter or force occurs."

Having cleared his way through the vegetable world, as he had previously done through inorganic nature, Mayer passes on to the other organic kingdom. The physical forces collected by plants become the property of animals. Animals consume vegetables, and cause them to reunite with the atmospheric oxygen. Animal heat is thus produced, and not only animal heat but animal motion. There is no indistinctness about Mayer here; he grasps his subject in all its details, and reduces to figures the concomitants of muscular action. A bowler who imparts to an 8-lb. ball a velocity of 30 feet consumes in the act $\frac{1}{10}$ of a grain of carbon. A man weighing 150 lbs., who lifts his own body to a height of 8 feet, consumes in the act 1 grain of carbon. In climbing a mountain 10,000 feet high, the consumption of the same man would be 2 oz. 4 drs. 50 grs. of carbon. Boussingault had determined experimentally the addition to be made to the food of horses when actively working, and Liebig had determined the addition to be made in the case of men. Employing the mechanical equivalent of heat, which he had previously calculated, Mayer proves the additional food to be amply sufficient to cover the increased oxidation.

But he does not content himself with showing in a general way that the human body burns according to definite laws, when it peforms mechanical work. He seeks to determine the particular portion of the body consumed, and in doing so executes some noteworthy calculations. The muscles of a labourer 150 lbs. in weight, weigh 64 lbs.; when perfectly desiccated they fall to 15 lbs. Were the oxidation corresponding to that labourer's work exerted on the muscles alone, they would be utterly consumed in 80 days. The heart furnishes a still more striking example. Were the oxidation necessary to sustain the heart's action exerted upon its own tissue, it would be utterly consumed in 8 days. And if we confine our attention to the two ventricles, their action would be sufficient to consume the associated muscular tissue in $3\frac{1}{2}$ days. Here, in his own words, emphasised in his own way, is Mayer's pregnant conclusion from these calculations:—"The muscle is only the apparatus by means of which the conversion of the force is effected; *but it is not the substance consumed in the production of the mechanical effect.*" He calls the blood "the oil of the lamp of life;" it is the slow-burning fluid whose chemical force in the furnace of the capillaries is sacrificed to produce animal motion. This was Mayer's conclusion twenty-six years ago. It was in complete opposition to the scientific conclusions of his time; but eminent investigators have since amply verified it.

Thus, in baldest outline, I have sought to give some notion of the first half of this marvellous essay. The second half is so exclusively physiological that I do not wish to meddle with it. I will only add the illustration employed by Mayer to explain the action of the nerves upon the muscles. As an engineer, by the motion of his finger in opening a valve or loosing a detent, can liberate an amount of mechanical motion almost infinite compared with its exciting cause, so the nerves, acting upon the muscles, can unlock an amount of activity wholly out of proportion to the work done by the nerves themselves.

As regards these questions of weightiest import to the science of physiology, Dr. Mayer in 1845 was assuredly far in advance of all living men.

Mayer grasped the mechanical theory of heat with commanding power, illustrating it and applying it in the most diverse domains. He began, as we have seen, with physical principles; he determined the numerical relation between heat and work; he revealed the source of the energies of the vegetable world, and showed the relationship of the heat of our fires to solar heat. He followed the energies which were potential in the vegetable up to their local exhaustion in the animal. But in 1845 a new thought was forced upon him by his calculations. He then for the first time drew attention to the astounding amount of heat generated by gravity where the force has sufficient distance to act through. He proved, as I have before stated, the heat of collision of a body falling from an infinite distance to the earth, to be sufficient to raise the temperature of a quantity of water equal to the falling body in weight 17,356°C. He also found in 1845 that the gravitating force between the earth and sun was competent to generate an amount of heat equal to that obtainable from the combustion of 6,000 times the weight of the earth of solid coal. With the quickness of genius he saw that we had here a power sufficient to produce the enormous temperature of the sun, and also to account for the primal molten condition of our own planet. Mayer shows the utter inadequacy of chemical forces, as we know them, to produce or maintain the solar temperature. He shows that were the sun a lump of coal, it would be utterly consumed in 5,000 years. He shows the difficulties attending the assumption that the sun is a cooling body; for supposing it to possess the high specific heat of water, its temperature would fall 15,000° in 5,000 years. He finally concludes that the light and heat of the sun are maintained by the constant impact of meteoric matter. I never ventured an opinion as to the accuracy of this theory; that is a question which may still have to be fought out. But I refer to it as an illustration of the force of genius with which Mayer followed the mechanical theory of heat through all its applications. Whether the meteoric theory be a matter of fact or not, with him abides the honour of proving to demonstration that the light and heat of suns and stars *may* be originated and maintained by the collisions of cold planetary matter.

It is the man who from the scantiest data could accomplish all this in six short years, and in the hours snatched from the duties of an arduous profession, that the Royal Society has this year crowned with its highest honour. Dr. Mayer had never previously received any mark of recognition from the society.

It was not in my power to be present at our late president's last address; but Sir Edward Sabine has done me the honour of sending me a printed copy of it. It contains the reasons assigned by him for the award of the Copley medal. Briefly, but appreciatingly, he expresses his opinion of the merits of Dr. Mayer, committing to Prof. Stokes the task of drawing up a fuller statement of the case. This statement is marked by an evident desire to act fairly towards Mayer, and at the same time to qualify the award so that no erroneous inferences may be drawn from it. It will be observed that Prof. Stokes confines himself to Mayer's first paper, the real value of which, however, is best appreciated in connection with Mayer's subsequent work, as the soundness of the root is best demonstrated by the vigour of the tree. Prof. Stokes writes thus:—

"In a paper published in 1842, Mayer showed that he clearly conceived the convertibility of falling force, or of the *vis viva*, which is its equivalent or representative in visible motion, into heat, which again can disappear as heat by reconversion into work or *vis viva*, as the case may be. He pointed out the mechanical equivalent of heat as a fundamental datum, like the space through which a body falls in one second, to be obtained from experiment. He went further. When air is condensed by the application of pressure, heat, as is well known, is produced. Taking the heat so produced as the equivalent of the work done in compressing the air, Mayer obtained a numerical value of the mechanical equivalent of heat, which, when corrected by employing a more precise value of the specific heat of air than that accessible to Mayer, does not much differ from Joule's result. This was undoubtedly a bold idea, and the numerical value obtained by Mayer's method is, as we now know, very nearly correct." Prof. Stokes then qualifies the award in these words:—"Nevertheless it must be observed that an essential condition in a trustworthy determination is wanting in Mayer's method; *the portion of matter operated on does not go through a cycle of changes.* Mayer reasons as if the production of heat were the sole effect of the work done in compressing air. But the volume of the air is changed at the same time, and it is quite impossible to say *a priori* whether this change may not involve what is analogous to the statical compression of a spring, in which a portion or even a large portion of the work done in compression may have been expended. In that case the numerical result given by Mayer's method would have been erroneous, and *might* have been even widely erroneous. Hence the practical correctness of the equivalent obtained by Mayer's method must not lead us to shut our eyes to the merit of our own countryman Joule, in being the first to determine the mechanical equivalent of heat by methods which are unexceptionable, as fulfilling the essential condition that no ultimate change of state is produced in the matter operated upon."

The judgment of Prof. Stokes, regarding the possible error of Mayer's determination of the mechanical equivalent of heat, gives me occasion to cite another proof of the insight of this extraordinary man. His paper of 1845 contains the details of his calculation, which were omitted from his first brief paper. Mayer prefaces the calculation with these memorable words:—

"To prove this important proposition, we must fix our attention on the deportment of elastic fluids towards heat and mechanical effect.

"Gay Lussac has proved by experiment that when an elastic fluid streams from one receiver into a second exhausted one of equal size, the first vessel is cooled, and the second one heated, by exactly the same number of degrees. This experiment, which is distinguished for its simplicity, and which, to other observers, has always yielded the same result, shows that a given weight and volume of an elastic fluid may expand to double, quadruple, in short, to several times its volume without experiencing, on the whole, any change of temperature; or, in other words, that for the expansion of the gas of itself (*an und für sich*), no expenditure of heat is necessary. But it is equally proved that a gas which expands *under pressure* suffers a *diminution* of temperature.

"Let a cubic inch of air at 1°, and under the pressure of 30 inches of mercury, be warmed by the quantity of heat x to 274° C., its volume being kept constant; this air, on being permitted to stream into a second exhausted vessel of the same size, will retain the temperature of 274°, and a medium surrounding the vessel will suffer no change of temperature. In another experiment, let our cubic inch of air be kept, not at constant *volume*, but under the constant *pressure* of the 30-inch mercurial column, and heated to 274°. In this case a greater quantity of heat is required; let it be $x + y$.

"In comparing these two processes, we see that in both of them the air is heated from 0° to 274°, and at the same time permitted to expand from one volume to two volumes. In the first case the quantity of heat necessary was $= x$, in the second case $= x + y$. In the first case the mechanical effect was $= 0$, in the second case it was equal to 15 lbs. raised one inch in height."

He then proceeds with his calculation.

Here it will be seen that Mayer was quite awake to the importance of the considerations dwelt upon by Prof. Stokes —that he knowingly chose for his determination a substance which, *an und für sich*, in expanding, consumes no heat. Hence, when by its expansion *against pressure* heat is consumed, no part of that heat is lost in producing "a change of state in the matter operated upon." The heat consumed is, therefore, the pure equivalent of the work done.

With regard to Dr. Joule, I have, to my regret, vainly endeavoured to find a mislaid document written a year ago, in which I ventured to describe his labours,* and to express the esteem I entertain for them. Supposing him to have derived his inspiration from Mayer's papers, that they had even caused him to prosecute his experiments on the mechanical equivalent of heat, he would still have rendered immortal service to science, and more than merited the honours bestowed upon him last year. For, wanting his work, the mechanical theory, however strong the presumptions, and however concurrent the evidence in its favour, could not be regarded as completely demonstrated. But Joule was not stimulated by Mayer. His work is his own, being practically contemporaneous with that of Mayer. He not only demonstrated experimentally the mechanical theory of heat, but in its completer form he was an independent creator of that theory. And so impressed was the Council of the Royal Society last year with the magnitude of his merits, that they actually added to the Rumford Medal already bestowed upon him, the final distinction of the Copley Medal. If England rated him as highly as I do, his reward would not be confined to mere scientific recognition.

As regards the latter, however, I do not think that the possibility suggested by Prof. Stokes represents any real danger. I do not imagine that the eyes of Science are in the least degree likely to be "shut to the merits of our own countryman." And I believe that the Royal Society, by stamping in two consecutive years these two men with the highest mark of its approval, will have strengthened that confidence in its impartiality which, throughout the whole scientific world, it has so long and so justly enjoyed.

JOHN TYNDALL

* Thanks to the friendly efforts of Dr. Sharpey, this document reached my hands just as the proof of this paper was being returned for press. With the permission of the Editor of NATURE I will publish the document, with some additional matter, next week. J. T.

AIRY ON MAGNETISM

A Treatise on Magnetism. By G. B. Airy, Astronomer Royal. (Macmillan and Co.)

THIS is a book written upon the true scientific principle expressed by Newton when he said "Hypotheses non fingo." The elementary laws of magnetism are deduced by rigorous induction from particular cases and are then applied to explain phenomena. The book contains the substance of a series of lectures delivered by the Astronomer Royal at the University of Cambridge. One great element of excellence in the book is that the mathematics employed throughout are of a simple character, so that the first principles of magnetism are thus thrown open to one who has gone no great way in mathematical reading.

Formulæ having been obtained in the early sections for the action of one magnet on another, and the law of the inverse square having been established by a comparison of calculation with experiment, the great bulk of the volume is occupied in investigations which bear more directly on terrestrial magnetism and the magnetism of iron ships. The methods of determining the values of the magnetic elements at any place are carefully explained and illustrated, and the necessary formulæ deduced from the theory established in the preceding sections. We would especially recommend to the reader's attention the articles on the theory of the dipping needle. One chapter of extreme interest is devoted to "Theories of Terrestrial Magnetism," and the beautiful theory of Gauss is sketched out. We sincerely hope that that theory which was carried by Gauss to the fourth order of approximation will be before long carried to a higher order. Data now exist for this advance, as it requires accurate determinations of only eleven more elements.

The subject of the deviation of the compass in iron ships is one upon which the Astronomer Royal is peculiarly justified in speaking or writing. All the sections relating to the disturbance of compass needles are full of most important and suggestive matter. One section is devoted to the continuous registration of small changes in terrestrial magnetism, and the concluding section just touches on the subject of the relation between galvanic currents and magnetic forces, without entering into any calculations.

The book supplies a distinct want which has hitherto existed in the list of our mathematical text-books, and is a most valuable contribution to the diffusion of physico-mathematical science.

JAMES STUART

OUR BOOK SHELF

Rudimentary Treatise on Geology.—Part II. Historical Geology. By Ralph Tate, A.L.S., F.G.S., &c. With Illustrations and an Index. (London: Lockwood and Co.)

THIS little book is partly based on Portlock's "Rudiments of Geology," and "is set forth in the full belief that it will be found to be an epitome of the history of the British Stratified Rocks." The first three chapters are introductory, and contain the usual table of the British Sedimentary Strata, with some brief remarks thereon, which are followed by what the author calls a "Palæontological Summary." In this summary he takes a rapid view of the animal and vegetable kingdoms, and points out briefly under which classes and orders fossil organic remains may be ranged. The rest of the volume is entirely occupied with descriptions of the Formations and their subdivisions, and with lists of characteristic fossils. We have no doubt that the preparation of this book has cost its compiler considerable labour; and he certainly has managed to cram a good deal into the short space at his command. The information, indeed, is just too tightly packed; it forms very dry reading, and will be apt to frighten a beginner. If it was necessary that the volume should be no larger than it is, we think some of the palæontological details might have been omitted, and here and there the description of minor subdivisions of formations conveniently cut even shorter than they are, so as to obtain room for certain particulars about the history of the strata, which are either too meagrely noticed or are altogether ignored. The references to former volcanic action in Britain are quite inadequate. We find no mention of the fact that volcanoes were active in the South-West of England during the deposition of the Devonian Strata; nor is there any notice taken of the occurrence of volcanic rocks in the Old Red Sandstone of Ireland. A slight allusion is made to the igneous rocks of the Scottish Middle Old Red Sandstone, but the far more extensive volcanic products belonging to the Lower Old Red series are passed over altogether. The igneous rocks of the Pentland Hills are not, as the author states, of "Upper," but of Lower Old Red Sandstone age. Again the reader, looking over what is said about the igneous rocks of Carboniferous age, would never learn that volcanoes played so active a part in Scotland during the accumulation of the Lower Carboniferous and Carboniferous Limestone periods; nor that in Ireland also volcanoes here and there piled up ejectamenta upon the bed of the Carboniferous Limestone sea. Surely in a book purporting to be an epitome of the history of British stratified rocks, the volcanic phenomena that characterise so many successive epochs of the past ought to have had a somewhat fuller notice. There are various other points in connection with physical geology which are quite ignored. For instance we find no mention of Prof. Ramsay's theory of the Glacial origin of certain breccias and conglomerates of Silurian, Old Red Sandstone, and Permian age—a theory which, whether Mr. Tate agrees with the Professor or not, ought certainly to have had some reference made to it no matter how brief. We had marked a number of passages where the author's meaning is not very clear and will be apt to puzzle a learner. One of these will suffice. Speaking of the Glacial epoch, the author says:—"Our inquiry has now come to that point where, though we still see in the recent results of geological phenomena evidence of the formative processes of nature, yet we are kept at a distance from the present epoch; for although the shells are all of living species, they are generally arranged in positions and associated with detrital matters of such a description that their appearance indicates the action of forces prior to the present order of things." Occasionally we come across statements which are very far from being consistent "with the opinions generally held by geologists." We read, for instance, that "the first trace of a land plant is at the very top of the Upper Silurian, and we may conclude that there were no terrestrial plants during the long Silurian epoch, a vast interval far exceeding in duration that of any other system."

Besides figures of characteristic fossils, the volume is illustrated with a number of diagrammatic sections. A copious index is appended. J. G.

Illustrated Catalogue of the Museum of Comparative Zoology at Harvard College. No. IV. Deep-sea Corals. By L. F. de Pourtales, Assistant U.S. Coast Survey. 1871.

COUNT POURTALES had the good fortune to be one of that band of naturalists who, dredging for the first time in deep water between Key West and Havana, came to the conclusion that "animal life exists at great depths in as great an abundance as in shallow water." This opinion was published in his "Contributions to the Fauna of the Gulf Stream at great Depths" (Cambridge, U.S., 1867). Moreover as a zoophytologist he had the credit of obtaining the first true stony corals from great depths. Numerous corals were dredged up under his superintendence in 1868 and 1869 from off the sea floor of the so-called Straits of Florida in the course of the Gulf Stream, and they were carefully described by him in Nos. 6 and 7 of the last-mentioned work. Now the results of the Deep-sea Dredging so far as the Corals are concerned, appear in the handsome essay in quarto before us; the specific descriptions have been revised, new forms are described, and the illustrations in lithography testify to the excellence of American printing from stone. The interesting coral fauna in the deep sea of Florida has already to a certain extent been compared with that of the cold and warm area of the North Atlantic, in the Proceedings of the Royal Society, March 24, 1870; and the new species described by M. de Pourtales, together with the remarks upon the classification of the corals, will probably enhance the importance of the labours of those English naturalists who have undertaken the description of the results of our abyssal dredgings. The great horizontal range of some of the deep-sea corals is as remarkable as the vertical range of others; and M. de Pourtales, although strongly impressed with the importance of some structural characters in the distinction of specific differences which are not thought so valuable and important in England, leans to the belief in these ranges. The American deep-sea coral fauna is not so rich in species, and apparently in individuals, as that of the North Atlantic and Lusitanian Coasts, but there is one form which is found in the globigerina mud off BahiaHonda, Florida, in 324 fathoms, which will always be of interest to the naturalist who studies palæontology. *Haplophyllia paradoxa* Pourtales, possesses all the essential characters of the Rugosa, and is allied to the simple coral, *Calophyllum profundum* Germar—the Permian *Polycœlia profunda* of King, but it has been shown to be also allied to *Guynia annulata* Duncan, a small rugose coral dredged off the Adventure Bank in the Mediterranean. Both Haplophyllia and Guynia have a strong central axis or columella, the existence of which is of generic importance, and it is therefore necessary to ally these two modern representatives of the old Rugosa which dominated in the coral fauna of the Palæozoic age with the Cyathaxonidæ of the Carboniferous rocks. M. de Pourtales is so gentle a critic that if one wished to differ from him in print, the desire would fail. When the Zoological Society print, which they are about to do, the Essay on the deep-sea corals dredged from H.M.S. *Porcupine*, nothing will be more satisfactory than that an interchange of notes and specimens should take place, so that in a supplement the American and English authors may terminate their unimportant little differences in classification. The beauty and correctness of the illustrations are extreme, and they do the artist, and especially the printer, great credit. It is to be hoped that some English lithographic printer will see the American triumph in this particular, and will forthwith mend his ways.

P. M. 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.*]

Alternation of Generations in Fungi

In Mr. Cooke's article on this subject, it is stated that I have shown that there are at least four consecutive forms of reproductive cells in the bunt (*Tilletia caries*). I imagine that by a slip of the pen he must have substituted this for hop mildew; but, be this as it may, what I really did say at a time (1847) when the formation of secondary fruit was not ascertained in Ustilago, Puccinia, and allied parasites, was as follows, after describing the curious anastomosing threads which are produced on the germinating processes of the bunt spores:—"I was at first inclined to think that it had something to do with the reproduction of the bunt, and it is quite possible that in plants, as well as in the lower animals, there may be an alternation of generations. This is, however, merely thrown out as a hint which may be followed out by those who have fewer avocations than myself. Many anomalous appearances, amongst Algæ especially, seem to indicate something of the kind." * This growth can only be regarded as an intermediate state, which is probably necessary for the propagation of the parasite, and the same must be said of other cases in which the anomalous form does not produce organisms similar to itself. In such cases as the hop and vine mildew, the Oidium forms may be propagated almost indefinitely with only an occasional production of another form, and this, perhaps, may safely be regarded as an alternation of generations, while mere conidia-bearing forms can scarcely be so regarded. In such cases as that of the Uredos, which accompany or precede Puccinia, though both are fertile, we can scarcely recognise such an alternation; but if it is once established that a Puccinia produces an Æcidium, or an Æcidium a Puccinia, we should have a clear case. The usual argument about wheat being subject to mildew where there are no berberry plants, or Rœstelia where there are no savines, does not seem to me to be good. It appears quite clear that wheat mildew may be produced, either from the germination of *U. rubigo vera*, or from its own secondary spores, and that almost indefinitely, where there is no berberry; but this does not show that the spores of Puccinia, when sown on the berberry leaf, may not produce the Æcidium, or the spores of the Æcidium the mildew. I quite agree with Mr. Cooke, that the observations of Oersted and De Bary are not absolutely conclusive, though I may be inclined to give them more weight than he does. The observations should certainly be repeated; but, if the results should be the same, I should certainly feel inclined to accede to their views, indisposed as I always am either to jump hastily to conclusions myself, or to accede at once to the crude observations of others.

M. J. Berkeley

Whether Mr. Cooke has sufficiently appreciated the labours of De Bary and Oersted, in his article published in your columns of last week under the above title, I leave for others to determine. I wish now merely to call attention to one sentence in his article, as follows:—"It is manifest that no amount of care in cultivation, under bell glasses or other exclusion from foreign influences, is sufficient against a contingency *which dates back to the seed of the nurse-plant.*" Does Mr. Cooke mean that the spores of the fungi themselves deposited in the seed of the nurse-plant are carried up, so to speak, in the process of growth, into the leaves, where they germinate; or that the *liability* to produce parasitic fungi is communicated from the seed to the mature plant by some process which combines the Pangenesis of Darwin with the spontaneous generation of Bastian? I see no other explanation of the sentence than one or other of these alternatives.

Mycelium

Leibnitz and the Calculus

Prof. Tait need not wonder if an attack that is "totally unexpected" should seem "appallingly sudden." In the absence of a statute of limitations restricting to two years and a half the right to take up a gage, there can be no reason why an attack should not be made, save its personal bearings; and the circumstances of the challenge might be cited in bar of any exception taken on that ground. I thank the Professor for his explanations. I could not have guessed that under cover of his challenge to produce a metaphysician who was also a mathematician, lurked the assumptions, that every mathematician was a metaphysician, and that every metaphysician was either a mathematician or (in the old sense) a physician. Well, he has a perfect right, for his own private convenience or pleasure, to identify two names which he had from the first asserted to be eternally distinct. Accepting his classification, then, for the sake of argument—certainly not for fruitless controversy—to wit, that everyone is either a mathematician or a non-mathematician, and that every *true* metaphysician must be either mathematician or physician (Faraday did not hate the term "physicist" worse than I do) we are confronted with some surprising results. Leibnitz, the author of the *Monadologie* and the *Théodicée*, works that are known to contain the germs of the *Kritik der reinen Vernunft*, was a *spurious* metaphysician. Why, in the name of common sense? "Because," says Prof. Tait, "he was a non-mathematician; there is no medium, you know; he must have been either a non-mathematician or a mathematician, and a mathematician he was *not*." What! Leibnitz not a mathematician? "Not a bit of it," says Prof. Tait; "for he was, I fear, simply a thief as regards mathematics, and in physics he did not allow the truth of Newton's discoveries." I do not object to the Professor calling a spade a spade; but I assure him that this charge is made just twenty years too late. It is exactly that time since the last vestige of presumption against the fair fame of the great German was obliterated. If Prof. Tait does not understand me, or, understanding me, disputes the unqualified truth of my statement, I promise to be more explicit in a future letter. But I incline to think the question is not susceptible of *proof* until the Council of the Royal Society, who so grossly disgraced themselves in 1712, shall do the simple act of justice and reparation required of them, viz., publish the letters and papers relating to this controversy, which since that date have slumbered in the secret archives. I advise Prof. Tait to utilise the meantime by reconsidering some of his utterances on the *Principia*, lib. ii. lem. 2.

It appears, too, that Descartes, notwithstanding his physics, which are very sad, *was* a mathematician, and therefore a *true* metaphysician, and this, I suppose, despite the *spurious* metaphysics of his *Discours* and his *Méditations*. By the way, when Prof. Tait parenthetically and admiratively corrects me for calling him *Cartes*, he surely overlooked the fact that *Cartes* is his English name, the name by which he was known to the readers of Dr. Samuel Clarke, &c., and is therefore preferable to the dog-latin alternative.

Such, then are some of the surprising results of adopting Pro Tait's classification of mathematicians and metaphysicians. But he objects to my classification of the former, that the greatest mathematicians of our own day—among which Prof. Tait will allow me to count himself—would fall into my second class, since they are not inventors of a calculus, and yet they are not mere experts. Among the names he adduces are Cayley and Sylvester, the co-inventors of a new calculus, viz., that which has been so fertile in its application to Linear Transformations; I mean, of course, the Higher Algebra. Accordingly, both would, of course, fall into my first class; and I will add, that I should assuredly think that "something is rotten in the state of Denmark" if I found the true mathematical ποιητής had ever contented himself with the improvement and application of other men's productions.

C. M. Ingleby

Highgate, Dec. 4

The Science and Art Department

I have been expecting, but in vain, to see Mr. Uhlgren's reply to the request made to him a few weeks since, to produce the Department's letter of which he spoke, and in which it was stated that the rumoured reduction of the number of certificates awarded had actually taken place through the examination papers having been returned for revision. I quite agree with your correspondent who challenged its production, that such a document ought to be made widely known if it exists; whereas if Mr. Uhlgren's statement is founded on any misapprehension, it ought to be corrected without delay.

If such a statement were unfounded, such complaints as those Mr. U. made are, I think, more likely to damage the cause of

* "Journal of Horticultural Society of London," vol. ii. p. 112.

science teachers, who have already enough grievances to urge against the Department on the score of its administration, than to obtain any amelioration of their status.

I do not think many science teachers will endorse more than one other of Mr. Uhlgren's complaints; so that it is of the greatest importance that that one which affects them all should be proved in the fullest and most circumstantial manner.

Plymouth, Dec. 9 A LOCAL COMMITTEE-MAN

Lunar Calendars

I WISH to call attention to the variations observable between the true period of new moon and the commencement of lunar months, as set forth in the following table:—

Period of New Moons A.D. 1872		H.M.	Jewish Calendar A.M. 5632-3			Mahomedan Calendar A.H. 1288-9		
Jan.	10	2 58 P.M.	Shebat	commences	11	Jan.	12	Dulkaadah
Feb.	9	1.52 A.M.	1st Adar	,,	10	Feb.	11	Dulhagee
Mar.	9	0.53 P.M.	2nd ,,	,,	11	Mar.	11	Mulharram
April	8	0.32 A.M.	Nisan	,,	9	April	10	Saphar
May	7	1.19 P.M.	Iyar	,,	9	May	9	Rabia (i.)
June	6	3 23 A.M.	Sivan	,,	7	June	8	,, (ii.)
July	5	6.25 P.M.	Tammuz	,,	7	July	7	Gomada (i.)
Aug.	4	9.46 A.M.	Ab	,,	5	Aug.	6	,, (ii.)
Sep.	3	0.54 A.M.	Ellul	,,	4	Sept.	4	Rajab
Oct.	2	3.31 P.M.	Tishri	,,	3	Oct.	4	Shabân
Nov.	1	5.28 A.M.	Heshvan	,,	2	Nov.	2	Ramadân
,,	30	6.25 P.M.	Kislev	,,	1	Dec.	2	Sbawal
Dec.	30	6.36 A.M.	Tebeth	,,	31	,,	31	Dulkaadah

As many eminent and practical astronomers write to NATURE, I shall be much obliged if some one will add a fourth column to the above, fully explaining these differences. My object is to ascertain if a calendar, founded on lunations, is at all susceptible of *universal* use, so as to be correct to time in all places. The true new moon is *invisible*, the visible new moon is not the true new moon; is there a medial average?

November 23 MYOPS

New Zealand Forest Trees

LET me recommend those of your readers who take an interest in this subject, to trust for *correct* information thereanent to the works whose names are appended, and *not* to the statements of recent correpondents of NATURE, who commit errors so great as to refer *Manuka* to the genus or family *Diosma!*

(1) Dr. Hooker's "Handbook of the New Zealand Flora," which contains at the end of vol. ii. an "Alphabetical List of Native and Vernacular Names" of New Zealand plants, including trees.

(2) A similar Catalogue of Native and Vernacular Names, published, subsequently to Dr. Hooker's list, by Dr. Hector, Director of the Geological Survey of New Zealand.

(3) "Report and Award of the Jurors" of the New Zealand Exhibition of 1865; which contains at page 474 an admirable table—showing the strength and other qualities of New Zealand woods, in connection with the names of the trees yielding the said timbers—carefully drawn up by the late Provincial Marine Engineer of Otago, J. M. Balfour, C.E.; and

(4) The 3 vols. already published of the "Transactions and Proceedings of the New Zealand Institute."

W. LAUDER LINDSAY

Solar Halo

SEEING in your last number an account of a Solar Halo, it has occurred to me that the following description of a similar phenomenon, which I saw in Norway this autumn, may not be uninteresting to some of your readers.

The sun, at 4 o'clock P.M., was just setting behind a range of mountains in the Romsdalen, when a bright halo of light appeared round it, forming a clearly-defined circle, and at the crown of the circle there appeared two horns, as of the beginning of another circle inverted, the junction of the two circles being very luminous; the limbs of the inverted circle—if I may so call it—were rather straight than curved, and were not very long. A second and outer circle, just twice the diameter of the inner one, shortly appeared, and this circle had all the colours of a rainbow most distinctly visible. These two bows were strongly defined for an hour at least, and during that time constant waves of light shot up and across the sky, not always from the centre, where the sun was, but often from some point within the inner circle to the south of its centre. At other times rays of light would shoot out at a tangent from the outer bow, sometimes on one side and sometimes on the other. Again, some would shoot from one circle to the other, forming a series of bars parallel with the horizon, and at last the rays seemed to concentrate, and, radiating from the centre of the inner circle, shot right through both circles across the sky over our heads, forming a series of gigantic ribs, which extended from west to east.

The day (it was September 23) had been perfect, with a bright sun, a cold, frosty atmosphere, and a blue, cloudless sky. Snow had fallen heavily about three days before, and was still lying everywhere; but on the day we saw this grand display not a cloud had been visible from morning till evening. After all was over, the clouds crept up, and we saw several brilliant shoots of the Northern Lights. W. W. HARRIS

Manningham, Bradford, Dec. 6

Proof of Napier's Rules

SUCH a structure in cardboard as that described by Prof. A. S. Herschel in NATURE, No. 106, may be found very useful in facilitating the study of the proof of "Napier's Rules," but the ingenious learner might object that the demonstration was confined to one particular species of triangle—the isosceles right-angled with a perimeter equal to a quadrant; for Mr. Herschel's angles a and b are plainly equal, and together with c make up a right angle. The corresponding construction for any case would be as follow:—Take a circular piece of cardboard with centre D (referring to Mr. Herschel's diagram), and on the circumference, in the same direction, take any two arcs B1, 12. Let a perpendicular from B or D1 meet it in D, and a second from C or D2 meet it in A, and be produced to reach the circumference in B'. Finally, a semicircle on A B' as diameter and another with centre A and radius A C will determine by their intersection the point C'. To a construction thus generalised all that Prof. Herschel adds would apply.

As a question of "Queen's English," it seems hard to connect the last clause in the first paragraph of Prof. Herschel's letter with what precedes. "Them" can only refer grammatically to "difficulties;" but surely Mr. Cooley did not propose to himself 'to render *them* as easily accessible as possible to the inquiring student in mathematics." J. J. W.

The Cause of Specific Variation

I HAVE only just read Mr. Mivart's "Genesis of Species," and was glad to find that his ideas, so ably expressed, are nearly, if not quite, identical with my own, which I laid before the Victoria Institute in a paper "On Certain Analogies between the Method of Deity in Nature and Revelation," May 10, 1869. On p. 259 of his "Genesis of Species" he has the following remarks:—"But are there any grounds for thinking that, in the Genesis of Species, an *internal* force or tendency intervenes, co-operating with and controlling the action of external conditions?" This question appears to me to exactly correspond with the sentiments of the following passage from the "Journal of the Transactions of the Victoria Institute," vol. iv., p. 265.:—

"Rather than venture on any attempt to explain the Divine methods by ordinary terms, I would prefer adopting some general expressions to convey an imagined idea of the causes of existing things, and as less liable to the charge of anthropomorphism.

"I purpose, therefore, adopting the general word *force*, and recognising all issues in nature as the effect produced upon matter by the resultant of component forces. These forces are separable into physical, chemical, biological, &c.; and, in addition to all those which the chemist and the physicist can eliminate and claim as the objects of their special studies, there still remains a residuum of forces in those organisms endowed with life, and which produce those results which we say are designed, and which it is customary to regard as witnessing to a Divine Intelligence.

"In recognising these latter forces, I would call them evolutive, but as being so far like others that their resultant with them produces relative effects only according as in their continual attempt at equilibration they are more or less counteracted or assisted by other natural forces.

"As an illustration I would recognise every special issue of evolution, as, for example, some well-marked variety of animal (say pigeon) or plant (say rose) as the effect of the combination of the usually so-called natural forces in conjunction with the evolutive, as a temporary stable form, so long as environing conditions to which it was subjected remain the same. Hence appears the permanency of some species and races. Subject them, however, to altered conditions, and thus bring an unaccustomed set of forces to bear upon them, *e.g.*, by domestication or cultivation; the forms once so stable soon 'break,' the equilibrium is overthrown, and variations once more ensue. . . . . . .

"After all, therefore, what I have here called evolutive forces in the organic world may prove to be only particular phases of those which conspire to constitute animal and vegetable life. And just as in the vital force itself it is usual to recognise two such phases, viz., the vegetative and reproductive, so the power of development or continual advance or alteration from an assumed type may ultimately appear as particular forms of life-force issuing in those results which we are accustomed to look upon as designed." GEORGE HENSLOW

ON DEEP-SEA THERMOMETERS*

THE objects of this paper and of the experiments and observations recorded therein, are :—

1. The ascertainment of the effect of pressure on thermometers used for deep-sea purposes.

2. To obtain a scale whereby observations made by the thermometers now in use could be corrected for pressure.

3. To obtain a scale whereby observations made previously by other thermometers can be utilised.

In the early part of the year 1868 the attention of the Hydrographer of the Navy was directed to the unsatisfactory nature of the deep-sea Six's thermometers then in use.

The objections made to these thermometers were :—

1. Their fragility, the slightest jar or blow often breaking them.

2. The necessity of their being always kept in a vertical position.

3. The uncertainty of the register, the indices being generally capable of being shaken down.

4. Their large size, in connection with friction in passing through the water.

5. The substance they were mounted on, being generally wood, became so swollen by pressure of the water as often to render them incapable of being withdrawn from the case.

It was also considered that in all thermometric observations at great depths we had been "working in the dark," in that we had no idea of the effect pressure had on the instrument, and consequently on the recorded results; and it was reasonable to suppose that as the action of a thermometer was affected *in vacuo*, an opposite effect would be had by placing them under pressure, the more especially as in the one case the pressure of only one atmosphere, or 15 lb. to the square inch, was removed, while in the other the atmospheres would have to be reckoned by hundreds and the pressure by tons. On this point we were not without actual observation; for Mr. Glaisher, during the year 1844, in some experiments made on the temperature of the Thames near Greenwich with delicately constructed instruments, found that the indications of temperature were affected by pressure on the bulb of the thermometers, and that at a depth of only 25 feet, or about three-fourths of an atmosphere, the readings were increased by 2°; but no definite conclusion could be arrived at from these observations in respect to our deep-sea thermometers, beyond the fact that they were liable to be so affected.

* Abridged from a paper read before the Meteorological Society, April 19, 1871, by Capt. J. E. Davis, R.N.

It was therefore suggested to the Hydrographer—

1. That the author might be placed in personal communication with different makers in respect to the best construction for the purpose required; and

2. That a series of experiments should be made by placing some thermometers in a hydraulic press in conjunction with one in an hermetically sealed iron bottle (as a standard) and subjecting them to pressure, that they should be kept under pressure sufficient time to allow the thermometer within the bottle to take up the temperature without, and then the whole compared with the standard.

The first suggestion was immediately acceded to; and those makers from whom the Meteorological Department obtained instruments were applied to, and a list of desiderata submitted to each. Three makers responded, and six instruments were ordered from each.

These instruments were sent in (hereafter called the Hydrographic Office pattern), and Mr. Balfour Stewart, of the Observatory at Kew, was consulted as to the *modus operandi* of testing by pressure, and he approved of that already suggested.

A difficulty arose in respect to a hydraulic press—the use of some in London could not be obtained, and others were not adapted to the purpose, so that the testing was deferred, and some of the instruments were sent to H.M.S. *Gannet*, then deep-sea sounding on the edge of the Gulf-Stream, and afterwards some to H.M.S. *Lightning* for her dredging cruise.

On the return of these vessels the conflicting nature of the temperatures obtained from those supposed to exist (as derived from observations in other localities) rendered the necessity of ascertaining the nature and amount of error due to pressure the more imperative.

At this juncture Mr. Casella undertook to have a testing apparatus constructed at his own expense, capable of producing a pressure of three tons to the square inch.

At a meeting of the Committee of the Royal Society, held in the Hydrographer's Room in April, 1869, and at which the plan of operation for testing the thermometers was discussed, that by means of an iron bottle approved. The late Dr. Miller, V.P.R.S., proposed encasing the full bulb in an outer covering of glass containing air, in order to permit the lighter fluid (air) to be compressed without affecting the bulb within, and one such was directed to be made; but instead of the outer casing being filled with air it was nearly filled with alcohol, which being heated to reduce the quantity of air, the bulb was then hermetically sealed. Mr. Casella was also directed to make others that would facilitate the observations.

At the time these experiments were proposed, it was not known that a thermometer had been constructed, at the suggestion of Mr. Glaisher, by the late Admiral Fitzroy's directions, with the view of removing the difficulty of pressure; this was done by encasing the long bulb at the back of the instrument in glass, and nearly filling the space between the case and the bulb with mercury;* and one on this principle was then in the Instrument-room of the Meteorological Office; but although some had been used for deep-sea purposes, the further issue of them had been stopped on account of their fragility, and thus the means for obtaining accurate observations were virtually the same as before.

It was decided to test them at pressures equal to the following depths in the ocean, viz., 250, 500, 750, 1,000, 1,250, 1,500, 1,750, 2,000, 2,250, and 2,500 fathoms, the rule to be applied being 33 feet = one atmosphere = 15 lb. on the square inch. From this a table was constructed for use.

On the 4th of May the following thermometers were taken to Hatton Garden, viz. :—

Nos. 56 and 57	Casella .	Hydrographic Office pattern.
66 and 67	Elliott . .	" " "
72 and 73	Pastorelli.	" " "

* See Meteorological Papers, No. I., 1863.

No. 1 . . . . Casella . Specially made with an extra-thick cylinder bulb to *defy* compression.
3 . . . . „ . Spherical bulb; extra-thick glass. This thermometer was made, at the special request of one of Mr. Casella's workmen, in order to resist effect by pressure.
4 . . . . „ . Short cylinder bulb: extra-thick glass.
6 . . . . „ . A glass cup fitting over bulb, designed by Mr. Siemens.

All the above were Six's thermometers with the bulbs unprotected.

No. 2 . . . . Casella . Glass-encased bulb, as proposed by Dr. Miller, but with the case nearly filled with spirit.
5 . . . . „ . Long cylinder bulb at the back, encased in glass, and nearly filled with spirit.

These instruments were first compared in air and then immersed in a tub of water, No. 57 being placed in an iron bottle. Set the indices and placed the thermometers in the cylinder of the press, and pumped on a pressure equal to 250 fathoms, and kept it on two hours.

It is useless to record the result of this first experiment; or it may rather be stated that the results were *nil*, except ascertaining the weak points of the process adopted.

The Miller-pattern thermometer subsequently proved so near perfection it was decided to use that as a standard for the Hydrographic Office pattern.*

It was found necessary to reduce the number of thermometers, and also of the readings, to a minimum.

With the view of testing the efficiency of Dr. Miller's pattern (No. 2) it was placed in the cylinder with No. 57, and subjected to a pressure of 4,032 lbs. (about 1,480 fathoms) for a quarter of an hour, with the following result.

EXPERIMENT NO. 1 (pressure = 1,480 fathoms).
Dr. Miller reading.

Thermometer.	Minimum.		Maximum.		Diff. of Max.
	Before.	After.	Before.	After.	
2	47°·5	47°·5	47°·5	48°·0	0°·5
57	47·5	47·5	47·5	55·0	7·5

This experiment at once proved the efficacy of the encased bulb; and the experiment was repeated with more thermometers, with the same pressure and for the same period of time.

It was found by this experiment that while the mean difference of the encased bulbs was only 0°·95, that of the two made to defy compression was 7°·25, that with the cover 10°·5, the Hydrographic Office pattern the same as in No. 1, 7°·5, and a Phillip's Alpine thermometer 70°·3.

The "Phillip's" was an ordinary make, with a very small bulb; and the great difference shown by it proved that the amount of compression is in proportion to the thickness of the glass; but in immediate connection with the subject the experiment clearly demonstrated two facts, viz.:

1. That very nearly all the difference, or error, is due to pressure on the full bulb; and

2. That by encasing the bulb we have nearly a perfect instrument.

Notwithstanding the satisfactory result obtained in enabling us to decide on a thermometer for future use, it was necessary, if possible, to establish a scale whereby temperatures already taken with instruments of the Hydrographic Office pattern might be corrected for pressure, and also to ascertain if all, or what part, of the difference shown under pressure in the Miller pattern was due to calorific effect produced by sudden compression of the water in the cylinder or by compression of the unprotected parts: preparation was accordingly made to continue the experiments.

It being necessary, as before stated, to reduce the number of the thermometers, and also the readings, to a minimum, the following were selected, viz.:—

Nos. 2 and 5 Casella . Encased bulbs.
56 and 57 „ . Hydrographic Office pattern.
73 . . . . Pastorelli „ „ „
67 . . . . Elliott . „ „ „
9641 . . . . Casella . Alpine.

These were attached to a float (to avoid immersing the hand in the water) and placed in the cylinder filled with water, to remain all night; the cistern, from which the water is pumped into the cylinder, was filled, and also a tub of water for replenishing placed by the side in order that the water in each might be, as nearly as possible, of the same temperature in the morning.

The thermometers were read in the order in which they are placed; when all were read, the indices were set as quickly as possible, and the instruments at once lowered into the cylinder and the pressure applied.

May 5. The first series of experiments were made, Mr. Casella reading.

FIRST SERIES OF EXPERIMENTS. Errors at different pressures. (*Abridged from original.*)

Thermometer.	No. 1. 250 fms. 682 lbs.	No. 2. 500 fms. 1,363 lbs.	No. 3. 750 fms. 2,045 lbs.	No. 4. 1,000 fms. 2,728 lbs.	No. 5. 1,250 fms. 3,400 lbs.	No. 6. 1,500 fms. 4,089 lbs.	No. 7. 1,750 fms. 4,771 lbs.	No. 8. 2,000 fms. 5,452 lbs.
2	1°·5	2°·1 ?	1°·0	1°·2	1°·2	1°·6	1°·4	1°·6
5	1·3	1·6	0·2	0·6	0·4	0·8	0·8	1·6
56	1·1	2·7	3·8	4·3	5·5	7·0	8·0	9·5
57	1·0	2·7	3·8	4·9	5·6	7·4	8·2	9·7
73	1·9	2·6	4·2	6·0	6·8	8·2	9·7	10·2
67	3·9	7·9	Broken†	...	...	...	...	...
66	...	...	...	...	13·3	16·4	18·7	Broken‡
Phillip's Alpine . .	...	...	...	...	...	...	...	71·0
Thomson §.	...	...	...	...	...	...	...	1·1

* I was not aware at that time of the existence of the enclosed Phillip's thermometer as designed by Sir William Thomson.
† The instrument was taken out safely, but while reading off the full bulb cracked right across.
‡ Broke at a pressure equal to 1,848 fathoms.
§ This insulated thermometer is a Phillip's encased in a glass cylinder containing a little spirit, designed by Sir William Thomson.

The thermometers were under pressure for an average time of 37 minutes in each experiment.

May 6.—The following experiment was made with the Hydrographic Office pattern (not used yesterday) for comparison. Mr. Casella reading.

Pressure = 2,000 fathoms = 5,452 lbs. Under pressure seventeen minutes.

Thermometer.	Error.
2	° 1·4
5	1·2
53	9·9
58	10·7
71	11·3
74	10·3
75	9·6
Thomson	1·0

SECOND SERIES OF EXPERIMENTS

June 21.—The thermometers were placed in the cylinder, which was filled with water; the supply-tub or cistern for pumping in from, and a tub of water standing near the press, were also filled and thus left all night.

June 22.—A dull morning, with no sun, and all conditions most favourable for observing.

Before commencing, obtained two tubs of water with 12° difference of temperature, and tested the thermometers as to time in taking up heat and contrariwise, and it was found that, by allowing the thermometers to remain under pressure eight minutes, the same results would be obtained as if they were allowed to remain half an hour or more, as in the first series of experiments.

The thermometers used were—

Standard . . .	Casella .	Dr. Miller's pattern.
No. 54 . . .	" .	Hydrographic Office pattern.
56 . . .	" .	" " "
76 . . .	Pastorelli	" " "
73 . . .	" .	" " "
Thomson . . .	Casella .	Encased (Sir William Thomson's design).

SECOND SERIES OF EXPERIMENTS. Errors at different pressures. (*Abridged from original.*)

Thermometer.	No 1. 250 fms.	No. 2. 500 fms.	No. 3. 750 fms.	No. 4. 1,000 fms.	No. 5. 1,250 fms.	No. 6. 1,500 fms.	No. 7. 1,750 fms.	No. 8. 2,000 fms.	No. 9 2,250 fms.	No. 10. 2,500 fms.
Standard .	° 0·7	° 0·7	° 1·2	° 1·5	° 1·6	° 1·5	° 1·7	° 2·0	° 2·0	° 2·2
54	1·4	3·1	3·9	5·2	6·4	7·8	8·3	9·7	11·1	12·9
56	1·8	2·8	4·0	5·3	6·3	7·8	8·8	9·9	10·9	12·0
76	1·2	2·5	4·2	4·9	6·3	7·2	8·4	9·6	10·9	11·7
73	1·4	3·0	4·6	4·9	7·4	7·8	10·2	11·5	12·3	13·7
Thomson .	0·0	0·1	0·0	0·3	0·1	0·5	0·3	0·6	0·8	0·4

The thermometers were under pressure eight minutes in each experiment.

The mean difference for each 250 fathoms by each thermometer is as follows (*abridged*):—

BY FIRST SERIES OF OBSERVATIONS

Thermometer.	Diff.
2	° + 0·20
5	+ 0·20
56	+ 1·19
57	+ 1·20
73	+ 1·27

BY SECOND SERIES OF OBSERVATIONS.

Thermometer.	Diff.
Standard	° + 0·22
54	+ 1·29
56	+ 1·20
76	+ 1·17
73	+ 1·37
Thomson	+ 0·05

EXPERIMENTS FOR CALORIFIC EFFECT.

The Phillip's encased maximum thermometers (Thomson's) being entirely protected from any effect by compression, it was decided to ascertain by their means the calorific effect produced by the sudden compression of the water in the cylinder; but, as in the two series of experiments recorded, there was such a gradual increase in the temperature of the air and also in the water used for supplying the cylinder, that for any delicate observation the conditions were not favourable; the observations for calorific effect were therefore delayed until the weather got colder, when a more equable temperature could be ensured throughout the experiment.

In order to ascertain what time it would require for these instruments to take up temperature (as it was of importance they should not be kept under pressure longer than necessary) observations were made for the purpose, and it was found that five minutes would be sufficient time for the Thomson thermometers to take up the most minute portion of heat observable.

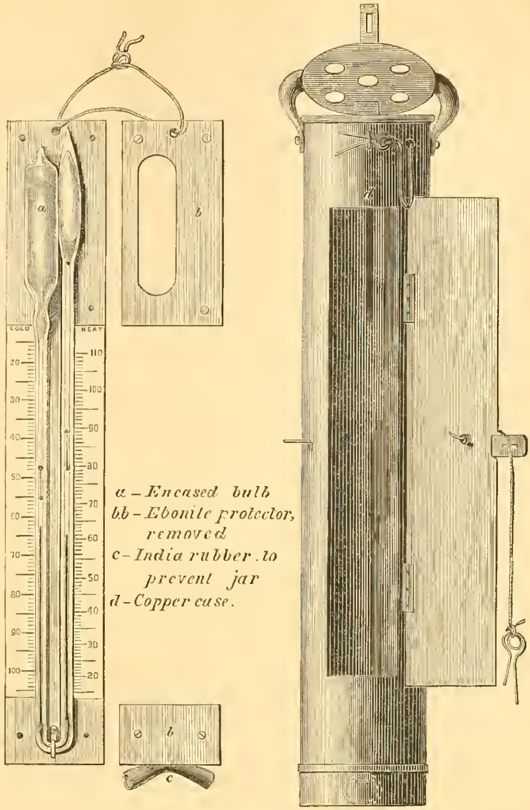
November 12.—The following observations were made day cloudy, all the conditions favourable.

No. 1. Pressure = 2,500 fathoms = 6,817lbs. Under ten minutes.

Thermometer.	Diff.	Remarks.
11,424	° + 0·1	Temperature in air . . ° 40·0
9,649	+ 0·4	" tub . . 41·6
9,645	+ 0·2	" cistern. 38·8
		" cylinder 38·0

EXPERIMENT No. 2 (same pressure). Under pressure twenty minutes.

Thermometer.	Diff.	Remarks.
11,424	° + 0·0	Temperature in air . . ° 43·6
9,649	+ 0·0	" tub . . 41·2
9,645	+ 0·2	" cistern. 41·2
		" cylinder 38·9



DEEP-SEA THERMOMETER USED BY THE HYDROGRAPHIC OFFICE

It will be observed that the water pumped into the cylinder was a little warmer than that in the cylinder; but as the valve through which it passed into the cylinder is near the top, while the bulbs of the thermometers were at the bottom, the small difference it could have made in the upper water could not have affected them.

BY MR. CASELLA (previously observed).

Thermometer.	Pressure.	Diff.	Remarks.
	fms.	°	
	500	+ 0·2	
	1,000	0·0	
	1,500	0·0	
	2,000	+ 0·2	
	2,000	+ 0·6	
	2,000	+ 0·3	

The result of the foregoing Experiments (some rejected in forming the mean):—

0·0178, calorific effect for each 250 fathoms' pressure.
0·18 " " 2,500 " "

It would seem almost unnecessary, for the purpose for which this paper is prepared, to record the above observations at all, so small is the result; but as the amount of heat caused by compression is supposed by some to be much greater, it has been thought best to give it.

EXPERIMENTS TO DETERMINE THE AMOUNT OF HEAT PRODUCED BY FRICTION.

To ascertain if any error could arise from heat created by friction in a thermometer passing rapidly through the water, one of Casella's Hydrographic Office pattern was towed astern of one of the fast river-steamers (*Naiad*), keeping the thermometer well submerged by means of a lead weight attached to the line before it; and with repeated trials at full speed not the slightest difference could be detected.

The error of the Miller-pattern thermometer as deduced from the observations (some rejected in forming the mean), *abridged*: –

Error per 250 fathoms as shown by hydraulic press	° 0·161 mean
Deduct for calorific effect	·018
True error for 250 fathoms	0·143
True error for 2,500 fathoms . . .	1·43

Mean Errors of Hydrographic Office pattern Thermometers, by testing-apparatus, corrected for calorific effect:—

Fathoms.	Casella.	Pastorelli.
	°	°
250	1 307	1·482
500	2 789	2 664
750	3·821	4·279
1,000	4·853	5·195
1,250	5·860	6·743
1,500	7·392	7·625
1,750	8 199	9·307
2,000	9·638	10·106
2,250	10 838	11·438
2,500	12 270	12·520

The Progressive Rate of Error of the Hydrographic Office pattern Thermometers, as deduced from the foregoing table, by testing-apparatus, is by Casella, equal to an increase of effect at the rate of 0°·014 per 250 fathoms' pressure; and by Pastorelli, equal to a decrease of effect at the rate of 0° 044 per 250 fathoms' pressure.

Thus, while one set of thermometers show an increase of effect under pressure, the other set denote a decrease, and the mean of the two would be so small a decrease as not to be appreciable; and the practical conclusion is, that, by the testing-apparatus, the elasticity of the glass is in exact proportion to the pressure applied.

Ocean Observations by Staff-Commander E. K. Calver

Although from the result of the experiments with the testing apparatus, a scale could be formed for the correction of the Hydrographic Office pattern thermometers, that scale may be said to be made under theoretical conditions rather than practical, and as it was necessary to verify its correctness by observations in the ocean, a number of the instruments used in the press were sent on board the *Porcupine* in 1869, and a series of most carefully taken observations were recorded by Staff-Commander Calver at the same depths as the calculated pressure applied in the press.

It is unnecessary to give the details of these observations; it will suffice to give the progressive error derived from the mean of them, and corrected for the error of the standard.

Fathoms.	Casella.	Pastorelli.
250	1·329	1·210
500	2·816	2·986
750	4 002	4·779
1,000	5·427	6·422
1,250	6·286	7·065
1,500	7·058	8 118
1,750	7·301	8·301
2,000	7·711	8·844

The progressive rate of error derived from the above is by Casella, equal to a decrease at the rate of 0·13 per 250 fathoms, and by Pastorelli, equal to a decrease of effect at the rate of 0·09 per 250 fathoms.

This result, contrary to that by the hydraulic press, proves that the elasticity is not regular or in ratio to the pressure, but that after continuing regular up to a pressure of 1,000 fathoms, it decreases in a compound ratio to a pressure of 2,000 fathoms, when its elasticity nearly ceases.

Comparison of the Hydrographic Office pattern Thermometers as found by the hydraulic testing-apparatus and by the Ocean Observations:—

Casella.

Pressure.	Error.		Per 250 fathoms.	
	Press.	Ocean.	Press.	Ocean.
fms.	°	°	°	°
250	1·307	1·329	1·307	1·329
500	2·789	2·816	1·394	1·408
750	3 821	4·002	1·274	1·334
1,000	4·853	5·427	1·213	1·357
1,250	5·860	6 286	1·172	1·257
1,500	7·392	7 058	1·232	1·176
1,750	8·199	7 301	1·171	1·043
2,000	9·638	7·711	1 205	0·964
2,250	10·838	...	1 204	...
2,500	12·270	...	1·227	...
Means			1·240	1·233
Error at 2,500 fathoms by the means . .			12·4	12·3

Pastorelli.

Pressure.	Error.		Per 250 fathoms	
	Press.	Ocean.	Press.	Ocean.
fms.	°	°	°	°
250	1,482	1·210	1·482	1·210
500	2·664	2·986	1·332	1·493
750	4·279	4·779	1·426	1·593
1,000	5·195	6·422	1·299	1·606
1,250	6·743	7·065	1·349	1·413
1,500	7·625	8·118	1·271	1·353
1,750	9·307	8 303	1·329	1·186
2,000	10·106	8·844	1·263	1·105
2,250	11·438	...	1·271	...
2,500	12·520	...	1·252	...
Means			1·327	1·370
Error at 2,500 Fathoms by the means . .			13·3	13·7

By this comparison, although the errors, as found by the two modes of observation, differ at individual depths or pressure, still the means of Casella's per 250 fathoms are almost the same, and those of Pastorelli's differ only three-tenths of a degree in 2,000 fathoms, the extent to which the comparison can be made.

There can be little doubt that, without the aid of the Miller pattern, by an extended series of observations a scale could have been obtained to correct the Hydrographic Office pattern to a very close approximation of the truth (in accordance with the proposed first intention of the experiments); but the timely suggestion of Dr. Miller has quite set at rest any difference of opinion as to the instrument for future use.

*OYSTERS IN IRELAND**

HIS Excellency the Lord Lieutenant of Ireland having had represented to him that the artificial propagation of oysters was imperfectly understood in Ireland, appointed in October 1868 Messrs. Blake, M.P., Francis, Hart, and Brady, commissioners to inquire into and report on the artificial cultivation and propagation of oysters.

The instructions to the Commission were to visit the principal places in France, England, and Ireland, where oyster cultivation is or can be carried on, to examine the best authorities on the subject, and to ascertain as far as possible the causes which have led to failures. It was also hinted that three weeks would suffice for Ireland, a

* Report of the Commission appointed to inquire into the Methods of Oyster Culture in the United Kingdom and France, with a View to the Introduction of improved Methods of Cultivation of Oysters into Ireland. (Presented to both Houses of Parliament by command of Her Majesty.) Dublin, 1870.

fortnight for England, and the same amount of time for France. The Commission proceeded in October 1868 to France to commence their fortnight's tour, and in June 1870 presented their report, which has now been laid before Parliament. The Report occupies about fifty pages; and 150 more are very usefully taken up with a series of appendices. Ten plates are also included in the volume.

The Report commences with a list of the places visited by the Commission, from which we notice the omission of Dublin Bay, although Howth and Malahide had each at one time a respectable name for oysters. It then proceeds to give the natural history of the oyster, which we pass over without further comment than that it is a pity the Commissioners did not consult some person tolerably skilled in malacology ere they printed it—to criticise it would be but to break a butterfly on a wheel. The various branches of oyster fisheries are well described, and an interesting epitome is given of Coste's labours. It would appear that the great bulk of the oysters bred at Arcachon are sent to Marennes and Tremblade, where the green tint, so much esteemed in France, is imparted to the beard of the oyster. Such a prejudice exists in England against this green tint, that the Essex oysters are largely exported to France. It should be recollected that oysters impregnated with copper have always a greenish tinge of body, while those with green beards do not owe their colour to copper but to their peculiar feeding. The reporters suggest that the Diatomaceæ are probably the cause, and give figures of some Diatoms, to which we would call the attention of Dr. Donkin, who is writing a monograph of this group; to say the least, they are very comical.

The diminution in oyster production which has taken place in England, though very considerable, is not so great as in France. The Hayling Island enclosure is described, and plans of the beds given. The various methods of oyster culture are described, and appropriate places for their cultivation are pointed out. In reference to this portion of the subject, we may refer to the elaborate report on the temperature of the surface of the sea on the coasts of Great Britain, Ireland, and France, by Prof. Hennessy, in which he deduces that:

"1. The temperature of the sea on the coast of Ireland varies within narrower limits than on the coast of Great Britain, or, in other words, it is more equable throughout the year and also during the summer season, when oyster breeding takes place.

"2. The temperature of the sea at noon on the Irish coast, especially on the south and west coasts during the months of June and July, is, upon the whole, higher than on the coast of Great Britian, and less than on the west coast of France.

"3. This temperature seems to be sufficient for the requirements of oyster breeding, and therefore, *a fortiori*, the temperature about two in the afternoon under the conditions above referred to.

"4. The highest temperature of the seas surrounding Ireland, and probably also of those surrounding Great Britain, is during the month of August, and the least during the month of February.

"5. Any advantages as to temperature possessed by the seas which wash the Irish coast are unquestionably due to the thermal influence of currents connected with the Gulf Stream."

Prof. Sullivan also appends an important Report on the Composition of the Soils of Oyster Grounds, and on the qualities which exert most influence on oyster cultivation, and comes to the conclusions:—

"1. That the influence of the soil upon the breeding and growth of oysters is complicated by: temperature, especially during the spawning season; sudden alternations of heat and cold, due to currents; alternation of depth of water, especially as regards whether the maximum of sun-heat and light concords with low water during the spawning season; velocity of tide, angle of inclination of shore, &c.

"2. That the soil of oyster grounds may be made up of materials of any of the great classes of rocks, arenaceous, argillaceous, or calcareous, provided they contain—

"3. More or less of a fine flocculent highly hydrated silt, rich in organic matter, which indicates that Diatomaceæ, Rhizopoda, Infusoria, and other minute creatures abound.

"4. That the character and abundance of such small organisms in a locality seems to be the true test of a successful oyster ground.

"5. And lastly, that although oysters do undoubtedly assimilate copper from water where mine-water containing traces of that metal flows into the sea in the neighbourhood of the oyster beds, the copper is chiefly, if not exclusively, confined to the body of the oyster, and does not appear to reach the mantle or beard. That the so-called green oysters of Essex, Marennes, and other places, on the other hand, are green-bearded and contain no copper, nor can the most minute trace of copper be detected in the soil of the oyster grounds where such green-bearded oysters are produced."

The Report concludes with the following recommendations:—

"1. That all regulations with regard to the close time around the Irish coast should be strictly maintained.

"2. That the inspectors of Irish fisheries should have power, whenever they determine to reserve a bank or any portion thereof from public dredging for the purpose of recovery, to make such arrangements as may seem desirable for keeping the restricted part free from weeds and vermin.

"3, That there should be procurable at each coastguard station, at a small cost, general information as to oyster culture, and simple instructions as to the best modes of proceeding.

"4. That the inspectors be empowered to adopt such other means as they may deem necessary to afford information and instruction to those requiring it with respect to oyster culture.

"5. That having unsizable oysters in possession in places where it is prohibited by any bye-law to take oysters from any public beds under a certain size, shall be *primâ facie* evidence that such oysters were taken in places so prohibited; such regulations not to apply to private oyster grounds.

"6. That facilities be afforded to the coast population to acquire the use of small portions of foreshore, or sea bottom, for oyster cultivation, and to obtain loans on satisfactory security for the preparation of same, and for the purchase of oysters, collectors, &c.

"7. That landed proprietors desirous of cultivating oysters on the shores adjoining their lands, be empowered to avail themselves of the provisions of the Irish Land Improvement Acts, for the purpose of oyster cultivation."

We would commend the perusal of this Report to those interested in this subject; of its importance there can be little doubt; and while we agree with the commissioners that no very extraordinary profits are to be made out of oyster culture, and that hence it is not a subject for extensive commercial speculation, yet we know of none more deserving of the attention of those interested in the general welfare of this country. E. P. W.

ARTIFICIAL MILK

AMONG the many sorrowful records of the Siege of Paris, one of the most enduring, and not the least touching in its melancholy eloquence, is afforded by the

Comptes Rendus of the Academy of Science. The construction, the filling, the guiding, and general management of balloons, occupied so much of the attention of the Academy, that, if all other records of the Siege were lost, its date and effective duration might be pretty accurately determined by the sudden appearance, the continuance, and sudden cessation of these abundant papers on aërostation.

There is another series of papers of equal, if not greater significance, viz., those on the utilisation of strange materials for food, the economising of waste nutritive materials, and their substitutive uses.

The investigations on these subjects have led to more practical results than the papers on aërostation. This has been especially the case with the researches that are described in the papers of M. Boillott, M. Dubrunfant, and M. Charles Fua, on "Alimentary Fats."

"Alimentary fats" is a wide expression, including some rather unsavoury hydro-carbons and very curious refuse materials. The main object of these investigations was to determine how such substances may be "usefully employed in alimentation," or, in plain unsophisticated English, how to make butter from candle-ends, dirty dripping, colza oil, fish oils, the refuse of slaughter houses, the restored grease of the wool-dresser, &c. The general result has proved that the "frying process"—which was not altogether unknown to certain enterprising Englishmen before the investment of Paris—is triumphant over all its rivals; that by simply raising the fat to 140° or 150° Centigrade, and in the mean time cautiously sprinkling with water, the cellular tissue, the volatile oils, the rancidity, offensive odours, and all other non-sentimental impediments to "alimentation," are removeable.

This frying process has already effected something like a revolution in the industry of soap-boilers, some of whom have changed their trade to that of butter-fryers. We may thus explain the remarkable fact that, although the excessively dry summer of 1870 reduced the dairy produce of England to about half the average, and had nearly the same effect on our other sources of cow-butter supply, there was no material reduction in the supply or consumption of fresh butter for the London and Provincial markets during the following winter, the only notable disturbance which occurred being in the demand for kitchen-stuff and empty Dutch butter-tubs.

M. Dubrunfant is not content with superseding the cow in the mater of butter, but has subsequently made similar attempts upon milk. He proceeds in a strictly scientific manner, commencing with the following summary of the results of Boussingault's analysis of cow's milk:—

Nitrogenous material (caseine and albumen)	0·0337
Fatty material (butter)	0·0376
Sugar (of milk)	0·0567
Salts	0·0020
Water	0·8700

Quoting the observations of Payen and others which show that milk is alkaline, and owes its alkalinity to soda, he proceeds to refute the theory of churning which has been generally adopted by microscopists, viz., that the fat globules in milk are invested with a delicate membrane which is ruptured in the churn, and thereby permits the agglomeration of the fatty material into butter.

M. Dubrunfant contends that milk is simply an emulsion of neutral fatty matter in a slightly alkaline liquid, such as can be artificially imitated; and that the process of churning consists in hastening the lactic fermentation, thereby acidifying the serum of the milk, and at the same time agglomerating the fatty matter which the acidity sets free from its emulsion. He further controverts the cellular theory by showing that the fat globules of milk do not display any double refraction, as do all organised membranous tissues.

Having thus examined the theoretical constitution of milk, he proceeds to the practical method of imitating it, and gives the following directions: Add to half a litre of water forty or fifty grammes of saccharine material (cane sugar, glucose, or sugar of milk), twenty or thirty grammes of dry albumen (made from white of egg), and one or two grammes of subcarbonate of soda. These are to be agitated with fifty or sixty grammes of olive oil or other comestible fatty matter until they form an emulsion. This may be done either with warm or cold water, but the temperature of 50° to 60° C. is recommended. The result is a pasty liquid, which, by further admixture with its own bulk of water, assumes the consistency and general appearance of milk.

Luxuriously-minded people who prefer rich cream to ordinary milk can obtain it by doubling the quantity of fatty matter, and substituting two or three grammes of gelatine for the dry albumen. The researches of Dumas and Fremy having reinstated gelatine among the nitrogenous alimentary materials, M. Dubrunfant prefers gelatine to albumen; it is cheaper, more easily obtained, and the slight viscosity which it gives to the liquid materially assists the formation and maintenance of the emulsion. He especially recommends this in the manufacture of "siege-milk," on account of the obviously numerous articles from which gelatine may be obtained.

The uses of artificial milk need not be limited to supplying the wants of the residents of besieged cities. As an ordinary element of the human breakfast table, it is not likely to supersede the product of the cow, but calves are suggested as being superior to vulgar human prejudices. In the ordinary course of rearing, these animals demand a large proportion of the milk of their mothers, and are commonly ill-fed or prematurely sacrificed on that account. By feeding them luxuriously on artificial milk (which may be still further cheapened by using colza oil, which has been rendered tasteless and alimentary by the frying process above described), the milk, butter, and cheese of the cow may be considerably economised, and the supply of veal improved both in quantity and quality, by keeping the calves a much longer time before they are killed.

I might make further suggestions in the direction of "dairy-fed pork," &c., but this is unnecessary, the commercial instinct is sufficiently strong to avail itself of all such cheapening applications of science. Those who are professionally engaged in detecting the adulterations of food will do well to study the physical peculiarities by which M. Dubrunfant's milk may be distinguished from that of the cow, both in the ordinary and condensed form. By substituting vegetable albumen for the white of egg or gelatine, the vegetarian may prepare for himself a milk that will satisfy his uttermost aspirations.

W. MATTIEU WILLIAMS

NOTES

THE following telegrams have been received from the Eclipse Expedition since our last:—"MANGLORE, Wednesday, Dec. 6.—We have landed here from the flagship; all well. The Government arrangements are admirable. The weather is promising. The parties are posted as arranged." From N. R. Pogson, at Avenashy, to the Astronomer Royal, Royal Observatory, Greenwich:—"Weather fine; telescopic and camera photographs successful; ditto polarisation; good sketches; many bright lines in spectrum.—Dec. 12." From Colonel Tennant, F.R.S., Dodabeta, Ootacamund, to W. Huggins, F.R.S., Dec. 12, 9.15 A.M.:—"Thin mist. Spectroscope satisfactory. Reversion of lines entirely confirmed. Six good photographs."

AT the meeting of the Geological Society on the 6th inst., the President announced the bequest to the Society, on the part of the late Sir R. I. Murchison, of the sum of 1,000*l.*, to be invested in the name of the Society or its trustees, under the title of the "Murchison Geological Fund," and its proceeds to be annually devoted by the Council to the encouragement or assistance of geological investigation. The donation of the proceeds of the fund was directed by the testator to be accompanied by a bronze copy of the Murchison Medal.

At the meeting of the Royal Geographical Society on Monday last, Sir Henry Rawlinson stated that the Council intended to address the Foreign Office, with a view of arranging, either directly from the Foreign Office, or through co-operation between the Foreign Office and the Society, some means of communicating with Dr. Livingstone, either by sending messengers into the interior of Africa, and offering a reward of 100 guineas to any African who will bring back a letter in Dr. Livingstone's handwriting to the sea-coast, or by organising a direct expedition, headed by some experienced and well-qualified European, who should himself penetrate to the point where Dr. Livingstone is supposed to be.

By a decree, dated April 18, 1866, of the Minister of Public Instruction in France, a prize of 50,000fr. (2,000*l.*) was offered for the most useful application of the Voltaic Pile, the period for competition to expire in April 1871. From a report of the minutes presented by the President of the Republic, it appears that candidates are few in number, and that in the opinion of the *savants* to whom the memoirs were submitted, none is of sufficient merit to have earned the prize. By a decree of the 29th of November, the competition is now extended for another period of five years, to terminate on November 29, 1876.

We learn from the *Lancet* that the promoters of the scheme for commemorating the life and labours of John Goodsir, late Professor of Anatomy in the University of Edinburgh, have got only 700*l.* instead of 2,000*l.*, and have had to relinquish the idea of a fellowship, and adopt that of a triennial prize, to be open to all graduates of the University of not more than three years' standing, to be given for an essay or treatise containing the results of original investigations in anatomy, human and comparative, either normal or pathological, or in experimental physiology. The Acting Committee of the Association for the better Endowment of the University of Edinburgh have prepared the deed of endowment for the Syme Memorial. The capital sum amounts to 2,500*l.*; whereof 2,000*l.* were paid over to the Association by the Syme Memorial Committee, and 500*l.* was added by the Association.

The authorities of the Museum of Comparative Zoology at Harvard College have placed in Prof. Allman's hands for determination the whole of the collection of hydroid zoophytes obtained by the United States Coast Survey during its late exploration of the Gulf Stream.

The Council of the University of Edinburgh has decided to take into consideration on the 21st inst. the appeal against the decision of the Senate as to rescinding the regulations for the education of women in medicine.

The Examiners in the Natural Science School at Oxford (W. Ogle, M.D., Corpus; J. A. Dale, Balliol; and R. H. M. Bosanquet, St. John's) on Saturday issued the subjoined class list:—Class I.—H. A. Black, Christ Church; W. T. Goolden, Magdalen; E. H. Jacob, Corpus; A. S. L. Macdonald, Merton; J. A. Ormerod, Jesus; A. G. Rücker, Brasenose; S. H. West, Christ Church. Class II.—E. H. Forty, Christ Church; J. Turner, Exeter; J. L. Twynam, St. Mary Hall. Class III.—*Nil.* Class IV.—*Nil.*

Mr. W. A. Brailey, who was second in the Natural Sciences Tripos at Cambridge, has been elected a Fellow of Downing College in that University.

M. Georges Delaporte, engineer of M. Tessié de Motay's Oxy-hydrogen Light Company, has been nominated a Chevalie of the Legion of Honour, as an acknowledgment of the services rendered to the State during the Siege of Paris in the application of the Electric Light to strategic operations.

The Lord President of the Council has nominated Mr. T. S. Aldis, formerly scholar of Trinity College, Cambridge (Second Wrangler in 1866), to be an Inspector of Schools.

The following are now announced as the probable arrangements for the Friday evening meetings at the Royal Institution before Easter 1872:—January 19, Mr. William R. Grove, F.R.S., on Continuity; January 26, the Archbishop of Westminster, on the Demon of Socrates; February 2, Prof. Odling, F.R.S., on the new metal Indium; February 9, Prof. Humphry, F.R.S., on Sleep; February 16, Dr. Gladstone, F.R.S., on the Crystallisation of Silver and other Metals; February 23, Mr. Henry Leslie, on the Social Influence of Music; March 1, Mr. C. W. Siemens, F.R.S., on Measuring Temperatures by Electricity; March 8, Mr. R. Liebreich, on the Effect of certain Faults of Vision on Painting, with especial reference to Turner and Mulready; March 15, Mr. John Evans, F.R.S., on the Alphabet and its Origin; March 22, Prof. Tyndall, F.R.S.

We learn from *Les Mondes* that the lamentable disagreement between M. Daubrée, the director of the mineralogical department of the Museum of Natural History at Paris, and his assistant, M. Stanislas Meunier, is now happily terminated, and that the latter is again permitted to carry on his researches at the Museum.

The Exhibition of the Photographic Society, held in its rooms in Conduit Street, closed on Saturday last. While among specimens of portraits the works of Grasshofer of Berlin, Rylander of Paris, and other Continental artists, challenged comparison with any of our home productions, there can be no question that in landscape photography, the exquisite workmanship of Bedford, Robinson, Cherrill, and some other English photographers, easily bore off the palm. There were some very fine specimens of Edwards's heliotype process, as well as of the autotype and other carbon-printing processes.

We learn from the *American Naturalist* that the State Microscopical Society of Illinois has issued a prospectus of *The Lens*, a Quarterly Journal of Microscopy and the Allied Natural Sciences; with the Transactions of the State Microscopical Society of Illinois. It will be an octavo, each number containing at least forty-eight pages of reading matter. Terms, 2 dols. per annum in advance. The editor will be Mr. S. A. Briggs, 177, Calumet Avenue, Chicago. Though its appearance has been delayed by the fire, we learn that it will soon be issued.

At a recent meeting of the Asiatic Society of Bengal Mr. W. T. Blanford exhibited a collection of chipped quartzite implements found about forty miles west of Bhadráchalam, on the Godávarí. The thirty-five specimens exhibited were all found within a space of about fifty yards square, and at least as many more were rejected on account of being badly made. The place where they were found was in dense jungle, the rock soft sandstone, and the implements, as was usually the case in Southern India, had evidently been chipped from pebbles. Several were formed of white vein quartz, an unusual circumstance. The forms of these implements were those of the kind most frequently found in French and English gravels, and they varied from about 3in. to 6in. in length. That the spot where they were found was a place of manufacture was probable, not only from the occurrence of ill-formed implements, but also from flakes, evidently chipped from the quartzite being abundant.

A very beautiful and extraordinary Aurora Borealis was witnessed at Montreal on November 21. The following account of the phenomenon has been sent us by Dr. Smallwood of the Montreal Observatory :—A few minutes past 5 o'clock yesterday evening, the eastern horizon showed a bank of cumulo-stratus clouds, which reached to an altitude of 9°, behind which was discernible an auroral light, which increased in intensity as the darkness became more dense. At 5.30 a diffused light of a bright crimson colour occupied the whole of the eastern and north-eastern horizons. Rising behind this bank of clouds, streamers were frequently observed, reaching to the constellation Cassiopeia. The light was frequently so dense as to prevent even the stars δ and γ Ursæ Majoris being seen through it. While these appearances were present, a far more brilliant display was seen in the north-west, triangular in shape, its base hidden by the Mountain, but which appeared about 10° in breadth, and extended upwards, passing part of the constellations Hercules, Corona Borealis, and Draco, to the zenith. The bright crimson colour was very intense ; its edges were occasionally softened by a band of narrow streamers of a palish green colour. Stars below the third magnitude were hidden from view, owing to the great density of this light. Small patches of cumulus clouds were seen passing across and in front of this display. The surpassing beauty of these appearances has rarely been equalled. At 6.15 P.M. the intensity of the brightness was much diminished, and at 7 only a soft auroral light was visible in the north and north-east. The declination magnet was very sensibly deflected from its normal state, showing a great easterly variation. The weather during the day was comparatively warm (having succeeded a slight fall of snow), with a rising barometer, which at 6 P.M. stood at 29·902 inches. Thermometer, 37°. Humidity, ·806. Wind west ; velocity three miles per hour.

PROF. PANCERI, of Naples, has been studying for some time past the phosphorescence of marine animals. He has examined *Noctiluca*, *Beroe*, *Pyrosoma*, *Pholas*, *Chætopterus*, and has lately published a paper on the phosphorescence of *Pennatula*. He finds in all cases that the phosphorescence is due to matter cast off by the animal—it is a property of dead separated matter, not of the living tissues. In all cases (excepting *Noctiluca*) he also finds that this matter is secreted by *glands*, possibly special for this purpose, but more probably the phosphorescence is a secondary property of the secretion. Further, the secretion contains epithelial cells in a state of fatty degeneration, and it is these fatty cells and the fat which they give rise to which are phosphorescent. Hence the phosphorescence of marine animals is brought under the same category as the phosphorescence of decaying fish and bones. It is due to the formation in decomposition of a phosphoric hydro-carbon, or possibly of phosphuretted hydrogen itself. In *Pennatula* Prof. Panceri has made phosphorescence the means of studying a more important physiological question—namely, the rate of transmission of an irritation. For when one extremity of a *Pennatula* is irritated, a stream of phosphorescent light runs along the whole length of the polyp-colony, indicating thus by its passage the rate of the transmission of the irritation. This admits of accurate measurement, and furnishes data for extending Helmholtz's and Donder's inquiries to animals so widely separated from their "Versuchs-thiere" as the *Cælenterata*. It is also a proof of the thoroughness of Prof. Panceri's investigation that he has made use of the spectroscope for studying the light of phosphorescence.

ATTENTION has been called in *Harper's Weekly* to the injuries to the Florida submarine cable supposed to have been caused either by the bites of the sea-turtles, or from some kinds of fish ; and we now learn that in China a similar difficulty has been experienced in consequence of the attacks of a minute crustacean. This is so small as scarcely to be perceptible to the naked eye, but can be readily defined under the microscope. Various breaks have been satisfactorily referred to the agency of these animals, which had embedded themselves in the gutta percha. It has become necessary, therefore, to envelop the cables in certain localities with an external supplementary layer of metallic wire, in order to prevent injury in this manner.

WITH a commendable promptness, the first volume of the Annual Report of the United States Commissioner of Patents for 1870 has made its appearance, and inaugurates the new order in regard to this document. Instead of publishing the specifications of the patents with wood-cut illustrations, the present volume embodies—first, an alphabetical list of patentees during the year ; second an alphabetical list of the patents extended during the year ; next, an alphabetical list of inventions and of reissues. It will be remembered that at the present time the patents are printed in detail, accompanied by photo-lithographic drawings of working size, 150 copies being published, some of them to be distributed, and sets placed for free public inspection in the various State and Territorial capitals, and in the clerks' offices of the District Court of the various judicial districts throughout the United States. The issue of additional copies is also authorised in proportion to the demand, to be sold at a price not exceeding the contract price for such drawings. The total number of patents issued during the year 1870 amounted to 13,321, of which considerably the largest number were made out to citizens of New York, Pennsylvania, Massachusetts, Ohio, Illinois, Connecticut, Indiana, and Michigan, in the order mentioned.

THE *Mechanics' Magazine* states that amber is reported by the collectors as being sometimes found in a soft "unripe" state ; Herr H. Spirgatis was fortunate enough to receive a specimen from the Baltic, near Brusterort, East Prussia. Its interior consisted of an almost transparent yellow resin, surrounded by a thin opaque crust. When freshly broken the centre was soft and elastic, but on exposure to the air it soon became hard and brittle. Its analysis differed so much from that of amber, that though it evidently belongs to the same class of substances, it is not to be mistaken for it. Its percentage composition agrees with that of Benntheim asphalte, and with the fossil resins from the East Indies, examined by Duflos and Johnston.

ON Nov. 7 at 2.30 P.M. a slight earthquake was felt at Smyrna. It was simultaneous at Mytilene and Cheshmeh.

TWO smart shocks of earthquake were felt at Cavalla in Macedonia at 11 P.M. on Nov. 28.

ON Oct. 13 an earthquake was felt in the fort of La Libertad, at 11 P.M. It was also felt at La Union and Nicaragua.

ABOUT a year ago many English and foreign scientific journals, following the *Bulletino Romano*, announced that a large meteorite had fallen near the town of Murzuk, in December 1869. M. Rose has lately made a communication to the Berlin Academy, in which he states that the results of his inquiries made both at Tripoli and Murzuk have shown that no such fall was ever observed, much less that any such meteorite had been found.

IT is stated in *Land and Water* that the whole of the pack of fox-hounds of the Durham County Hunt has been condemned to be destroyed in consequence of the prevalence among it of a form of hydrophobia defined as "dumb madness," which has run through the kennels, and has carried off twelve couple of hounds. As to the details of this "dumb madness," it will be interesting to hear more of the exact symptoms attending it. Old works upon canine diseases used to specify seven species of canine madness, "dumb madness" among them, the last and worst being "running madness," which was undoubtedly hydrophobia, though probably many other phases of so-called madness were simply distemper, which in primitive days was little understood as a specific disease.

SCIENTIFIC SERIALS

Journal of the Franklin Institute, September. This number opens with numerous editorial notes, principally abstracts from other scientific journals; there is also the commencement of a description of the Stevens Institute of Technology in Hoboken. Amongst the notes we notice an account of Grubb's automatic spectroscope, and a description of the properties of Nitroglycerine as found by M. Champion. It is stated that when pure it may be heated up to 200° without explosion, but at 257° it deflagrates violently; and although it explodes with terrific force by a blow, the electric spark does not affect it. A number of original communications follow. Under the head of Civil and Mechanical Engineering, we find a paper containing some useful "formulæ, rules, and examples for cases of earth-work under warped and plain surfaces," and another "On Descriptions of Wood-working Machinery." Under mechanics, physics, and chemistry, there is a paper "On Apparatus Illustrating Mechanical Principles;" the various pieces of apparatus are intended to show experimentally the truth of problems, such as the parallelogram of forces, the parallelopipedon of forces, and so on; a machine is also described to illustrate the action of the forces of gravity and projection in giving a projectile its parabolic trajectory. They are designed by J. Pemberton, and seem to be well adapted to the various purposes which have hitherto been neglected. The continuation of a lecture on the sun by Dr. Gould follows; he deals shortly with the prismatic analysis of light and with the solar spectrum, explaining the curves of thermal, luminous, and chemical intensity. Prof. Leeds contributes a valuable paper for the use of students "On the Measurement of the Angles of Crystals," and Mr. Coleman Sellers reviews Mr. Crookes's Experimental Investigation of a New Force; he boldly states that he believes Mr. Crookes has been deceived, giving several reasons why he is of this opinion. An editorial note is attached to this paper, stating that Mr. Sellers is very accomplished in the field of legerdemain, which would lend peculiar value to his view.

Journal of the Franklin Institute, October. The editorial notes contain several valuable abstracts, amongst which may be noticed one on Fluorescence, originally published by E. Lommel in the "Repert. der Physik." From his observations Lommel shows that Stokes's law "that the refrangibility of the exciting rays is always the upper limit of the refrangibility of the excited rays" does not always hold good, and also that the very common opinion that Fluorescence is an action by which refrangible rays are converted into less refrangible rays is not altogether true.—Prof. Thurston communicates a report "On a Steam Boiler Explosion," to which is added a clear statement of many of the causes of such explosions. Prof. Heines contributes the first of a series of papers on binocular vision; he deals shortly with the human eye and monocular vision, and then proceeds to some phenomena of binocular vision. The last paper was read before the American Association for the advancement of Science by Prof. Owen, "On Physiographic and Dynamical Geology involving the discussion of Terrestrial Magnetism," in which it is thought probable that the sun is the source of the modifications on the earth, giving the form and dimensions to the land, and that magnetism, either directly or by conversion into chemical force, has been the most powerful agent in causing various natural phenomena, such as the geysers, volcanoes, ocean currents, &c.

SOCIETIES AND ACADEMIES

London

Royal Society, December 7.—"On the Fossil Mammals of Australia. Part VI. Genus *Phascolomys*, Geoffr."—By Prof. Owen, F.R.S. In this paper the author premises a reference to former ones on the Osteology of existing *Marsupialia*, in the "Transactions of the Zoological Society," and to his "Catalogue of the Osteological Series in the Museum of the Royal College of Surgeons," in which are defined cranial characters serving to distinguish existing species of the genus *Phascolomys*, Geoffr.; and after showing, in subsequently received materials, the kind and extent of variety of such characters in the same species, he proceeds to apply the knowledge so gained to the determination of some fossil remains of species of Wombat, similar in size to the known existing kinds. The extinct *Phascolomys Mitchelli*, indicated by remains brought to England in 1835 by Sir Thomas Mitchell, C.B., the discoverer of the bone-caves of Wellington Valley, Australia, is determined by specimens subsequently obtained by Prof. Alex. M. Thomson and Mr. Gerard Krefft, from the same caves. A second species, distinguished by characters of the nasal bones, is called after its discoverer *Phascolomys Krefftii*. Modifications of the lachrymal, maxillary, and palatal bones in the existing kinds of Wombat are also applied to the determination of the fossils: specimens from the fresh water deposits of Queensland are thus shown to belong to the species *Phascolomys Mitchelli*, originally founded on fossils from the breccia-caves of New South Wales. The author next proceeds to point out the characters of the mandible in existing Wombats, available in the determination of extinct species of *Phascolomys*. On this basis he defines specimens which he provisionally refers to his *Phascolomys Krefftii*. He then points out the mandibular characters of *Phascolomys Mitchelli*, and shows that the existing *Phascolomys latifrons* was represented by mandibular fossils from the breccia-caves of Wellington Valley. Proceeding next to the description of fossil mandibular remains of the genus *Phascolomys* from the fresh water deposits of Queensland, the author defines *Phascolomys Thomsoni*, *Phasc. platyrhinus*, and *Phasc. parvus*. The latter, seemingly extinct, species is markedly inferior in size to any of the known existing species. An account of the extinct kinds of Wombat, exceeding in size the existing species, will be the subject of a succeeding communication. The present is illustrated by subjects occupying seven plates and eight woodcuts, all the figures being from nature, and of the natural size.

"On Fluoride of Silver. Part III." By G. Gore, F.R.S.

"On the Solvent Power of Liquid Cyanogen." By G. Gore, F.R.S.

Zoological Society, December 5.—John Gould, F.R.S., V.P., in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the months of October and November 1871, and called particular attention to a young female specimen of the Cape Fur-Seal (*Otaria pusilla*), presented by Sir Henry Barkly, Governor of the Cape Colony, being the first example of this interesting animal received alive in Europe.—A letter was read from Dr. Burmeister, of Buenos Ayres, containing remarks on Messrs. Sclater and Salvin's "Synopsis of the Cracidæ," published in the Society's "Proceedings" for 1870.—Dr. E. Hamilton exhibited and made some remarks on an adult skull of the newly-discovered Chinese Deer (*Hydropotes inermis*), and compared it with an immature skull of the same species exhibited by Mr. R. Swinhoe at a meeting of the Society, February 10, 1870. Dr. Hamilton also drew attention to the statement made by his correspondent respecting the wonderful fecundity of this animal, which tended to corroborate the facts stated by Mr. Swinhoe on that occasion.—Mr. Sclater exhibited and remarked on a skin of the Water Opossum (*Chironectes variegatus*), which had been sent to him by Mr. Robert B. White, from Medillin, United States of Columbia.—Prof. Newton exhibited and made remarks on the humerus of a Pelican (believed to be *Pelecanus crispus*), which had been found in the English fens.—A communication was read from Surgeon Francis Day, Inspector-General of Fisheries of British India, containing remarks on the fresh-water Siluroids of India and Burmah, with observations on the range of the species, their classification, and general geographical distribution.—Mr. A. G. Butler read a paper on a small collection of Butterflies made at Loanda, the capital of the Portuguese Settlements of Angola. A second paper by Mr. Butler gave the description of a new genus of Lepidoptera, allied to *Apatura*, which was proposed to be called *Eulaceüra*.—A paper by Mr. E. A. Smith was read, containing a list of species of Shells from the Slave Coast, West Africa, collected by the late Commander Knocker, R.N., the majority of which had been dredged at Whydah, on the Dahomey shore.—Prof. Newton communicated some notes by Herr Robert Collett, of Christiana, on the singular asymmetry of the skull in Tengmalm's Owl (*Strix tengmalmi*).—Mr. Sclater read the third and final portion of a series of notes on rare or little-known animals now or lately living in the Society's Gardens. Mr. Sclater gave an account of a collection of Birds from Oyapok, on the river of the same name which divides Cayenne from the northern frontier of Brazil, amongst which were two species believed to be undescribed, and proposed to be called *Ochthoëca murina* and *Heteropelma igniceps*. A third communication from Mr. Sclater contained remarks on the species of the genera *Myiozetetes* and *Conopias*, belonging to the family Tyrannidæ.—Mr. E. W. H. Holdsworth read some notes on the Red-spotted Cat (*Felis rubiginosa*) of

Ceylon, and its varieties. — Mr. D. G. Elliot read a paper on various Felidæ, rectifying the synonomy of several species, and giving a more perfect description of one recently obtained from North-West Siberia, which he proposed to call *Felis euptilura*. Dr. Günther made a reply to some critical remarks in a paper by Surgeon Francis Day, read at a recent meeting of the Society.

Geologists' Association, December 1.—The Rev. Thomas Wiltshire, M.A., F.G.S., president, in the chair. — "On the Glacial Drifts of North London," by Mr. Henry Walker. These drifts were described under the classification and nomenclature given to the glacial deposits by Mr. Searles V. Wood, jun. They were traced from East End (Highgate) and Muswell Hill to Finchley, Colney Hatch Lane, and Whetstone. The profusion of chalk found in the glacial clay at these places bears out the designation of the main deposit in south-eastern England as the great Chalk Boulder Clay; but it is also found that the sands and gravels of the Middle Glacial, which Mr. Wood seems to restrict to a much lower horizon than Finchley, are also to be found at these localities. At Whetstone the Chalky Boulder Clay is found overlying twenty-five feet of gravel and sand, and in the apparently corresponding beds at Finchley and Hendon Lane, drift fossils and casts are occasionally found. Mr. Henry Hicks agreed with the conclusion that these sands and gravels are Mr. Wood's Middle Glacial. Mr. Caleb Evans thought that the heights to the north of London marked the southern termination of the glacial drift. Mr. Batt considered that the Drift had extended to the country south of the Thames. Several other gentlemen took part in a very animated discussion.—Collections of fossils and boulders from the Middlesex Drift were exhibited, and a quantity of peat obtained from the same source, was shown by Mr. J. T. B. Ives.

Society of Biblical Archæology, December 5.—Prof. Donaldson, B.A., F.R.S., in the chair. A paper by the Chev. de Saulcy, membre de l'Institut, "On the true sites of Capernaum, Chorazin, and Bethsaida (Julius)" was read by the secretary. In the chevalier's paper, which took the form of a letter (addressed to the Dean of Westminster), he stated that, having considered the whole tenor of the argument first advanced by him in the *Revue Archæologique* twenty years ago, he could come to no other conclusion than that the traditional town of Bethsaida and the identification of Kerâzeh as Chorazin and Tel Houm as Capernaum were unsupported by geographical evidence, and were contrary to the express statements of Josephus, who would be sufficiently exact in describing the town where he was wounded. At the same time the ruins of Kerâzeh were too extensive to be those of insignificant village like Chorazin; and those of suppositious Bethsaida were too few, and contained no indications of the Family Mausoleum of Herod Philip. The conclusion of the author was that Tel Houm was more probably the real site of Capernaum. A considerable amount of philological evidence illustrated these statements. On the close of the reading of this paper an interesting discussion ensued, in which the chairman and the following gentlemen took part:—Mr. W. R. A. Boyle, Dr. Cull, Mr. S. M. Drach, Mr. John Macgregor, and Captain Wilson.

Entomological Society, December 4.—A. R. Wallace, president, in the chair.—Mr. Shearwood exhibited an extraordinary variety of *Argynnis aglaia*, taken at Teignmouth. Mr. Bond exhibited varieties, or malformations, of various British *Lepidoptera*.—Mr. Janson exhibited a large collection of insects (chiefly *Coleoptera*) from the diamond fields of South Africa. —Mr. Higgins exhibited examples of *Tetracha crucigera* of MacLeay, from Australia.—Prof. Westwood made some remarks concerning *Papilio Thersander*, figured by Donovan, and arrived at the conclusion that this species (figured originally by Jones in his "Icones") was founded on the combination of a *Papilio* with *Charaxes Fabius*. A discussion ensued concerning the right of named figures of insects, by the older authors, to be regarded in questions of priority.—With reference to the question of the liability of large dragon-flies to the attacks of birds, Mr. Müller called attention to a statement by Natterer, to the effect that some species of *Falconidæ* habitually prey upon dragon-flies. Mr. Horne stated that during his residence in India he had never seen those insects attacked by birds of any description.—Major Parry communicated notes concerning *Lissapterus Howittanus*, and Mr. W. F. Kirby on the synonymy of various Lepidoptera.

Linnean Society, December 7.—Mr. G. Bentham, president, in the chair. "On the formation of British Pearls and their possible improvement," by R. Garner. The author referred to the theory, now generally adopted, that the production of pearls in oysters and other mollusks is caused by the irritation produced by the attacks of the minute parasite known as *Distoma*, and believed that, by artificial means, this parasite might be greatly increased. British pearls are obtained mostly from species of *Unio*, *Anodon*, and *Mytilis*, but it is probable that all mollusks, whether bivalve or univalve, with a nacreous lining to the shell, might be made to produce pearls. An interesting discussion followed, in which Mr. Gwyn Jeffreys, Mr. Holdsworth, and Dr. Murie took part.—"On certain Coleopterous Larvæ," by Dr. Burmeister, of Buenos Ayres.—"On the Botany of the Speke and Grant expedition," by Lieutenant-Colonel Grant. Notwithstanding the difficulties of their journey, and that they had more than once to destroy or abandon their whole collection, Captain Speke and Captain Grant succeeded in bringing home between 700 and 800 species of plants, many of them entirely new, which have been described by Prof. Oliver, and will be published in the "Transactions" of the Society, with at least 100 plates.

Anthropological Institute, December 4.—Sir John Lubbock, Bart., president, in the chair.—Messrs. J. Cordy Burrows, J. Park Harrison, and P. C. Sutherland were elected members. Captain Richard F. Burton read his second paper on "Anthropological Collections from the Holy Land." The paper included a *catalogue raisonné* of articles presented to the museum of the institute, found by Mr. John S. Rattray at Sáhib El Zamán (Lord of the Age), the reputed tomb of Hezekiah. This "find" consisted of fragments of human skulls and long bones, old copper bracelets, brass bracelets, coins, bits of lachrymatories (the glass being highly iridescent), portions of Syrian majolica of the type of that usually made at Damascus by the Tartars, beads of various kinds, &c. The tomb was situated in a hollow on the Eastern slope of the Libanus, and proved to be an artificial cavern, with a shaft for ventilation. A full detailed description of this very interesting discovery was given. Another interesting discovery made by Captain Burton was at the upland village of Ma'alulah, distant three hours from the large convent Saidnáyá, roughly speaking N.E. of Damascus, and occupying a position on the N.E. ranges of the Anti-Libanus. This find consisted of various fragments of skulls and lower jaws, which, together with the human remains from the tomb at Sáhib El Zamán, were described by Dr. Carter Blake. The third part of Captain Burton's paper was occupied by an account of a series of flint and stone implements and flakes, and articles of bronze and bone found near Bethlehem. In a detailed description of these articles Mr. John Evans, F.R.S., pointed out for special notice a basaltic hammer, which differed from the usual form of similar instruments discovered in Scandinavia, in Britain, and in North America, inasmuch as in the specimen the lateral depressions were absent.—Prof. Busk, F.R.S., read a communication from the Rev. Mr. Dale on flint implements from the Cape of Good Hope, which were exhibited on the table; and Mr. F. W. Rudler, F.G.S., exhibited a stone implement of unique form, also from the Cape. The President submitted for inspection some stone implements of rare beauty from Greece.

Quekett Microscopical Club, Nov. 24.—Prof. Lionel S. Beale in the chair. A paper was read by Mr. M. C. Cooke on "The Minute Structure of Tremelloid Uredines (*Podisoma*)," in which the structure of the Tremelloid masses, commonly found on juniper bushes, was detailed, together with the results of the observations of Tulasne, Oersted, and others on the germination and development of these fungi, with a critical examination of the species described under the genera *Gymnosporangium* and *Podisoma*. It was held by the author that no good foundation existed for the constitution of two genera, since the minute structure and development of both were identical. Some conversation ensued on the phenomena of alternation of generations which these and other fungi present, and especially in cases where some of the phases of existence were presumed to be passed on different hosts. Especial reference was made to the opinions entertained by Prof. Oersted that the Podisomas were found in one state parasitic on leaves of Pomaceous trees, as Rœstelias, &c., in another stage inhabiting the branches of junipers, as Podisoma. The author of the paper did not consider that this supposed phenomenon was satisfactorily proved.

MANCHESTER

Manchester Literary and Philosophical Society, October 31. — E. W. Binney, F.R.S., president, in the chair.—

Mr. Wm. Boyd Dawkins, F.R.S., gave a short account of the discoveries in the Victoria Cave, made since the last account was published in the Transactions of the Society. The clay forming the bottom of the cave, and which hitherto had been barren, was now yielding broken fragments of bone, some of which had been gnawed by the cave-hyæna. A lower jaw of this animal was found, which indicated the presence of the characteristic Pleistocene mammalia in a part of Yorkshire in which they had not been known to have existed up to the present time. There were, therefore, three distinct groups of remains in the cave, the Romano-Celtic on the surface, the Neolithic beneath, and lastly that which has been furnished by the clay which is glacial in character. And since two feet of talus had been accumulated above the Romano-Celtic layer during the last 1,200 years, it is very probable that the accumulation of debris of precisely the same character between the Romano-Celtic and Neolithic layers, six feet in thickness, was formed in about thrice the time, or 3,600 years. If this rough estimate be accepted, and it is probably true approximately, the Neolithic occupation of the cave must date back to between 4,000 and 5,000 years ago. There is no clue to the relative antiquity of the group of remains found in the clay; but it may safely be stated to be far greater than that of the Neolithic stratum. Throughout Europe the break between the Pleistocene age represented in the cave by the bones in the clay and the Prehistoric age - the Neolithic of the cave—is so great and so full of difficulty that it cannot be gauged by any method which has hitherto been invented. Mr. Boyd Dawkins also exhibited a remarkably perfect javelin head of bronze which had been dug up in a field near Settle.—"Species viewed Mathematically," by Mr. T. S. Aldis. We have learnt that all energy is really one, whether seen in heat, constrained position, or motion. Many also believe that life is really one, whether seen in man or a toadstool. But for our part we have often felt a difficulty. Why, if all life be one, do we not see it passing through every variety of form instead of being restricted to certain well-defined types? The present paper is an attempt to explain this. Let us consider what Plato might have called the αὐτοζῶον or complete type of animal. It consists of a certain definite number of organs, composed of a certain definite number of parts. It will also have certain aliments, location, enemies, &c., which we may call its province, necessary for its life. Thus our type animal is capable of a flux passing through all possible forms and provinces in all possible combinations. I include amongst these, of course, many arrangements necessarily absurd. To each arrangement of organs and provinces thus imagined would correspond a certain vitality or power of living in the type. I mean not merely power of individual existence, but existence as a race. The vitality is therefore a function of a large number of variables, some independent, others connected by equations of condition. It is to us quite an unknown function, but not therefore indefinite. Therefore, as in any other function of variables, certain relations amongst the variables will give maxima values of the vitality. These maxima of vitality constitute species. Vitality is not mere physical might or agility or fecundity, but compounded of all. Now for a maximum, we know that any change in the variables lessens the function. We thus see how species are stable. In the constant variation, for no being seems capable of reproducing itself exactly, all individuals have less vitality as they depart from the special type which gives the maximum of vitality, and will be choked out by those which, being nearer to the type, possess more vitality. So hybrids, intermediate between two maxima, will possess less vitality than either, and will be choked out, though the main cause of failure is that the process is like that devised by Swift's Laputan philosopher, who sawed the Whigs' and Tories' heads in half, and changing them, left each brain to settle its politics in itself. So the poor mule, with a bundle of habits, half horse and half ass, in this intestine conflict, has little power to take care of itself. Of course all maxima may not have plants or animals representing them. If there be several maxima suited for nearly the same province, the maximum of greatest intensity will choke out the others. So, too, there are probably many maxima now unoccupied, as, for instance, the thistle represented a maximum of vegetable life in South America, but till man imported the thistle to fill it up, other maxima of less intensity held the ground. In some cases possibly several maxima are closely related, and differ little in their intensity, so that slightly differing species exist together, and may in their variation pass one into the other, as perhaps in brambles and some species of St. John's wort, &c. If then the province of a species, *i.e.*, the physical geography of a country, alter, and its enemies and food with them, clearly the maximum will shift and the species change. But this is not the evolution of new species, though to a person who only notes geological evidence it appears so. For, just as in a storm the lightning shows the trees still, though really waving to and fro, so the different species in geology are probably but steps in a constant change. Such a change of course must be slow for life to follow it, for a species consists quite as much in a bundle of acquired and transmitted habits as in a certain formation of organs, and the change in habit will probably be far slower than the change in form. How then do new species arise? For we see that, if the species be a maximum of vitality, in a multitudinous progeny those nearest the type will choke out the others, and the species will be stable. Varieties will be connected with maxima of vitality in two ways. Firstly, slight differences in the province will slightly shift the maximum. Thus mountain sheep would be more agile than lowland sheep. Secondly, in such a way as this. Suppose this table a low mound, narrow though long. Then the height at any point will be a function of the distances from the north and east walls of the room. There will be one point of maximum height, but whilst a change north or south produces a great change in the altitude, one east or west will produce but little. So there will be variations in some characteristics which will produce little alteration in the whole vitality. Thus, amongst wild oxen probably no varieties without horns would exist, for they affect the vitality. Amongst protected races they do not, and so hornless varieties arise. Still these varieties are but varieties, and are not steps towards a new maximum which a gulf of lesser vitality still separates them from. Or let us consider the varieties that we try to make by select breeding. These are least of all likely to produce new species. We simply by main force depress vitality in removing individuals as far as we can from the normal type, and when the vitality is sufficiently depressed we can go no further. As for altering the province, the independent variables, so to speak, we know so little how to do it, and certainly could not do it gradually enough, that we have no chance in this way of effecting anything. How then can new species arise? Apparently in some such way as this, by what we may call the bifurcation of a maximum. If we drew a horizontal line along which the variation of the organs of an animal were expressed and the corresponding vitality were drawn by ordinates, we should get a curve we might call the vitality curve, whose maxima values would be species. As time elapses and the conditions of the earth, &c., alter, the constants, so to speak, of the curve alter, and we get our curve to vary and the maxima shift; and as the curve alters, one maximum may separate into two or more others, and thus in the lapse of time, one species may separate into two or more others. Roughly to illustrate it, suppose some species developed free from the influence of carnivora, and that, owing to various causes, size little affects its vitality, it may vary all through, from little and swift to big and heavy. Now, introducing carnivora, we can see how a bifurcation of our maximum would take place. The very light and swift would preserve themselves by their agility, the strong and heavy by their strength, whilst the intermediate would be killed out, and thus two distinct species would arise, which might in course of time by further variation separate still further apart. Doubtless, however, this bifurcation goes back to very remote times. Carnivores and herbivores probably separated not as mammals but as reptiles, or even long before, whilst ruminants and non-ruminants may have separated since they became mammals. Thus Australia seems to have possessed at one time only one marsupial, which has bifurcated into various marsupials, but not into any of another kind. The older the species grow, the deeper is the gulf between them, and, like a river, we have to ascend nearly to the source before we can make a passage from one bank to the other. To recapitulate—Maxima of vitality are species. Any alteration from the normal type produces less vitality, hence the normal type is stable. A slow change of physical geography, &c., slowly changes these maxima, and the species change with them, extinct species being generally glimpses of steps in this change. New species will generally arise from the bifurcation of maxima under circumstances over which man can exercise little control, and which, if he could, he would very likely alter so as either hardly to affect the maximum at all, or too rapidly for the species to shift with it. Selected breeding produces types of less vitality, and therefore will hardly produce new species. Thus the present stability of species is no argument against the doctrine of evolution.

GLASGOW

Geological Society, November 30.—Dr. Robert Brown, F.R.G.S., delivered a lecture on "Greenland: Its Physical Geology and Fossil Flora." After alluding to the interest which Greenland possessed, as presenting a picture of what the British Isles were supposed to have been during the glacial period, Dr. Brown gave a graphic sketch of the coast scenery of the country, which he compared to a succession of islands with water on the one side and ice on the other. He described the interior of Greenland as one vast sheet of ice of great thickness, pressing out on all sides to the sea, and occupying as separate glaciers the fiords which indent the coast. These glaciers in many instances push their way out to sea, where portions are broken off and drift away as icebergs; in other cases, the glacier dissolves near the head of the fiord, and great stores of muddy water escaping from it form a deposit of fine clay, which has sometimes silted up part of the fiord so effectually as even to turn the glacier aside into another channel. From what he had observed in Greenland, he was inclined to hold that the lower *till*, or boulder-clay, as it exists in the Forth and Clyde valley, was formed by such a sheet of massive land ice slowly moving over the country, while what he had described as resulting from the waste of the glaciers near the sea might account for some of the well-known beds of laminated clay associated with that deposit. He questioned whether icebergs really did much in the way of conveying rocks or *débris* to any distance. So far as he had observed they bore wonderfully little of such material in or upon them; and he thought that to call in their agency, as had sometimes been done, to account for the dispersion of plants, &c., was highly visionary. Dr. Brown then alluded to the rock-formations of Greenland, and to the plant remains of the Carboniferous and middle Tertiary periods which had been found in the country, showing that it once enjoyed a very different climate from that to which it is now subjected. The Carboniferous plants had only been recently discovered by Dr. Pfaff, and he trusted that gentleman, who was resident on the spot, would be enabled to make further researches.

PARIS

Academy of Sciences, December 4.—M. Chasles presented a number of theorems relating to the harmonic axes of geometrical curves, and M. C. Jordan a paper on Gauss's sums with several variables. — M. Tresca read a paper on the effects produced during the planing of metals; and M. H. Resal communicated some investigations on the calculation of the fly-wheels of steam-engines.—Letters were read from Father Secchi on a new method of measuring the heights of the solar protuberances, and on the temperature of the sun. Upon the latter M. Faye made some remarks.—M. Le Verrier presented a note on the shooting stars of the month of November, from observations made in France and Italy. Many meteors issued from the constellation Leo, but the point of radiation was slightly displaced. Five or six currents of meteors in different directions were observed. In August a displacement of the point of radiation was observed between the 9th and 11th.—An extract from a letter from M. J. F. J. Schmidt to M. Delaunay on the November meteors observed at Athens was also read.—M. C. Saint-Claire Deville communicated a note on the early cold weather of 1871, which appears to have prevailed over the whole of France.—M. F. de Biseau recorded the observation of aurora borealis in Belgium on the nights of the 9th and 10th November.—A note from M. de Magnac on the determination by means of chronometers of the differences of longitude of distant places was read.—M. Lecoq de Boisbaudran presented a note on the separation and quantitative determination of some metals by means of a voltaic current.—M. A. Béchamp communicated some observations on a recent note by M. Ritter on the formation of urea by albuminoid materials and permanganate of potash.—M. Wurtz presented a note by M. L. C. de Coppet on a new method of preparing supersaturated saline solutions, in which the author stated that solutions identical with those called supersaturated could be prepared by dissolving certain dehydrated salts (sulphate and carbonate of soda) in cold water.—M. Peligot presented a note by M. T. Schlœsing, containing a comparison of the two conditions of a soil in part wooded and in part cleared and treated with lime.—M. Peligot also presented a note by M. A. Renard on the determination of ground-nut oil in olive oil. The process, which is rather complicated, consists in the saponification of the oil, and the separation from the soap of the arachidic acid which is characteristic of ground-nut oil.—M. Balard communicated a note by MM. Scheurer-Kestner and C. Meunier on the composition and heat of combustion of lignites, containing the analyses and results of combustion of six lignites from various parts of France, and from Bohemia. The heat of combustion was always found to be inferior to that of the carbon and hydrogen contained in the lignites.—M. Elie de Beaumont exhibited a collection of minerals from Bolivia, Chili, and Peru sent by M. Domeyko.—M. S. Meunier presented a note on a new method of obtaining Widmannstätten's figures by attaching a polished plate of meteoric iron to the positive pole of a Bunsen's battery and a plate of silver to the opposite pole, and plunging both into a solution of bisulphate of potash.—M. Husson communicated an analysis of the milk of cows attacked by contagious typhus.—A note was read on the Garumnian type of the department of the Aude, by M. A. Leymerie, in which the author maintains the distinctness of this geological stage, and indicates some of the fossils which characterise it.

BOOKS RECEIVED

ENGLISH.—Marvels of Pond Life: H. J. Slack (Groombridge and Sons).—The Amateur's Flower Garden: Shirley Hibberd (Groombridge and Sons)—Flowers for Sundays: P. Spenser (Longmans).—The Laws of the Wind prevailing in Western Europe; No. 1, with Charts and Diagrams; W. C. Ley (E. Stanford).

FOREIGN.—(Through Williams and Norgate.)—Die Axendrehung der Welt-körper: E. F. T. Moldenhauer.

DIARY

THURSDAY, DECEMBER 14.

ROYAL SOCIETY, at 8.30.—Contributions to the History of Orcin. No. II. Chlorine and Bromine Substitution Compounds of the Orcins; Note on Fuedsol: Dr. Stenhouse, F.R.S.—On some recent Discoveries in Solar Physics: and on a Law regulating the Duration of the Sunspot Period: W. De La Rue, F.R.S., B. Stewart, F.R.S., and B. Loewy.

MATHEMATICAL SOCIETY, at 8.—On the Celebrated Theorem that every Arithmetical Progression, if it contains more than one must contain an Infinite number of Prime Numbers: J. J. Sylvester, F.R.S.

FRIDAY, DECEMBER 15.

LONDON INSTITUTION, at 4. Elementary Physiology, by Prof. Huxley, F.R.S. No. 7. (Extra Lecture.)

SUNDAY, DECEMBER 17.

SUNDAY LECTURE SOCIETY, at 4.—On the Physiology of Contagion and Infection: Dr. John S. Bristowe.

MONDAY, DECEMBER 13.

ANTHROPOLOGICAL INSTITUTE, at 8.—The Anthropology of Auguste Comte: Joseph Kaines.—On the Hereditary Transmission of Endowments: George Harris.

LONDON INSTITUTION, at 4. No. 8.

TUESDAY, DECEMBER 19.

STATISTICAL SOCIETY, at 7.45.—On the Comparative Health of Seamen and Soldiers: Dr. Balfour.

WEDNESDAY, DECEMBER 20.

GEOLOGICAL SOCIETY, at 8.—Further Remarks on the Relationship of the Limulidæ to the Eurypteridæ and to the Trilobita: Henry Woodward, F.G.S.—Further Notes on the Geology of the neighbourhood of Malaga: M. D. M. d'Orueta.

ROYAL SOCIETY OF LITERATURE.—On a capital Joke recorded by Suetonius: Dr. C. Mansfield Ingleby.—On a Collection of Roman Brick Stamps in the Ashmolean Museum at Oxford: Mr. Vaux.

SOCIETY OF ARTS, at 8.—On the Study of Economic Botany, and its Claims Educationally and Commercially Considered: James Collins.

THURSDAY, DECEMBER 21.

ROYAL SOCIETY, at 8.30.

LINNEAN SOCIETY, at 8.—On the Anatomy of the American King-Crab (*Limulus polyphemus*, Latr.): Prof. Owen, F.R.S.

CHEMICAL SOCIETY, at 8.

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THURSDAY, DECEMBER 21, 1871

THE COPLEY MEDALIST OF 1870

THIRTY years ago Electro-magnetism was looked to as a motive power which might possibly compete with steam. In centres of industry, such as Manchester, attempts to investigate and apply this power were numerous, as shown by the scientific literature of the time. Among others Mr. James Prescot Joule, a resident of Manchester, took up the subject, and in a series of papers published in Sturgeon's "Annals of Electricity" between 1839 and 1841, described various attempts at the construction and perfection of electro-magnetic engines. The spirit in which Mr. Joule pursued these inquiries is revealed in the following extract: "I am particularly anxious," he says, "to communicate any new arrangement in order, if possible, to forestal the monopolising designs of those who seem to regard this most interesting subject merely in the light of pecuniary speculation" He was naturally led to investigate the laws of electro-magnetic attractions, and in 1840 he announced the important principle that the attractive force exerted by two electro-magnets, or by an electro-magnet and a mass of annealed iron, is directly proportional to the square of the strength of the magnetising current; while the attraction exerted between an electro-magnet and the pole of a permanent steel magnet varies simply as the strength of the current. These investigations were conducted independently of, though a little subsequently to, the celebrated inquiries of Henry, Jacobi, and Lenz and Jacobi on the same subject.

On the 17th of December, 1840, Mr. Joule communicated to the Royal Society a paper on the production of heat by Voltaic electricity; in which he announced the law that the calorific effects of equal quantities of transmitted electricity are proportional to the resistance overcome by the current, whatever may be the length, thickness, shape, or character of the metal which closes the circuit; and also proportional to the square of the quantity of transmitted electricity. This is a law of primary importance. In another paper, presented to but declined by the Royal Society, he confirmed this law by new experiments, and materially extended it. He also executed experiments on the heat consequent on the passage of Voltaic electricity through electrolytes, and found in all cases that the heat evolved by the proper action of any Voltaic current is proportional to the square of the intensity of that current multiplied by the resistance to conduction which it experiences. From this law he deduced a number of conclusions of the highest importance to electro-chemistry.

It was during these inquiries, which are marked throughout by rare sagacity and originality, that the great idea of establishing quantitative relations between Mechanical Energy and Heat arose and assumed definite form in his mind. In 1843 Mr. Joule read before the meeting of the British Association at Cork a paper "On the Calorific Effects of Magneto-Electricity and on the Mechanical Value of Heat." Even at the present day this memoir is tough reading, and at the time it was written it must have appeared hopelessly entangled. This I should think was the reason why Prof. Faraday advised Mr. Joule not to submit the paper to the Royal Society. But its drift and results are summed up in these memorable words by its author, written some time subsequently: "In that paper it was demonstrated experimentally that the mechanical power exerted in turning a magneto electric machine is converted into the heat evolved by the passage of the currents of induction through its coils, and on the other hand, that the motive power of the electro-magnetic engine is obtained at the expense of the heat due to the chemical reaction of the battery by which it is worked."* It is needless to dwell upon the weight and importance of this statement.

Considering the imperfections incidental to a first determination, it is not surprising that the "mechanical values of heat," deduced from the different series of experiments published in 1843, varied somewhat widely from each other. The lowest limit was 587, and the highest 1,026 foot-pounds for 1° F. of temperature.

One noteworthy result of his inquiries, which was pointed out at the time by Mr. Joule, had reference to the exceedingly small fraction of the heat which is actually converted into useful effect in the steam-engine. The thoughts of the celebrated Julius Robert Mayer, who was then engaged in Germany upon the same question, had moved independently in the same groove; but to his labours due reference will doubtless be made on a future occasion. In the memoir now referred to Mr. Joule also announced that he had proved heat to be evolved during the passage of water through narrow tubes; and he deduced from these experiments an equivalent of 770 foot-pounds, a figure remarkably near to the one now accepted. A detached statement regarding the origin and convertibility of animal heat strikingly illustrates the penetration of Mr. Joule and his mastery of principles at the period now referred to. A friend had mentioned to him Haller's hypothesis, that animal heat might arise from the friction of the blood in the veins and arteries. "It is unquestionable," writes Mr. Joule, "that heat is produced by such friction, but it must be understood that the mechanical force expended in the friction is a part of the force of affinity which causes the venous blood to unite with oxygen, so that the whole heat of the system must still be referred to the chemical changes. But if the animal were engaged in turning a piece of machinery, or in ascending a mountain, I apprehend that in proportion to the muscular effort put forth for the purpose, a *diminution* of the heat evolved in the system by a given chemical action would be experienced." The italics in this memorable passage, written it is to be remembered in 1843, are Mr. Joule's own.

The concluding paragraph of this British Association paper equally illustrates his insight and precision regarding the nature of chemical and latent heat. "I had," he writes, "endeavoured to prove that when two atoms combine together, the heat evolved is exactly that which would have been evolved by the electrical current due to the chemical action taking place, and is therefore proportional to the intensity of the chemical force causing the atoms to combine. I now venture to state more explicitly, that it is not precisely the attraction of affinity, but rather the

* Phil. Mag. May 1845.

mechanical force expended by the atoms in falling towards one another, which determines the intensity of the current, and, consequently, the quantity of heat evolved; so that we have a simple hypothesis by which we may explain why heat is evolved so freely in the combination of gases, and by which indeed we may account 'latent heat' as a mechanical power prepared for action as a watch-spring is when wound up. Suppose, for the sake of illustration, that 8 lbs. of oxygen and 1 lb. of hydrogen were presented to one another in the gaseous state, and then exploded; the heat evolved would be about 1° F. in 60,000 lbs. of water, indicating a mechanical force expended in the combination equal to a weight of about 50,000,000 lbs. raised to the height of one foot. Now if the oxygen and hydrogen could be presented to each other in a liquid state, the heat of combination would be less than before, because the atoms in combining would fall through less space." No words of mine are needed to point out the commanding grasp of molecular physics, in their relation to the mechanical theory of heat, implied by this statement.

Perfectly assured of the importance of the principle which his experiments aimed at establishing, Mr. Joule did not rest content with results presenting such discrepancies as those above referred to. He resorted in 1844 to entirely new methods, and made elaborate experiments on the thermal changes produced in air during its expansion: firstly, against a pressure, and therefore performing work; secondly, against no pressure, and therefore performing no work. He thus established anew the relation between the heat consumed and the work done. From five different series of experiments he deduced five different mechanical equivalents; the agreement between them being far greater than that attained in his first experiments. The mean of them was 802 foot-pounds. From experiments with water agitated by a paddle-wheel, he deduced, in 1845, an equivalent of 890 foot-pounds. In 1847 he again operated upon water and sperm-oil, agitated them by a paddle-wheel, determined their elevation of temperature, and the mechanical power which produced it. From the one he derived an equivalent of 781·5 foot-pounds; from the other an equivalent of 782·1 foot-pounds. The mean of these two very close determinations is 781·8 foot-pounds.

At this time the labours of the previous ten years had made Mr. Joule completely master of the conditions essential to accuracy and success. Bringing his ripened experience to bear upon the subject, he executed in 1849 a series of 40 experiments on the friction of water, 50 experiments on the friction of mercury, and 20 experiments on the friction of plates of cast-iron. He deduced from these experiments our present mechanical equivalent of heat, justly recognised all over the world as "Joule's equivalent."

There are labours so great and so pregnant in consequences, that they are most highly praised when they are most simply stated. Such are the labours of Mr. Joule. They constitute the experimental foundation of a principle of incalculable moment, not only to the practice, but still more to the philosophy of Science. Since the days of Newton, nothing more important than the theory of which Mr. Joule is the experimental demonstrator has been enunciated.

I have omitted all reference to the numerous minor papers with which Mr. Joule has enriched scientific literature. Nor have I alluded to the important investigations which he has conducted jointly with Sir William Thomson. But sufficient, I think, has been here said to show that, in conferring upon Mr. Joule the highest honour of the Royal Society, the Council paid to genius not only a well-won tribute, but one which had been fairly earned twenty years previously.*

Comparing this brief history with that of the Copley Medalist of 1871, the differentiating influence of "environment" on two minds of similar natural cast and endowment comes up in an instructive manner. Withdrawn from mechanical appliances, Mayer fell back upon reflection, selecting with marvellous sagacity from existing physical data the single result on which could be founded a calculation of the mechanical equivalent of heat. In the midst of mechanical appliances, Joule resorted to experiment, and laid the broad and firm foundation which has secured for the mechanical theory the acceptance it now enjoys. A great portion of Joule's time was occupied in actual manipulation; freed from this, Mayer had time to follow the theory into its most abstruse and impressive applications. With their places reversed, however, Joule might have become Mayer, and Mayer might have become Joule.

JOHN TYNDALL

THE BROWN INSTITUTION

IN 1852 a large sum of money was bequeathed by the late Mr. Thomas Brown to the University of London for the purpose of "founding and upholding" an Institution for "investigating, studying, and if possible endeavouring to cure" the diseases and injuries of animals useful to man. The sum was to be allowed to accumulate for a limited period, at the end of which the principal and interest were to be applied in the manner directed. And it was provided that in case the University should fail to carry out the trust imposed upon it within nineteen years after the testator's death, the whole sum with the accumulations should be transferred to the University of Dublin, to be applied for the endowment of certain philological professorships. The will contains various directions for the administration of the proposed Institution. The most important are those which relate to the appointment of a Committee of Management and of a Professor. The committee must be appointed by the governing body of the University, and must consist either of members of the Senate or of other persons, members of the medical profession. As regards the qualifications of the professor nothing is said. He must be appointed by the University, must give a course of lectures annually, and must have a residence adjacent to the Institution.

The nineteen years have now almost expired. In pursuance of the testator's directions, the "Brown Institution" has just been opened. Last summer a large plot of freehold land was acquired by the University in the Wandsworth Road, close to the goods station of the South-

* Had I found it in time, this notice should have preceded that of the Copley Medalist of 1871.

Western Railway. On this ground a Hospital for Animals has been built, consisting of stables for the reception of the larger quadrupeds, and of houses of various descriptions for those of a smaller size. All of these buildings are constructed in the best style, with a view to the well-being of the creatures they are destined to contain, being thoroughly drained, paved, and ventilated, and warmed with hot-water pipes. Adjoining them there is a spacious exercise ground.

As many of our readers already know, the Senate have placed the Institution under the management of Dr. Burdon Sanderson, of University College, London, who, as Professor, will, in future, deliver the annual course of lectures.

If the scope and purpose of the Brown Institution were limited to the care and cure of diseased animals, its establishment would scarcely be worthy of record in the pages of NATURE, for, however desirable it may be that the animals that serve us should be kindly and skilfully treated when they are sick, the object has so remote a relation to the promotion of physical science that our readers could not be expected to take any special interest in it. But, happily alike for humanity and for science, the late Mr. Brown showed by his selection of persons to be entrusted with the carrying out of his intentions, by the instructions contained in his will for their guidance, and by the terms in which he defined the purposes of the proposed Institution — placing study and investigation first, cure afterwards—that he was not actuated by a mere sentimental sympathy for the lower animals as such, but that he desired, by promoting the scientific study of their diseases, to benefit mankind.

As might have been expected, the Senate of the University of London have not only fulfilled the letter of the testator's dispositions, but have proved by the manner in which they have done so, that they are actuated by the same noble purpose. They have shown this first of all in their selection of a Committee of Directors. What could be a better guarantee for the future good administration of the Institution than the fact that among its directors are to be found such men as Busk, Carpenter, Gull, Paget, Quain, Sharpey, Sibson, and Simon, men eminent as physiologists, pathologists, or clinical teachers ; of each of whom it may be said that he has contributed a large proportion to the total amount of work done in his own branch of science in England during the past thirty years. We do not think that it would have been possible, even if their choice had been perfectly unlimited, to have selected persons more fitted for the purpose, whether as regards personal character or scientific attainments.

Under the direction of Dr. Sanderson, a laboratory intended, to quote the terms of the will, " for the study and investigation of disease," has been built on the ground already referred to at Vauxhall, adjoining the hospital for animals. The laboratory consists of four admirably-lighted and spacious working rooms, connected by a corridor. Underneath these are four other rooms, which, although not so lofty, are also well adapted for many kinds of research. In the same building is included a stable for the reception of animals intended to be the subjects of special observation.

In the work of the laboratory the Committee of Direction have most wisely associated with Dr. Sanderson, under the title of Assistant Professor, Dr. E. Klein, whose name is well known as the contributor of valuable articles to Stricker's " Histology," and of several important embryological researches. Well trained as a pupil of Brücke and Stricker in the methods of research, whether physical, chemical, or microscopical, young in years though old in accomplished work, Dr. Klein is singularly fitted for the post. Dr. Sanderson is much to be congratulated in having so able a coadjutor.

It may not be out of place if we attempt to give our readers an idea of the work which we suppose will be done or attempted in the laboratory of the Brown Institution.

The facts on which the science of disease, so far as it may as yet be called a science, is founded, are gathered from two sources, the bedside and the laboratory. In clinical studies the same, or even greater, exactitude is required as in those of the physicist or chemist ; but even when they are conducted in the wards of a hospital, the Harveian method of " searching out the secrets of nature by way of experiment," can only be applied under limitations which very materially embarrass the inquiry. The pathologist at the bedside is not in the position of an experimenter, but only in that of a student, who stands by at a greater or less distance, while another, over whom he has no control, performs experiments in his presence, without deigning to explain to him their nature or purpose. The true physician fears to meddle with the processes of which he is the attentive and anxious spectator. Although the more ignorant members of the medical craft—the so-called " practical " men—may sometimes, with the best intentions, experiment on their patients with harmful drugs, such experimentation is repudiated by the man of science.

There are, however, many questions relating to disease, of the most profound importance to the human race, which cannot be solved, and never will be solved, by thus, as it were, standing on one side and watching what goes on at a distance ; such questions, for example, as the nature of contagion, and those which relate to the origin and proximate causes of our most common diseases, such as inflammation, fever, and tubercle. The knowledge which has been acquired on these subjects during the last few years has been gained by work done in laboratories. The advantages of this mode of inquiry, as compared with the indirect clinical method, are of two kinds—the one relating to the objects of observation, the other to the means which are at the disposal of the inquirer. In dealing with animals, he is embarrassed by scarcely any of the limitations which render clinical observation so difficult. The very considerations, indeed, which in the case of man, absolutely forbid his entertaining any other purpose excepting that of prolonging life and alleviating pain, not only allow, but encourage him, in the case of animals, to disregard altogether the present suffering for the future benefit. We are clearly justified in profiting by the sufferings of the lower animals for man's sake. We may subject them experimentally to the action of remedies without any immediate view to their being thereby benefited. We may place them under conditions which we know will produce disease, for the purpose of studying the mode of action of those conditions. We have at least as good a right to kill sick animals for the purpose of investigating the anatomical changes produced by disease, as to slaughter healthy animals for food. And even if in the pursuit of our inquiries we are compelled to inflict pain, we are perfectly right in doing so—provided that truths valuable to humanity are to be learnt by it.

The other respect in which the comparative pathologist has an advantage over the clinicist, lies in the choice of means. It is true that during the last few years much progress has been made in the application of instruments of precision to the investigation even of human diseases; but, after all, there are few of those instruments which are really valuable. In the case of animals it is entirely different. The microscope may be applied to the investigation of tissues unaltered by those changes which speedily follow the extinction of life. The measurement of the temperature of the body, whether with relation to the changes which it undergoes in disease, or to the differences between diseased and healthy parts, can be performed in animals with all the exactitude which such investigations require—in man such exactitude is impossible, because the conditions of observation cannot be controlled. Instruments of precision may be used for the investigation of the changes which disease produces in the mechanical functions of respiration and circulation, which, for reasons already adverted to, could not be applied in the sick room, or in the wards of a hospital—and if they were applied, would yield no satisfactory results.

Again, in animals it is possible to apply the ordinary methods of chemistry to investigate the modifications produced by disease in the process of nutrition; whereas in man this is attended with such insuperable difficulties, that it may be regarded as impossible.

Many other similar examples might be mentioned; but these may serve to explain the way in which we hope to see the new laboratory at Vauxhall brought into relation with the hospital for sick animals. Believing that the study of pathology, like that of physiology, of which it forms part, can only be successfully prosecuted by observing the operation of chemical and physical laws in the living diseased body, and applying the same methods as are used by the chemist and physicist to their investigation, and that the more this principle is acted on, the more rapid and solid will be the progress made, we regard the establishment of the Brown Institution as an important step in the right direction. We should have been still better pleased if it had been a laboratory of physiology, for this ought to have preceded the other. We think it, however, not unlikely that it may, by setting an example of good work, exercise a considerable indirect influence in the promotion of physiological studies in this country.

We must not omit to mention that although the laboratory is intended for research rather than for instruction, it will be open to those who may wish to engage on their own account in scientific inquiries. The only condition imposed by the directors on those who desire admission to the laboratory as workers, is the possession of "previous scientific training." Each worker will have to defray the expenses of material, but no other payment will be required of him. It is understood that the laboratory will be opened on the 1st of January, 1872.

FOREIGN YEAR-BOOKS

Jahrbuch der Erfindungen. Herausgegeben von H. Hirzel und H. Gretschel. Sechster Jahrgang. (Leipzig: Quandt und Händel; London: Williams and Norgate, 1870; pp. 472.)

THE sixth volume of this series fully sustains the high character achieved by its predecessors. Astronomy, physics and meteorology, mechanics and mechanical technology, and chemistry and chemical technology form the subjects of the respective chapters.

We cannot open any part of the work without observing the care with which it is edited. We shall select for special notice the latter part of the chapter on chemistry, which treats of organic compounds, beginning with the following paragraph upon the products of oxidation of paraffin. After describing the recent improvements introduced by Hübner in the preparation of this substance from coal-tar, and in its mode of purification, and noticing its remarkable stability (it being unaffected by concentrated hydrochloric or sulphuric acids, and by the alkalies), the reporters state that there are certain oxidising agents, and especially chromic and nitric acids, which it is unable to resist. Gill and Meusel have studied the action of these reagents on paraffin, and have arrived at the following results:—

"The paraffin in common use fuses at 56° C., and by repeated crystallisation from sulphide of carbon the fusing point may be raised to 60° and upwards. If we boil from 300 to 500 grammes of pure paraffin with 120 grs. of bichromate of potash, and 180 grs. with sulphuric acid diluted with twice its volume of water for three or four days in a glass retort till the chromic acid is completely reduced to chrome-oxide, acetic acid and other acids of the same series, and principally cerotic acid, are formed; the latter being a white solid substance that does not fuse at a lower point than 78° C., and also occurs as a main constituent of bees'-wax. If we boil paraffin continuously with five or six times its volume of nitric acid of 1·3 sp. gr., which has been previously diluted with 1½ times its volume of water, we likewise obtain cerotic acid, in addition to acetic, butyric, valerianic, and succinic acids, and other products" (p. 261).

Passing over a section on "Fats, fatty oils, and allied substances, and the products of their decomposition," in which is a notice of the explosive compounds derived from glycerine, we come to one treating of "Resins," in which there is a notice of Puscher's interesting and highly-practical communication on shellac-ammonia solutions. Perhaps the most valuable of the applications of these solutions is their property of dissolving certain of the aniline colours, as aniline green, aniline yellow, and fuchsine.

The organic non-nitrogenous acids, the carbo-hydrates, alcohol and its products, the albuminous bodies and their allies, newly-discovered organic bases, pigments and pigment-yielding bodies, both natural and artificial, nutritious matters, and disinfectants, are all duly considered. The report on artificial pigments is especially deserving of commendation. It consists of nearly fifty pages full of practical matter, and, taken in conjunction with a previous report that appeared in the second volume (for 1866), forms the most complete summary of this important department of practical chemistry, that, taking its limits into consideration, we are acquainted with.

As usual the volume concludes with a necrology for the past year.

OUR BOOK SHELF

Marvels of Pond-Life: or a Year's Microscopic Recreations among the Polyps, Infusoria, Rotifers, Water-Bears, and Polyzoa. By Henry J. Slack, F.G.S., &c. Second Edition. (London: Groombridge and Sons.)

THIS little volume is already so well and favourably known to microscopists that any formal notice or commendation is scarcely necessary. Professing only to be a first book on "Pond-Life," it does not attempt more than to guide the young student in searching after, collecting, and examining the various animal organisms which inhabit fresh water. The division into months indicates that it is also popular rather than abstruse, and the number of species mentioned or figured is very limited. There appears to be no good reason why the present edition should not have made an advance beyond its predecessor, and given us an additional chapter or two on the construction and management of small aquaria at home, adapted especially and entirely to minute pond-life, by means of which metropolitan students might continue the study when unable to go to the ponds; and also on those artificial ponds for the evolution of Infusoria, so much alluded to of late, infusions of organic substances. Keeping in view the simple pretensions and elementary character of this volume, it fully answers the design of its author, and we are glad to announce the appearance of a second edition.

Physikalisches Repetitorium, &c., &c. Von Dr. Ferdinand Bothe. Second Edition, revised and enlarged. (Brunswick: Vieweg, 1871.)

A BRIEF enumeration of the more prominent facts and formulæ of physics; carefully divided into subjects, and with occasional dates and names of inventors or discoverers. We conceive that to make an excellent work of this kind (if such a thing be at all desirable), all that is necessary, is to take a really good treatise on natural philosophy and construct something between an Index to, and an Abstract of, its contents. It seems probable that some such process has been employed by Dr. Bothe; but either he cannot have used a trustworthy book for analysis, or his analysis is not a faithful one. In fact, if we look on it seriously, a more painful volume we have not often met with; nor a more amusing one, if we could fancy its blunders intended to amuse. We simply open its pages at hazard, and make a few pickings:—

"64. The density and resilience (*Spannkraft*) increase in proportion to the pressure, the volume is inversely as the pressure, and *vice versâ*—Boyle's or Mariotte's Law, 1679." James Bernoulli was a contemporary of these men, and says in his work, "De Gravitate Aetheris," "Veritas utriusque hujus regulæ manifesta fit duobus curiosis experimentis ab Illustr. Dn. Boylio hanc in rem factis, quæ videfis [*sic*] in Tractatu ejus contra Linum." The date of this tract of Boyle's is 1662, and it is to be observed that Bernoulli does not mention Mariotte at all. We notice, in passing, that Young's name is not mentioned under Capillarity, and we arrive at the following curiosity:—"140. Unit of momentum or of work (*Arbeit*) is the force (*Kraft*) which can in one second communicate to unit of weight a velocity of unit of length. (Its) metrical measure is the kilogramme-mètre; in Prussia, England, &c., the foot-pound." But we beg Dr. Bothe's pardon. We had no right to render *Arbeit* by "work," which is its usual equivalent in scientific books; for looking back we find:—"129. The product of the weight of a body into its velocity is called Momentum, and also *Arbeit*"! It is scarcely possible to conceive a more hopeless jumble of essentially different things than these sentences exhibit. The Heliotrope is (468) ascribed to Gauss, 1830 (?). Did not Drummond use it in 1826? 471 gives Bunsen and Kirchhoff the credit of the spectroscope, with its collimator, &c. What of Swan? As to the equality of absorption and radiation, Angström is given without date, Stokes and Balfour Stewart not mentioned. "472. The planets and comets (!) send back only the rays which the sun has sent to them." 484. In the enumeration of the earliest attempts to produce photographic impressions, there is no mention of Wedgwood, &c. 558. No mention is made of Northmore, whose long priority in the liquefaction of gases was insisted on by Faraday. 592. The old story of Mayer and the dynamical theory of heat. His date is given as 1842; Davy and Rumford (who did all that is referred to in the text more than forty years before) are not mentioned. Joule is coupled with Clausius, and the date 1853 is assigned to them! Of Carnot, Colding, Rankine, Thomson, &c., not a word. 598-600. The experimental laws of heat of combination are very imperfectly given, and, without any mention of Andrews and Hess, handed to Thomsen and Favre and Silbermann, with the date 1853! 666. The similarity of the order of bodies considered separately as conductors of heat and electricity is given to Wiedemann and Franz in the same prolific year. Surely Forbes pointed it out twenty years earlier! So far as we have seen, Sir W. Thomson, Clerk-Maxwell, &c, are not even named in the book.

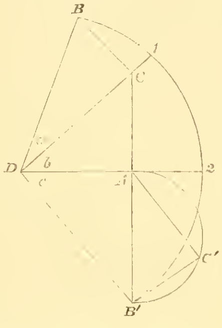
If the reader remember that these are merely the things which have caught our eye in turning over the pages at random, he will not blame us for absolutely declining to examine the work more closely. A series of working tables is appended, but without very close examination we should hesitate to trust them, after what we have seen of the character of the book. That we have noticed it at all is due to the circumstance that some consolation is to be derived from the mere fact of its existence. We are all (in consequence, perhaps, of recent events) more or less imbued with the notion that Germany (Prussia especially) is rapidly taking the lead in matters of scientific education and investigation; and no doubt there is some truth in this. But the game is not lost, we are not yet passed in the race, and our old supremacy is quite within our reach even now, provided we make speedy and sufficient exertions to regain and maintain it. It will not drop into our mouths for a mere wish; but is it reasonable to wonder at the state of science in this country, where so few statesmen pay the least attention to it, when we find that even in enlightened Prussia, such a book as the above can be written by a recognised teacher, and published in a second edition by one of the highest firms in the world?

LETTERS TO THE EDITOR

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

Proof of Napier's Rules

I AM greatly obliged to "J. J. W." for pointing out the objection of a want of generality in the construction of the figure contained in my former letter (in NATURE, No. 106), for the proof of Napier's Rules; which the more general construction now described by "J. J. W." most simply and most effectually removes. To illustrate his more perfect general construction with a figure—D is the centre, and B12B′ a part of the circumference of a circular piece of cardboard, upon which the arcs B1, 12 are taken equal to the sides of the right-angled spherical triangle which it is required to represent. If we join DB, D1, D2, and draw BC, CA perpendicular to D1, D2, the latter perpendicular prolonged meeting the circle of the circumference in B′, and join DB′; and on AB′ as diameter describe the semicircle AC′B′; and with the centre A, and radius AC, another circle, meeting the semicircle in C′, so that the straight line AC′ is equal to AC; and join B′C′. Then it is easily shown that if AC CB are the two sides, AB′ is the hypothenuse of a right-angled triangle, which, when the four triangles are closed together so as to form a solid figure, will coincide with the triangle AC′B′. As BC (or B′C′) will then be perpendicular both to CD and to CA (or C′A), it will be perpendicular to the plane DCA; and the arc B1, which is in the same plane with it, will be at right angles to the arc 12. The third arc 2B′ will therefore be the hypothenuse of a right-angled spherical triangle, of which B1, 12, are the two sides. Calling these arcs or the angles of the faces resented by them, a, b, c, and the angles opposite to them in the spherical triangle, A, B, C, the proof of Napier's Rules, with this solid figure, proceeds by the same direct steps as those already described, with a special example of the figure in my former letter. As the construction there described is confined to the representation of a particular kind of right-angled spherical triangle, and is therefore inapplicable to illustrate the proof of



Napier's Rules experimentally in every given case, the general construction supplied by "J. J. W.," which is limited by no such restrictions, and which is at least equally convenient, will evidently serve more effectively the same practically useful and instructive purpose.

Instead of "accessible," as applied to the difficulties of the geometrical proofs produced by Mr. Cooley in his letter on "Elementary Geometry" (in NATURE, No. 103), which are indeed there obviously overcome, I would have used the word "surmountable" as more descriptive of geometrical difficulties, properly treated and discussed, had the word immediately presented itself to me; but having often found an easily executed model extremely useful and convenient in practical applications of Napier's Rules, with whose design, as a general resource to facilitate their study. I was not, however, so fully satisfied, I applied, perhaps unconsciously, to Mr. Cooley's demonstrations a term expressing strictly only the diffidence with which I ventured to present to readers of NATURE my own very imperfect geometrical contrivance. In thus making my difficulties accessible to "J. J. W.," I very gratefully acknowledge the assistance which I have derived from his remarks on my letter in NATURE, No. iii., and I cheerfully admit the merit and superiority of the general rule for constructing a proper model in cardboard, to illustrate the proofs of Napier's Rules, and to facilitate their study, which he has kindly consented to describe.

Newcastle-on-Tyne, Dec. 16 — A. S. HERSCHEL

Alternation of Generations in Fungi

I AM sure that the Rev. M. J. Berkeley will exonerate me from any deliberate intention to misrepresent him; nor do I think that there is, after all, much difference of opinion between us regarding the present subject, unless, perhaps, that I am more sceptical. I alluded to the paper cited by him from the "Journal of the Horticultural Society," on propagation of bunt spores, and not to his communications on the hop or vine mildew. I was under the impression that he regarded the "four consecutive forms of reproductive cells in the bunt" as an instance of alternation of generations. On reference to the original paper, I find that he did not go so far then as to indicate four consecutive forms of reproductive cells; but that Tulasne followed on his track in 1854, and in 1857 Mr. Berkeley seemed to have accepted the results of Tulasne's observations, since, in his "Introduction," he gives figures at page 318, in the description of which the following phrases occur:—"spores of the second order," "spores of the third order," "spores of the fourth order." Here are the "four consecutive forms of reproductive cells" to which I alluded. At page 321 he writes concerning the bunt:—"The spores, however, are not immediate means of propagation; they are, in fact, only a sort of prothallus, from which the mycelium grows, producing at the tips, or on lateral branchlets, bodies of various forms, which are themselves capable of germination, and immediately reproduce the species." The real issue between us seems to lie in the phrase, "alternation of generations." If the bunt spores, on germination, produce fusiform bodies, which, after conjugation, produce short cylindrical spores, and thus intermediate reproductive cells unlike the parent cell come between that and the ultimate reproduction of the species, I am induced to call it an "alternation of generations." It would be waste of time to discuss phrases, or I might take exception to the application of this phrase to the *Erysiphei*. The conidia and pycnidia of the hop mildew may be developed without sporangial conceptacles, and the parasite reproduced without sporangial fruit, but I cannot recognise alternation of generations in the reproduction of a species by means of conidia, stylospores, or sporidia, or by one of these alone. If such may be construed into an alternation of generations, it must be by permitting greater elasticity to the phrase. Conidia germinating and producing pycnidia, the stylospores of the pycnidia germinating and producing sporangial conceptacles, containing the sporidia which, upon germination, will produce the mycelium and conidia again, returning to the original form after two or three consecutive departures from it, appears to me a perfect type of alternation of generations. I fully admit that "if it is once established that a Puccinia produces an Æcidium, or an Æcidium a Puccinia, we should have a clear case, especially when the third form reverts to the first again." Without the slightest desire to "depreciate the labours of Oersted and De Bary," I cannot admit that they have established facts until their observations are confirmed, especially when there is an evident possibility of their having been deceived. I shall have no hesitation in accepting the facts when they are confirmed by independent and equally trustworthy observers, although I may be unable to account for some of the phenomena. At present I must confess that I am not so sanguine as Mr. Berkeley appears to be.

The correspondent signing himself "Mycelium" wishes to know if "the liability to produce parasitic fungi is communicated from the seed to the mature plant." In some instances we know such to be the case, in others perhaps only suspect it. The "bunt" is an instance, or why the steeping of seed corn? or how did the Rev. M. J. Berkeley succeed in producing bunted wheat plants from seed corn inoculated with bunt spores? Two or three years since I published particulars of a similar instance of celery seed and *Puccinia Apii*. It would be as rash to affirm that this is always the case as to deny its occurring at all.

M. C. COOKE

In Re Fungi

THE letters in your last two numbers have reminded me how ill this subject is studied by some botanists in this country. I will give two recent instances: 1. In the last number of the *Journal of Botany*, p. 383, it is positively stated that *Agaricus cartilagineus* (a rare and very critical species by the way) was determined by a growth which is there described a mere *mass of mycelium*. He must have been a bold man who ventured to name an agaric (above all things) from a *mass of mycelium*. 2. In the first number, October 1871, of the *new edition* of "Paxton's Botanical Dictionary"—"enlarged and revised"—under the article *Agaricus* there is to be found such a collection of obsolete names and absurd errors as to make the article simply ridiculous.

W. G. S.

Mr. Lowne and Darwinian Difficulties

MR. LOWNE (NATURE, December 7) sees no difficulty whatever in explaining by what natural process an insect with a suctorial mouth is developed from one having the mandibular type of mouth, but still he does not explain. He affirms there is no doubt that "the pupa state is a modification (!) of the ordinary process of skin shedding," and that this is "proved" by so many facts that he cannot understand how it could be "denied," &c., but he does not prove it.

For aught I can tell, every internal tissue and every external scale of the butterfly may be represented in the larva; but I do not know and cannot prove that this is so, nor do I believe any one can prove it. That the changes which take place during the pupa state are very different from those that occur during any portion of the larva period, will be admitted by every one who has kept silkworms or bred butterflies. The assertion that there is absolutely only a difference in the time at which the successive skins are formed in this and in ordinary ecdysis, is but assertion on the part of Mr. Lowne. Indeed, controversy becomes profitless if authority is to be substituted for fact, and an attempt made to silence opponents and stop inquiry by such positive assertions as the above and the following:—"The imaginal skin is likewise derived from cells laid down in contact with the imaginal discs." If Mr. Lowne will be so good as to explain what no books tell me, and I fail to make out myself, I will study what he says with great attention, and thank him heartily. He knows me well enough to feel assured that I would do so; but it is useless, and he must permit me to say that it is not in good taste, for him to comment about the "return of darkness," and to use expressions more positive and arbitrary than are called for.

Let us, if we can, get at the facts concerning some of these marvellous changes. For this there is nothing like discussion, carried on with care and consideration, even for an opponent; and though the fittest may be certain that he will survive, don't let any one be in too great haste to proclaim himself either survivor or fittest, or call himself strong and others weak, as has been done once already by one distinguished evolutionist. Evolution is a much quieter and far more complex process than some enthusiasts would have us believe.

Mr. Lowne appeals to the fly. By all means let the fly be the subject of our inquiries. Of this creature he says, the nervous system undergoes *modification* but not *degeneration*. Now I ask, what part of the nervous system that is present in the maggot can Mr. Lowne find in the fly? I have studied both fly and maggot carefully, have worked at the matter long, and have utterly failed to find a trace of the nerve tissue of the maggot in the fly. Not only so, but I find the nerves of the fly as different as are the muscles from those of the maggot. The latter are altogether distinct in structure and in action. They contract at a very different rate, and are very different in many particulars.

Again, I must ask Mr. Lowne if he has seen any vestige of the mouth organs in the larva, for he says:—"It is the mouth organs of the larva which are new formations, not those of the imago." I have failed in my attempts to find any traces. There are other assertions about the alimentary canal and the sexual organs which are not proved. Does Mr. Lowne mean to say, for instance, that he or anyone else can adduce any reliable observations to prove that "the sexual organs are gradually developed, even from the time when the embryo is enclosed in the egg"? On p. 112 of his book on this very matter he says that he has not been able to verify Dr. Weissmann's assertion as to their presence, even in the larva; and now he suggests they exist in the egg!

But I must ask Mr. Lowne to explain what he means by saying in his letter, that it is an "utter mistake to suppose that any insect is re-developed during the pupa state," and that the nervous system "never undergoes degeneration;" because on p. 116 of his own book, published only last year, I find the following passage: "All the tissues of the larva *undergo degeneration*, and the imaginal tissues are *re-developed* . . . under conditions similar to those appertaining to the formation of the embryonic tissues from the yolk"!

LIONEL S. BEALE

The Auditory Nerves of Gasteropoda

In your issue for October 26, I notice an account of Leydig's recent paper on the auditory organ of the Gasteropoda, which, though excellent in other respects, has an error of omission which I should like to see rectified. When so important a discovery for morphology is discussed as that of the innervation of the otolithic sac from the supra-œsophageal in place of the sub-œsophageal ganglion which is its apparent connection in all Gasteropoda (excepting the Heteropodous forms), the credit of it should be given to the right man. That man is the most eminent and accurate of French comparative anatomists—M. Lacaze-Duthiers. Prof. Leydig states in the beginning of his own paper that Lacaze-Duthiers' statements on this subject (published in the *Comptes Rendus* about three years ago, if my memory serves me, and curiously mistranslated, *sus-œsophagien* being rendered *sub-œsophageal* in one of the first numbers of the Monthly Microscopical Journal), caused him to direct his attention again to this subject, and he has, as a result, confirmed the observations of the French *savant*, which were in opposition to the previously-received views of all observers, himself and Leydig included. Germany has a host of indefatigable anatomists, and the services of Franz Leydig, of Tubingen, are brilliant enough to eclipse most zootomical reputations; but let us not, at this moment above all others, forget to do justice, when the opportunity occurs, to a naturalist whose comprehensive, accurate, and beautiful zootomical monographs, rich in discoveries, have done more than those of any other Frenchman to sustain the great name of Cuvier's school.

Naples, Dec. 8 E. R. LANKESTER

DR. CARPENTER AND DR. MAYER

AT the Anniversary Dinner of the Royal Society on November 30, I was honoured by a request from the President to say a few words in acknowledgment of the toast to the Copley Medalist. I did so, stating briefly the origin of my acquaintance with Dr. Mayer's writings. Though Dr. Carpenter at the time was within sight of me, it did not occur to me to introduce his name into my remarks. A few days afterwards I was favoured by a letter from Dr. Carpenter, in which he reminds me somewhat sharply of this and other lapses as regards himself, and requests me to rectify the omission by a brief communication to the *Athenæum* or to NATURE. It will be fairer to Dr. Carpenter, and more agreeable to me, if he would state his own case *in extenso*. Here is his letter:—

"University of London, Burlington Gardens, W.,
"December 5th, 1871.

"MY DEAR TYNDALL,—If I correctly apprehended what you said at the Dinner of the Royal Society in regard to Dr. Mayer, you repeated what you had previously stated in your Lecture at the Royal Institution in 1863, as to the entire ignorance of Mayer's work which prevailed in this country until you brought it into notice on that occasion.

"Now, I very distinctly remember that a few days previously to that Lecture, I mentioned to you that as far back as 1851 I had become acquainted, through the late Dr. Baly, with one of Dr. Mayer's earlier publications; and that, in bringing before the readers of the *British and Foreign Medical Review* (of which I was then the Editor) the 'Correlation' doctrine, as developed in Physics by Grove, and in Physiology by myself, I had stated that we had both been to a great extent anticipated by Mayer—as I should have shown much more fully if the pamphlet had earlier come into my hands.

"I also most distinctly remember that, as you stated in that Lecture, no one in this country—'not even Sir Henry Holland, who knows everything'—had ever heard of Mayer, I spoke to you again on the subject a few days afterwards; and that you then expressed your regret at having entirely forgotten what had previously passed between us on the subject.

"As it would seem that this second mention of the matter has also passed from your mind, I shall be obliged by your looking at the passages I have marked in pp. 227 and 237 of the accompanying volume, from which I think that you will be satisfied that I had at that date correctly apprehended Mayer's fundamental idea, and that I have done the best to put it before the public that I could under the circumstances—the article having been in type and ready for press before his pamphlet came into my hands.

"Since, in thus bringing forward Mayer, I spontaneously abdicated the position to which I had previously believed myself entitled, of having been the first to put forward the idea that all the manifestations of Force exhibited by a living organism have their source *ab extra*, and not —as taught by physiologists up to that time—*ab intra*, I venture to hope that you will do me the justice of stating the real facts of the case in a short communication either to the *Athenæum* or to NATURE.—I remain, my dear Tyndall, yours faithfully, "WILLIAM B. CARPENTER

"Prof. Tyndall."

This letter was accompanied by a volume of the *Medico-Chirurgical Review*, containing an article headed, "Grove, Carpenter, &c., on the Correlation of Forces, Physical and Vital." As I am very anxious that my *amende* to Dr. Carpenter should be all that he could desire, I shall deem it a favour to be permitted to publish in NATURE the passages to which, by marginal pencil marks, he has directed my attention. The first of them is this :—

"We now come to the memoir 'On the Mutual Relations of the Vital and Physical Forces,' communicated to the Royal Society by Dr. Carpenter, which bears date June 20, 1850, and which is published in the 'Philosophical Transactions' for last year. This, we believe, is the first *systematic* attempt that has been made, in this country at least, to work out the subject, and, as it is mainly an expansion of the ideas which had been put forth in our own pages at the beginning of 1848, the author may claim priority as regards the enunciation and development of the idea, both of Dr. Fowler and Dr. Radcliffe, although to a certain degree anticipated by Mr. Newport. We shall presently find, however, that both these gentlemen were themselves anticipated in a quarter they little guessed, and the whole case is obviously one of a kind of which the history of physiology as well as of other sciences furnishes many examples, in which a connecting idea, developed in another department of inquiry, struck many individuals at once as applicable to the same class of facts, and was wrought out by them in different modes, and with various degrees of success, according to their previous habits of thought."

The impersonal way in which this and other passages of the article distribute merit among scientific authors caused me to ask Dr. Carpenter who wrote it. His reply to me was "I thought I had made it sufficiently plain to you that the article was written by myself."

Here follow the other marked passages quoted in full :—

"We must not omit, however, to give our readers some account of the remarkable production of Dr. Mayer, who seems to have arrived at conclusions in all essential respects similar to those of Prof. Grove and Dr. Carpenter previously to the publication of the first edition of the 'Correlation of the Physical Forces,' though subsequently to the delivery of the lectures in which Prof. Grove first announced his views and to the publication of the abstract of them. Of the existence of this treatise we have only recently been made aware, and we venture to affirm that Prof. Grove and Dr. Carpenter were alike ignorant of it. We bring it before the public now, both as an act of justice to its author, and also because it affords additional evidence in favour of the Correlation doctrine, that it should have been independently worked out by a clear and intelligent thinker.

"The first part of Dr. Mayer's treatise is concerned entirely with physical forces. He starts with the two axioms, 'Ex nihilo nil fit,' and 'Nil fit ad nihilum,' and founds upon abstract considerations his first argument for the unity of force, and for the convertibility of those which are commonly accounted distinct forces. Of this convertibility he then proceeds to adduce experimental proof, in very much the same mode with Prof. Grove, and he at last arrives at the following scheme expressive of their relations.

	1. Force of Gravity.	Mechanical Force.
	2. Motion.	Mechanical Effect.
	A. Simple.	
	B. Undulating, vibratory.	
Imponderables.	3. Heat.	
	4. Magnetism, Electricity, Galvanic current.	
	5. Chemical decomposition of certain elements.	Chemical Force.
	Chemical combination of certain other elements.	

"He then passes on to the study of vital phenomena, and he finds, like Dr. Carpenter, the source of all change in the living organism, as well animal as vegetable, in the forces acting upon it *ab externo;* whilst the changes in its own composition he considers to be the immediate source of the forces which are generated in it. He does not enter, like Dr. Carpenter, into an analysis of the phenomena of growth and development, but fixes his attention rather upon the production of heat, light, electricity, and (above all) motion by living bodies, and aims to show that all these forces are developed in the course of material changes in the organism, and hold a certain definite relation to them. On these points his exposition is very full and complete, and the perusal of his essay will amply repay any who desire to see how much may be done in imparting precision and clearness to physiological reasoning by minds trained in the school of exact science."

To these passages I would add one other brief quotation regarding the conversion of heat into electricity :—

"Of the production of electricity by heat, the phenomena first brought into view by Seebeck, and known under the name of 'thermo-electricity,' afford the most characteristic example. When dissimilar metals are made to touch, or are soldered together, and are heated at the point of contact, a current of electricity is set in motion, which has a definite direction according to the metal employed, and which continues as long as an increasing temperature is pervading them, ceasing when the temperature is stationary, and flowing in the contrary direction whilst it is decreasing" (pp. 213-14).

Having thus, it may be tardily, done justice to Dr. Carpenter, a very few words regarding his letter will complete the subject.

1. Dr. Carpenter has *not* correctly apprehended what I said at the dinner of the Royal Society in regard to Dr. Mayer. Neither at that dinner nor on any other occasion did I say that the ignorance of Mayer's labours in this country was "*entire*."

2. I have not been altogether unmindful of Dr. Carpenter's desire to have his name mentioned in connection with this subject. In the printed report of the lecture referred to by Dr. Carpenter, delivered not in 1863 but in 1862, and published in the Proceedings of the Royal Institution for that year, these words appear—"Mayer's physiological writings have been referred to by physiologists—by Dr. Carpenter, for example—in terms of honouring recognition. We have hitherto, indeed, obtained fragmentary glimpses of the man, partly from physicists, partly from physiologists ; but his total merit has never yet been recognised as it assuredly would have been had he chosen a happier mode of publication."

3. If this be not sufficient, my error was one of ignorance, not of will ; for it is an entirely new idea to me that Dr. Carpenter regarded his relationship to Dr. Mayer in the light of a "spontaneous abdication," and it explains to me, what I could not previously understand, the importance attached by Dr. Carpenter to the passages above quoted.

4. I have looked at p. 227, and, indeed, throughout the entire article in the *Medico-Chirurgical Review* (and elsewhere), for evidence to prove that "at that date" (or at any other date), Dr. Carpenter had "correctly apprehended Mayer's fundamental idea," which is that of quantitative or numerical equivalence. Had I found such evidence, it would give me sincere pleasure to reproduce it here, but my search for it has not been successful.

5. This however entirely depends on my ability to appreciate such evidence. Holding the opinion that he does regarding the claims of his work to public recognition, Dr. Carpenter is perfectly consistent in demanding that even in an after-dinner speech those claims shall not be ignored.

JOHN TYNDALL

THE GEOLOGY OF OXFORD*

PROFESSOR PHILLIPS'S new work on the Geology of Oxford and of the Thames Valley is a most important contribution to the knowledge of the ancient history of the earth, and supplies a need which happens just at this time to be keenly felt. The Palæozoic rocks had been described and the forms of life which they contain unfolded, in "Siluria." The second, or Mesozoic chapter, is written with remarkable ability in the present work.

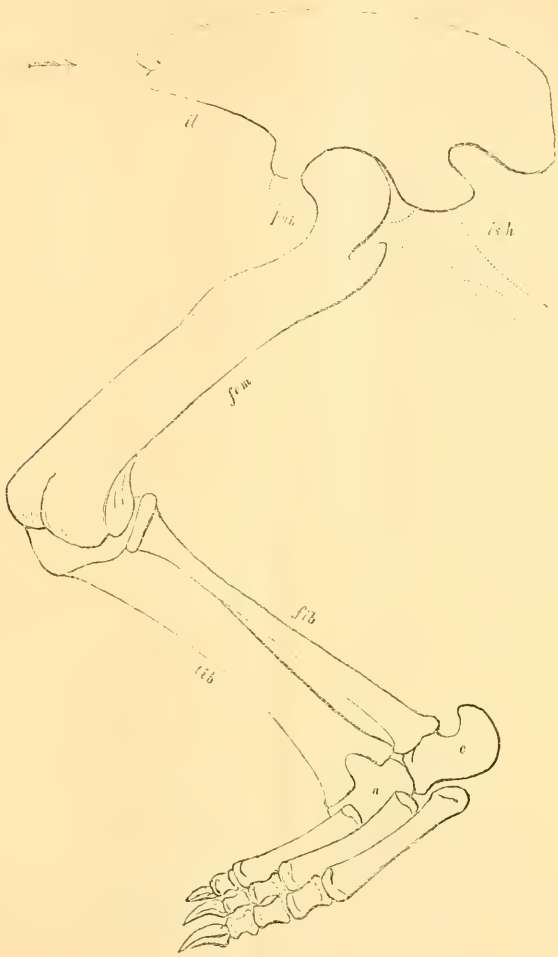


FIG. 1.—Megalosaurus—hind leg. Scale, one-tenth of nature.

This restoration in outline of the left hind limb of Megalosaurus is drawn from specimens, with the exception of the fibula, calcaneum, and ordinary phalangal bones—the claw-bone is known. Dotted lines represent the probable position of the pubic and ischial bones (according to the view of Professor Huxley); these being preserved io the British Museum and in the collectinns of the University of Oxford.

The principal bones are marked:—il. = ilium, pub. = pubis, isch. = ischium, fem. = femur, tib = tibia, fib. = fibula, c. = calcaneum, a. = astragalus. Cuvier supposed the calcaneum to be smaller than here represented.

The position of Oxford relatively to the formations which traverse Britain diagonally from the north-east to the south-west, equidistant on the one hand from the Malvern Hills which overlook the low-lying vale of Tewkesbury, and on the other from the basin of the Lower Thames, renders it a convenient centre around which to group observations which are primarily local, but which also affect the general question of Mesozoic Geology. In its latter aspect the book demands a most careful attention. The large number of plates and the carefully prepared lists

* "Geology of Oxford and the Valley of the Thames." By John Phillips, M.A., F.R.S., F.G.S. (Oxford Clarendon Press: 1871).

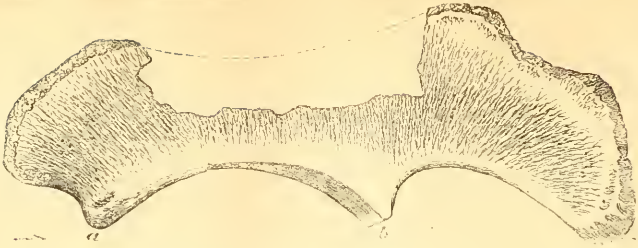


FIG. 2.—Ilium of Ceteosaurus, seen on the external face. Scale, one-tenth of nature. *a b*. The acetabulum.

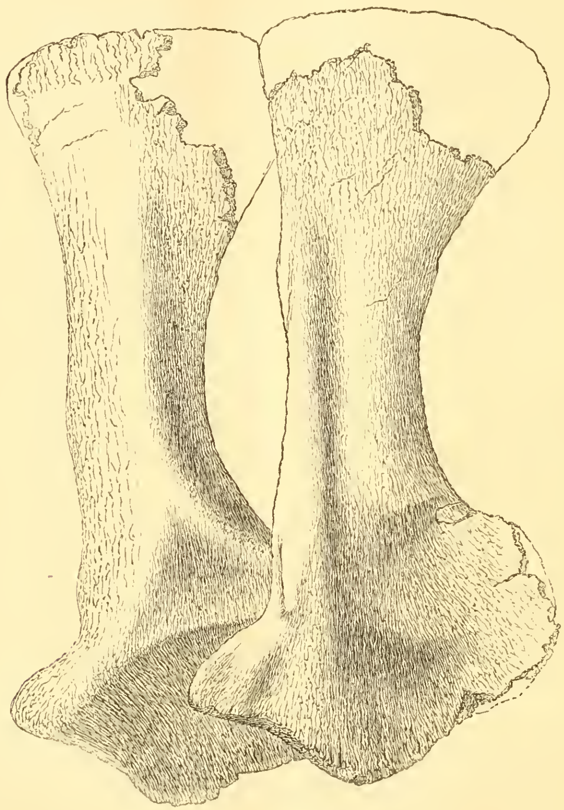


FIG. 3.—Scapulæ of Ceteosaurus. Scale, one-tenth of nature

of fossils will be welcomed by all palæontologists; and those who enjoyed the advantage of studying geology at Oxford will find in this book the subject-matter of many of the lectures, and will have recalled to their minds the many pleasant associations connected with the expeditions of the Professor.

The work, as might be expected from the great and varied knowledge of the writer, is many-sided. In it the physical geographer will find the delicate questions of denudation, and of the excavation of hill and valley, discussed; the meteorologist will find the rain-fall tabulated; the hydraulic engineer the amount of water which is available for the use of Oxford and of London; the physicist the temperature and the prevailing winds; and the surveyor the position and thickness of the various strata from the Malverns eastward to London.

Prof. Phillips has, however, devoted his main strength to the description of the wondrous forms of reptilian life which

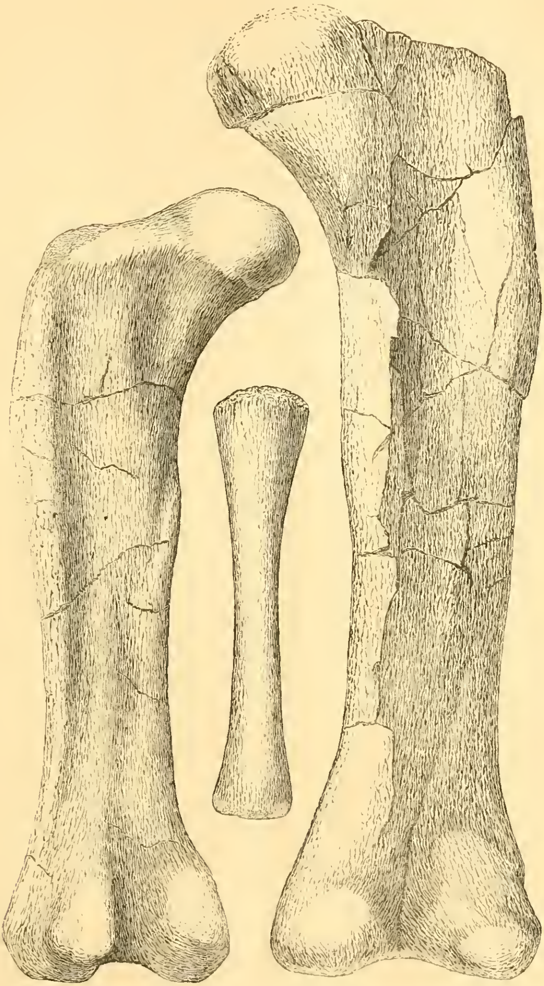


FIG. 4.—Femora and Fibula of Ceteosaurus. Scale, one-tenth.
The left-hand figure represents the specimen found in 1848; the right-hand figure that found in 1868; in the middle a small fibula found in 1848 is shown.

have been furnished by the neighbourhood of Oxford, and which are preserved in a museum which is worthy of an old and wealthy University. The description of the Megalosaurus, and especially of the Ceteosaurus, is a most valuable addition to Palæozoology.

We owe to Prof. Huxley the clue to the right interpretation of the bones of both these animals, and the right definition of the whole group of Deinosauria, or Ornithoskelida, to which they belong, as being intermediate in character between the struthious birds and the reptiles. To this conclusion, however, he was largely aided by Prof. Phillips, and that it is true is rendered almost certain by the independent observations of Prof. Cope on the fossil reptiles of America. When the large pelvic bone from Stonesfield had been assigned to its true position in the skeleton of its possessor, and the so-called "clavicle" shown to be in all probability a long, stiliform, bird-like ischium, there could no longer be any doubt as to the kind of animal to which it belongs. The massive anchylosed sacrum of five vertebræ, and the whole arrangement of the pelvic arch, as well as the peculiar form of the astragalus and the shape of the coracoid and scapula, indicate a close alliance with the birds; while, on the other hand, the rest of the structure is mainly reptilian.

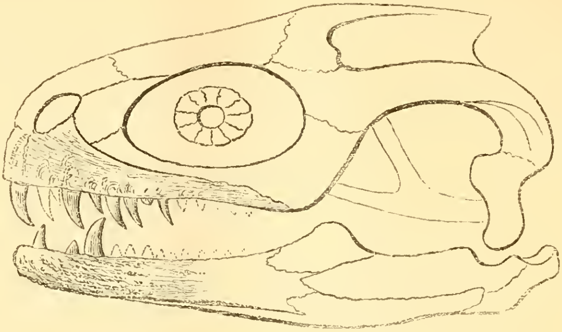


FIG. 5.—Head of Megalosaurus. Scale, one-tenth of nature.
Restoration of the head and lower jaw, of which, however, only the anterior portions are known. These are shaded. The type of Varanus is followed in general, but the postorbital arrangement is different, the bony circle there being completed from considering iguana and other lizards with some eye to crocodile. The length of head as thus drawn (thirty-nine inches) is less than that usually allowed (five feet).
The posterior part of the maxillary bone is separated from the orbit, notwithstanding its smooth, apparently free edge, by an intervening continuation of the jugal. This may be objected to. The nasal cavity is supposed to be divided by a median ridge (the single nasal continuous with the intermaxillary bone) into two openings, as in some of the monitors. The intermaxillary bones, which originally included four teeth each, appear united to the maxillary in this adult specimen.

FIG. 6.—Megalosaurus. Scale, one-tenth of nature.
The left aspect of the shoulder girdle is here restored in outline from specimens in the Oxford Museum, which are complete except in regard to the lower end of the humerus. It will be remarked how bird like in the general arrangement and the forms of the bones is the humero-scapular structure, and specially how closely it resembles Apteryx.
1. Scapula. 2. Caracoid. 3. Humerus.

The specimens which are preserved in the Oxford Museum, and which have been figured by Prof. Phillips, afford a very complete idea of the creature. The magnificent upper maxillary described by Prof. Huxley in the "Geological Journal," enables the front portion of the cranium to be restored with considerable certainty, and the accompanying woodcut (Fig. 5) may be taken to represent the entire head.

The premaxillaries of the Megalosaurus from the Oxford clay, in the collection of Mr. James Parker, are traversed by foramina which may indicate the presence of a small horny beak, or snout.

The arrangement of the shoulder girdle may be seen in Fig. 6, in which 1 = Scapula; 2 = Coracoid; and 3 = Humerus, as well as that of the pelvic arch and hind leg (Fig. 1), and the comparison of the two diagrams, will show the enormous disproportion of the hind to the fore limb in respect of size. All these three figures are drawn to one-tenth of natural size, and enable us to realise the form of one of the most remarkable of the fossil reptiles. The recent discovery of a nearly perfect skeleton by Mr. James Parker establishes the fact that some, at least, of the opistho-cœlian vertebræ, on which the genus Streptospondylus has been based by Prof. Owen, belong really to this animal. In point of time, the Megalosaurus lived from the Liassic to the Wealden age, and was one of the most formidable inhabitants of the great Mesozoic continent. The pains and labour which Prof. Phillips has bestowed in collecting and putting together the fragments and *disjecta membra* of the animal, and the careful criticism to which he has subjected each bone, render this portion of the work peculiarly valuable.

Nor is the chapter on the most gigantic of the fossil reptiles, the Ceteosaurus, inferior in interest to that which relates to Megalosaurus. The bones discovered in the Great Oolite at Enslow Bridge, near Oxford, in 1870, settled for ever all doubt as to the animal having been aquatic or terrestrial. The scapula (Fig. 3) and the ilium (Fig. 2) resemble in general outline those of Megalosaurus, and show that the animal belongs to the same Deinosaurian class, although "its fore limbs are more crocodilian," and "its pelvic girdle more lacertian." And the evidence offered by the articular ends of the bones of the extremities being adapted for movement in particular directions, the possession of large claws, and the hollowness of the long bones, indicate that it was of terrestrial, and not, as its name seems to imply, of marine habit. It may, however, have been, as Prof. Phillips suggests, "a marsh-loving or river-side animal." Its gigantic size may be gathered from the fact that one of the femora measures no less than 64, and a humerus 51·5 inches (Fig. 4).

Nor is there evidence wanting as to its diet. From the mutilated fragment of a tooth in the Oxford Museum, Prof. Phillips infers that its possessor lived on vegetables, since it resembled "that of an iguanodon in general shape (as far as can be known, one edge being broken), with a similar sweep of the concave surface seen in the diagram, and corresponding alternation towards the edge. The edge is not serrated, but the striæ of accretion are so arranged as to suggest that it may have been." The truth of this conclusion is proved by the subsequent discovery of a nearly perfect crown by Mr. Burrows, one of my students, in the Enslow Quarry, which has very much the appearance of a young tooth. It presents the serrations which have been worn away in the specimen above described, and bears out completely Prof. Phillips's description.

I have chosen merely these two animals as illustrating the subject-matter of the book, which is in every sense worthy of the high reputation of its author. W. B. D.

PARTHENOGENESIS AMONG THE LEPIDOPTERA

THE part of the *Archives Néerlandaises*, published by the Societé Hollandaise des Sciences à Harlem, for 1870, contains the results of some very interesting experiments undertaken by M. H. Weijenbergh, jun., on the above subject, one fraught with considerable interest to others besides entomologists. By Parthenogensis is meant the power that is possessed by females of producing eggs endowed with vitality, and from which young ones are produced, without impregnation taking place on each occasion. This subject has been extensively treated by von Siebold in his "Wahre Parthenogenesis bei der Schmetterlinge und Bienen," Leipzig, 1856, but confirmatory and new investigations were much needed. Those of M. Weijenbergh were conducted with every possible care and precaution, so that they can be relied upon. In the autumn of 1866 he saw a male and female of the species *Liparis dispar* together, and some days afterwards he saw in the same place a great quantity of the eggs, about 500 in number. In order to leave the rearing of these to natural processes, as far as possible, he left them exposed all the winter in the open air, and in April 1867, he removed them into his house. Before the end of the month the caterpillars had successively made their appearance. These were regularly fed, and by the middle of July each of the chrysalides which had been formed during June gave birth to a perfect butterfly. It was easy, with a little practice, to distinguish the sexes whilst in the caterpillar state, and all the males were removed as far as possible, and the females were placed in a box closed to all access from without. So successfully was this separation of the sexes effected, that only one male butterfly made its appearance among the females; and, as these had been successively removed to a third closed box as soon as they escaped from the chrysalis state, it was only necessary to sacrifice the three or four females which were in the box at the time. In all, about sixty females were obtained, to which there was absolute certainty that no male could by any possible chance have had access. Of these, two-thirds laid eggs in the autumn,—some, one, two, or three eggs only; others as many as ten or twenty, but yet even at the most not one-twentieth of the eggs of their mother. The other one third laid no eggs at all In all about 400 eggs were collected, which were removed and carefully packed up till April 1868, when a large number of little caterpillars were seen. These were immediately placed on leaves in a large glass vase and watched carefully. It was easily to be seen that this batch of caterpillars possessed far less vitality than those of the previous year. A large number of the eggs dried up and were worthless, some fifty caterpillars alone appearing, and of these only about forty survived to become chrysalides. From these, by the end of July, twenty-seven butterflies made their appearance. The same precautions having been taken as before, the number of females was found to be fourteen. Of these, when again there had been no possibility of male access, one half laid no eggs, the remaining half, however, laying in all a fair number. As in previous years, these were removed and left all the winter carefully packed up, till, in April 1869, three years after the commencement of the experiments, young caterpillars again made their appearance. From these, strange to say, the number of butterflies obtained was in excess of those obtained in the previous year. The number of females as compared with males, was almost the same, in contradiction to the results of other investigators, which had indicated the probability of the ratio of the males to the females greatly increasing with each additional year. The eggs laid by the females of this year, carefully isolated as before, were packed up during the winter, but when examined in the spring of last year, 1870, no caterpillars made their appearance, the eggs became shrivelled up, and the experiment was at an end. There is every reason to believe that it was most carefully conducted, and that every regard was paid to strict accuracy during the whole three years or more that the experiment was being carried on. The results amount to these:—

(1.) Aug. 1866, eggs laid by impregnated female; April 1867, caterpillars appear; and, in July, perfect butterflies.

(2.) Aug. 1867, eggs laid by females of this year without impregnation; April 1868, caterpillars appear, and, in July, perfect butterflies.

(3.) Aug. 1868, eggs laid by females of this year without impregnation; April 1869, caterpillars appear, and, in July, perfect butterflies.

(4.) Aug. 1869, eggs laid by females of this year without impregnation; April 1870, no results—the eggs all dried up.

Thus, after the first impregnation of the female in the autumn of 1866, three successive broods of caterpillars and, ultimately, of butterflies made their appearance; and four successive times were eggs laid without further impregnation, in three of which they proved endowed with vitality. It would take a long series of experiments, each conducted with the same care as this, before an average could be drawn to determine the limit of this strange reproductive power. These experiments are so easily performed, and yet so valuable when accurately made, that a wide field is opened to those who do not care to undertake long and elaborate scientific investigations, and to such we most cordially commend them. Their value, as bearing on the theories of spontaneous generation, is very great, as there is much apparent probability that this power of Parthenogenesis will increase as we descend in the scale of life just as it decreases as we ascend. By its aid many phenomena, now apparently very strange and perplexing, will be found to be but obeying one great and universal law of nature, which becomes less visible the higher we ascend in the scale of life, but yet never ceases.

In conclusion, it may be stated that this power of Parthenogenesis has been found in many species of butterflies, and also among bees; and M. Weijenbergh, at the end of his interesting paper, gives a list of the seventeen or eighteen species which are known to him, or which are recorded as possessing this power. It is extremely probable that the more the subject is investigated, the more commonly will it be found to exist. J. P. E.

RESULTS OF SANITARY IMPROVEMENT IN CALCUTTA

WHEN a great public work is being done, it is a duty to call attention to it. In March 1862, Prof Longmore, of Netley, who had acted as Sanitary Officer during the Mutiny at Calcutta, gave the following evidence before the Royal Commission on the sanitary state of the Indian Army:—"As regards the chief part of this extensive city (Calcutta)—that inhabited by the native population—the pestilential condition of the surface-drains and yards, and many of the tanks among the huts and houses, would not be credited by any one who had not been among them." In the "Report on Sanitary Improvements in India up to June 1871," recently printed by the India Office, is given a table showing that the cholera mortality in Calcutta had, for twenty years preceding 1861, averaged nearly 5,000 deaths per annum. In 1860 the cholera deaths were 6,553, and in 1866 they were 6,823. About this latter date works of drainage and water supply were commenced and have been gradually extended. Water is taken from the Hooghly and thoroughly filtered—it is then conveyed in pipes 12¼ miles in length to a reservoir in Calcutta and thence distributed. The whole population had this benefit conferred on them in the beginning of 1870, from which date the use of foul tank and river water was discontinued.

The drainage works are as yet confined to the southern districts, the sewage from which is conveyed to an outfall at the Salt Lake, and will be passed over a square mile of reclaimed land there, for irrigation of crops. The mortality from cholera in 1870 was 1,563, and the general mortality has fallen year by year with the extension of the works. Last year (1870) the death-rate was 23·4 per 1,000, considerably less than half what it was in 1865.

At a Social Science meeting held in Calcutta last March, a native physician, Dr. Chuckerbutty, gave his experience of the sanitary results as follows:—"I am in the habit of visiting, in the pursuit of my profession, the houses of the rich, as well as of the poor, in both divisions of the town, and I frankly confess that in the southern division, wherever the drainage works have been brought into play, the dwellings even of the humblest cottagers are in an infinitely better sanitary state than the mansions of the richest millionaires in the northern division where the drainage operations have not been extended. Before the completion of the water-works and the partial operation of the new drainage works, the mortality in Calcutta from dysentery, cholera, and fever, was most appalling. In 1865 dysentery was so common and fatal that sloughing cases of it were of daily occurrence. Such cases are now rarely to be seen. My annual share of cases of cholera in the Medical College Hospital before the completion of the new water-works was about 700, and I declare to you that, during the last eight months, I have scarcely had a dozen cases of that disease. Fever, too, has decreased during the same period in a like manner." The actual deaths from cholera in April, May, and June, of the present year were 85, 29, and 26, respectively.

After such results as these, we need not feel surprised that the Justices of Calcutta, a large proportion of whom are enlightened native gentlemen, decided unanimously last August to extend the drainage works all over the city, notwithstanding the opposition on purely theoretical grounds of certain British medical officers who ought to have known better, to the use of ordinary house drainage for Indian houses.

The opinion of the Army Sanitary Commission on this subject is quoted as follows in the India Office report:—"The municipal authorities of Calcutta and their officers have set an example of enlightened administration and effective expenditure to other Indian municipalities, which it is hoped will be followed. There are indeed few cities anywhere which can show so much good work done in so short a time and with such promising results for the future."

The laws of nature are the same everywhere. Calcutta has in times past suffered as London used to do from fatal fevers and bowel diseases, and there is now every prospect that a few years of active work will remove this stigma from the capital of the East, as it has been removed from the metropolis of the British Empire.

NOTES

THE following telegrams respecting the Total Eclipse of Dec. 12 have been received since our last:—"From the Governor of Ceylon to the Earl of Kimberley, dated, Colombo, Dec. 12, 10.45 A.M.:—'A telegram from Jaffna states that splendid weather prevailed during the eclipse. Most satisfactory and interesting observations have been made.'" "Mangalore, Dec. 16.—The eclipse observations have been very successful. The extension of the corona above hydrogen apparently small. Five admirable photographs have been taken." From Mr. Davis, photographer to the English Eclipse Expedition, through Lord Lindsay:—"Mangalore, Baikul.—Five totality negatives; extensive corona; persistent rifts; slight external changes." The French Academy of Sciences has received from M. Janssen the following telegraphic despatch, dated Octacamund, December 12, 5h. 20m.:—"Spectre de la Couronne attestant matière plus loin qu'atmosphère du Soleil."

WE can hardly credit the report which has just reached us that the Treasury has, at the last moment, declined to sanction the expenditure of public money on the publication of the Eclipse Reports of 1860 and 1870. We understand the combined report is now nearly ready, and both Parliament and the nation are entitled to receive a statement of the manner in which the public money has been expended. There are innumerable cases which may be cited as precedents for the publication of similar documents by the Government; as, for example, the Survey of Sinai, and the annual Greenwich Reports of Observations. After the Government has so generously granted money for recent scientific observations, we can hardly believe that the spirit of parsimony will so far prevail at the last moment as to mar, in this manner, the services it has performed towards Science.

THE death is announced on October 10, in Nicaragua, of fever, of Dr. Berthold Seemann, one of our most enterprising travellers and naturalists. Born at Hanover in 1825, Dr. Seemann was, in 1846, appointed naturalist to H.M.S. *Herald*, in its survey of the Pacific, during which voyage he had the opportunity of exploring, more thoroughly than almost any other European, the Pacific countries of South America and the Isthmus of Panama. In the same vessel he subsequently visited the Arctic regions, and the "Narrative of the Voyage of H.M.S. *Herald*," by Sir John Richardson and Dr. Seemann, is an important contribution to the natural history of previously little-known regions, the portion contributed by the latter comprising an account of the flora of Western Eskimo-land, north-western Mexico, the Isthmus of Panama, and the island of Hong-Kong. In 1860 he was sent by the English Government to the Fiji Islands, then lately acquired, and on his return published two works, one containing a narrative of his mission, the other, under the title of "Flora Vitiensis," a history of the vegetable productions of the islands. Since 1864, he has been greatly interested in the mining capabilities and other resources of the various states of Central America, and has spent much of his time there in the interest of different trading communities, and in promoting the route across the Isthmus. Dr. Seemann is the author of several popular botanical works in German and English, and has been since its foundation, Editor of the *Journal of Botany, British and Foreign.*

PROF. SEDGWICK's appeal for subscriptions from members of the University of Cambridge, to enable him to purchase the valuable collection of fossils belonging to Mr. Leckenby, has resulted in the collection of the sum required, 800*l*. Arrangements have been made for the completion of the purchase, and it is expected that in a few weeks Mr. Leckenby's valuable collections will be deposited in the Cambridge Geological Museum. This prompt and liberal response to the touching appeal of the venerable Professor demonstrates the regard in which he is universally and deservedly held by the members of the University.

THE following is the result of the examination for the Natural Science Tripos at Cambridge:—First Class—Garrod, John's; Lydekker, Trinity; Lewis, Downing; Warrington, Caius. Second Class—W. Edmunds, John's; Fox, Peter's; Read, John's; Owen, Downing; Everard, Trinity; Maudslay, Trinity-hall: Brewer, John's; Buddon, John's; Wigan, Trinity; Blunt, John's. The following acquitted themselves so as to deserve ordinary degrees:—Burrows, Caius; Murphy, John's; Phelps, Sydney; Pittman, Corpus; Wakefield, Caius. In the second class Fox and Reed are bracketed, also Brewer, Buddon, and Wigan.

NEXT term, Mr. Ruskin, Slade Professor of the Fine Arts at Oxford, will deliver a course of lectures on "The Relation of Natural Science to Art."

THE Government is advertising the appointment, by open competition, of a clerk to the Curator of the Royal Gardens at Kew, and of a second assistant in the Herbarium. The salaries commence at 100*l*. and 60*l*. respectively, and the specified age is in one case from 20 to 30, and in the other from 18 to 30. The examinations will take place on January 16.

THE following lectures have already been delivered this winter at Manchester, as Science Lectures for the People:—The first on November 3 on "Yeast," by Prof. Huxley; November 10 "Coal Colours," by Prof. Roscoe; November 16, "The Origin of the English People," by Prof. A. S. Wilkins; November 24, "The Food of Plants," by Prof. Odling; December 1, "The Unconscious Action of the Brain," by Dr. Carpenter. These lectures are always well attended, but since they are all reported and printed at the low price of a penny each, they appeal to a much wider circle than most of a similar character. This is the third year of these Science Lectures. The lectures for this session and those of past years are published by John Heywood, Deansgate, Manchester.

THE *Pall Mall Gazette* states that the approaching 400th anniversary of the birth of Copernicus has revived a contest of long standing between Poland and Germany, each of which claims the great astronomer as a son. The Germans argue that he was a German because he was born in Thorn, which at the time of his birth was under German rule; to which the Poles reply that Thorn was then really a Polish town, having been separated from Poland only seven years before; that his father and mother were Poles; that when he studied at Padua he enrolled himself among the students of the Polish nationality; and that throughout his life he gave constant proofs of his attachment to Poland and her King. Poland has always honoured Copernicus as one of her greatest men. A statue of him was erected by national subscription many years ago at Warsaw, and there are two others at Cracow, besides which numerous Polish medals and books have been issued in celebration of his memory. The anniversary above mentioned will be celebrated on the 19th of February, 1873, and great preparations are already being made at Posen for the occasion. The "Society of the Friends of Learning" in the old Polish city held a meeting the other day, at which it was decided, on the motion of a Polish clergyman, Canon Polkowski, to offer a prize for the best life of Copernicus, comprising the results of the latest investigations on the subject, and to publish it in the Polish, French, and German languages.

WITH a view towards the completion of the collection of water colour paintings illustrating the history of that art, Mr. William Smith, Vice-President of the National Portrait Gallery Trustees, has allowed Mr. Redgrave, R.A., the Inspector-General for Art, to select from his choice and valuable collection as many rare specimens as, in Mr. Redgrave's judgment, would illustrate the *early period* of the art. The works selected by Mr. Redgrave have been presented by Mr. Smith to the nation.

IT has been arranged that the new machines for printing, composing, and distributing type, which have been recently perfected at the *Times* printing office, shall be completely exhibited in working at the London International Exhibition of 1872. The power of rapid production by these several means is probably threefold in advance of any existing modes of printing. The *Mail* newspaper will be printed three times a week, and if possible the daily supplement of the *Times*.

THE third part of Mr. W. H. Baily's "Figures of Characteristic British Fossils, with Descriptive Remarks," has just been published. Part 4, which will complete the first volume, is in progress; each part consists of ten beautifully-executed plates, and the text is interspersed with many woodcuts. These latter are chiefly of recent forms. The figures are for the most part original, and this little work most worthily fills up a blank in biological literature.

FROM the commencement of November till December 12, a period of six weeks, the temperature at London was below the average, with the break of only a single day. The tables forwarded weekly by Mr. Glaisher to the *Gardener's Chronicle* show the average depression during the whole of that period to have amounted to as much as 6°·5 F. below the mean of the last fifty years, the minimum being on December 8, when the thermometer fell to 18°·6, and the temperature of the twenty-four hours was 19°·3 below the mean. Throughout France the month of November was very severe, the mean temperature of the month having been lower only four times during the last century. According to statistics presented to the Academy of Sciences by M. Ch. Sainte-Claire Deville, the thermometer fell as low as −11°·3 C. (11°·7 F.) at Montargis on December 3, while even at Marseilles the remarkably low temperature (for that latitude) of −2°·5 C. (27°·5 F.) is recorded on November 23. During the present month the frost is stated to have been still more severe in France and Italy, where much snow has fallen at Rome; and the unusual depression appears to have extended to North America.

THE Smithsonian Report, 1869, contains an account of the eruption of the Volcano of Colima in June 1869, by Dr. Charles Sartorius. The height of the volcano is 11,745 feet, and it had remained in repose since the last eruption in 1818. On June 12, 1869, dense smoke issued from the crater, and violent detonations were heard. On the 13th smoke and stones were ejected from the crater, and a "glowing upheaval" of the surface was seen. It was visited on June 15, when it was found that an upheaval of some 114 feet by 754 feet had taken place, forming a flattened arch. The appearance was that of a wild mass of volcanic red-hot rocks heaped one upon another, and constantly in motion, not unlike freshly-burned lime when sprinkled with water. The rocks which rolled down were, on cooling, of a grey colour. A piece broken off rang like glass, and was vitreous and porous.

In the middle of the upheaved mass the movement was strongest; three large clefts and intense light were displayed, while engulphed stones, which were swallowed up in great masses, were followed by a noise as of violent wind, and by clouds of smoke sometimes blue, sometimes yellow. The temperature of the air in the vicinity was 126° F. The stones in the midst of the heaving mass seemed to be softened, though not melted, and no flow of lava took place. This upheaval had taken place on a small, flat plain upon the north-east side of the mountain, it ascended to the scarp of the cone, and stretched in the direction of the snow peak, which was some $2\frac{3}{4}$ miles distant. On reaching this summit the temperature was found to be 41 F. From here the whole of the new upheaval could be surveyed. In the middle of it the most vehement movement was in progress, attended by the constant upheaving and descent of rocky masses, fire, and blue and yellow columns of smoke. The upper ancient crater has a diameter of 492 feet, and from it arose dense sulphurous vapour. Later explorers found a fissure from the new upheaval to the upper peak, 1—3 feet wide and about 3 feet in depth, but neither heat nor vapour issuing from it. Such volumes of fetid gases issued from the fissure that the inhabitants of the district were forced to leave their abodes. Cows and sheep were killed by it, so that it was found necessary to drive away the herds from the neighbourhood of the volcano.

PROF. VERRILL has lately given, in the *American Journal of Science*, an account of the researches in marine zoology prosecuted by him during the past summer at Wood's Hole, Massachusetts, in connection with investigations of Prof. Baird respecting the food fishes of the coast of the United States; and in this he calls the attention of zoologists to some of the more important features of these examinations, promising a fuller account hereafter. One of these results consisted in ascertaining that, while the shores and shallow waters of the bays and sounds, as far as Cape Cod, are occupied chiefly by southern forms belonging to the Virginian fauna, the deeper channels and central parts of Long Island Sound, as far as Stonington, Connecticut, are inhabited almost exclusively by northern forms, or an extension of the Acadian fauna. Both the temperature observations at the surface and the deep-sea dredgings prove that there must be an offshoot of the arctic current settling into the middle of Vineyard Sound. Quite a number of interesting ascidians, both simple and compound, were met with by Prof. Verrill, several of them entirely new to science. Several new sponges were collected, and also a large number of crustaceans and molluscs previously unrecorded in that region. We would refer our readers to Prof. Verrill's article in the November number of the *American Journal of Science* for these interesting facts.

Harper's Weekly furnishes the following additional information of the great exploring expedition upon which Prof. Agassiz has been expecting to engage during the voyage of the Coast Survey steamer *Hassler*, from Boston to San Francisco, by way of the Straits of Magellan. The expedition was originally to start as early as July or August, and in that event the exploration in question would have commenced off the coast of the United States. Owing, however, to unexpected delays, the vessel has but recently fitted out and reported at Boston, where she has been detained, undergoing alterations of her machinery. We have already noticed the general plan and objects of the expedition. The scientific corps, as will be remembered, consists of Prof. and Mrs. Agassiz, Count Pourtales, ex-President Hill, o Cambridge, Dr. White, Mr. James Blake, and Dr. Steindachner, each gentleman having special charge of a particular department of the work, and interested in its successful accomplishment. The vessel itself is under the command of Captain P. C. Johnson, with Messrs. Kennedy and Day as lieutenants. Owing to the lateness of the season, the original plan of making extended explorations in the West Indies and off the eastern coast of South America has necessarily been modified, and the vessel will probably proceed almost directly to the Falkland Islands and the Straits of Magellan, there to commence the comprehensive investigations proposed, as otherwise a sufficient share of the summer season of the Straits could not be secured. The Atlantic Ocean work thus given up will, in all probability, partly at least, be performed by the *A. D. Bache*, a consort of the *Hassler*, next year.

THE American Museum of Natural History, established at Central Park, New York, has, we learn from *Harper's Weekly*, had a most liberal offer made to it. The collection of shells of Dr. John C. Jay, formerly of New York, but now of Rye, is well known as one of the largest in the world; indeed, some years ago it was decidedly the finest in the United States; and although, with the lapse of years, the doctor has been less energetic in keeping it up to the present date, yet it forms a cabinet of magnificent extent, embracing, it is said, 14,000 species, 20,000 varieties, and 50,000 specimens, and costing many years of labour, and over 25,000 dols. in money. In addition to this, there is a library of 850 bound volumes, almost approaching completeness in its extent upon the subject of conchology. This has cost the doctor 10,000 dols., many of the works having been purchased at a time, too, when they were cheaper than at present. The doctor now offers to sell this library to the Museum of Natural History for the sum of 10,000 dols., and with it to present the entire collection of shells just referred to, so that the whole may go together, and form a complete section of the museum.

ADVICES from Portland, Oregon, under date of November 17, announce the arrival of Prof. O. C. Marsh, with his party of Yale College students, from an extended geological and palæontological exploration in the Blue Mountains and the John Day Valley. As might have been anticipated from the previous discoveries of the Rev. Thomas Condon, of Portland, in the same region, under much less favourable auspices, very extensive collections of fossil animals were made, which, when placed, as intended, in the museum of Yale College with those previously gathered by Prof. Marsh, will make a series of the extinct vertebrates of North America unequalled in any other cabinet.

AT the meeting of the Norfolk and Norwich Naturalists' Society, held Nov. 18, Mr. Barrett read some further notes on the coast insects found at Brandon, which he considered confirmatory of the opinion expressed by him in a former paper, that these species have occupied this district, now far inland, from the time when it was part of the sea-coast. Amongst other coast species mentioned by Mr. Barrett was *Agrostis Tritici*, and of this species he remarked that, although it occurs sparingly on inland heaths, all the specimens are of a dull brown colour, whilst those found on the sea-coast are generally distinctly marked and richly coloured; all those taken by him at Brandon had precisely the deep style of colour and markings which characterise it on the sea-coast. *Agrostis cursoria*, although very abundant on the sea-coast, is not to be found at Brandon; and this Mr. Barrett considers a very strong proof hat the other strictly littoral species enumerated have not reached their present situation by migrating across the intervening land from the present sea-coast. This species he thinks it not improbable was an immigrant from the eastward at a comparatively recent date, and that it has attained its greatest abundance on the spot where it first obtained a footing. It would not, therefore, have been an inhabitant of this portion of the post-glacial coast.

AN earthquake shock was felt in New Jersey, Delaware, and Pennsylvania in the United States, on October 9. At Delaware it was noticed at 9.40 A.M., and at Philadelphia at the same time.

*THE MONOCOTYLEDON THE UNIVERSAL TYPE OF SEEDS**

IT must be evident to those who heard my paper on "Adnation in Coniferæ" at the Chicago meeting of the Association that the observations there detailed could scarcely be accounted for, if the belief be true which is generally held by botanists, that the leaf originates at the node from which it seems to spring. It is not, however, an object with me to attack existing theories, or establish new ones, but simply to present facts as I see them. The origin of the leaf will no doubt prove a question which will in time take care of itself. But this generalisation cannot be avoided by the readers of that paper, that the whole plant is originally a unity; and that the subsequent formation of elementary organs, and their complete development, or absorption into one another, is the result of varying phases of nutrition. The leaves in Coniferæ were found to be free or united with the stem in proportion to the vigour of the central axis. Following up the subject, I now offer some facts which will show that all seeds are primarily monocotyledonous; and that division is a subsequent act, depending on circumstances which do not exist at the first commencement of the seed growth.

It is well known that in some species of Coniferous plants the number of cotyledons varies. I have noticed in addition to this that whether the cotyledons are few or many, there is no increase in the whole cotyledonous mass. In the Norway spruce, *Abies excelsa*, there are sometimes as many as ten cotyledons, in others only two. In the latter case they are broad and ovate, while in the former they are narrow and hair-like; in short, when in the two cotyledoned state it is not possible to note any difference between a seedling Norway spruce and a Chinese arbor vitæ, *Biota orientalis*, except by the lighter shade of green. The two-leaved condition is not common, but specimens of threes and others I exhibited to Drs. Torrey and Gray at the Troy meeting. Any one who will examine sprouting seeds of the Norway spruce will agree to the proposition that the cotyledons are not original and separate creations, but a divided unity. My next observations were on some acorns of *Quercus agrifolia*, the division into cotyledons were numerous and irregular. Cut across vertically, some represented the letter C, others the letter N, and again, with four cotyledons the letter M. Here again it was clear that whatever the form and number of the cotyledons, there was no increase of the original cotyledon mass. Examining sprouting peach kernels, the variations in form and number were of the most remarkable character. I need not repeat them in detail here, as they are reported in the April and May "Proceedings of the Academy of Natural Sciences of Philadelphia." In addition to the fact of no increase in the whole cotyledon mass, it was here clear that when the cotyledons were duplicated, the duplications at least were subsequent to the original ones. Still so far nothing had been seen to indicate when the first pair of cotyledons were formed. *Quercus macrocarpa* and *Quercus palustris* were silent to my questions. In a large number I found no variations whatever. Each mass was divided smoothly and exactly into two cotyledons. *Quercus robur*, the English oak, however, gave some curious evidence. Two germs under one seed coat were numerous, and often three, and the cotyledons took on a variety of forms. But there was never any more increase in the cotyledonous mass than if but two lobes had been formed, and there was no more rule in the division than there would be in the sudden breakage of a piece of glass. A detailed account of these will also be found in the "Proceedings of the Academy of Natural Sciences of Philadelphia" for May. *Quercus rubra*, the American red oak, furnished the one link wanting to connect the first division into lobes with the other phenomena. All the acorns examined had three or four sutures in the cotyledon mass, and extending all along the longitudinal surface externally, without any reference to cotyledonal divisions. These sutures extended sometimes but a line in depth, at others almost to the centre of the mass, always accompanied by the inner membrane, as is the case in ruminated seeds. The whole mass was divided only in two parts in any that I examined of this species, but the division was always in the direction of the sutures. Hence each cotyledon was very irregular. Sometimes one-third the mass only went to one while the other had two-thirds of the whole mass. It was easier to burst in the weaker line of resistance. But the interest for us is to note that ordinarily the cotyledonous mass was a unit—then the sutures or fissures were formed, and ultimately the two divisions of the lobes followed in their direction. The division was the last condition, not the first. I know how much we should guard against generalising on a limited supply of facts, but it requires an effort to believe that oaks, pines, and peaches, as we have seen primordially monocotyledons, are in this respect different from other so-called dicotyledonous plants; and if we grant that all seeds are primarily monocotyledonous, may we not ask why in any case they are divided? We have seen that there is no increase of mass in the division, the same amount is furnished in one as in many. Would it in any way injure the Indian corn to have its mass divided into two lobes? or would not the plantlet be as well provided for if the acorn were in one solid mass? Division would seem to be a necessity occurring subsequent to organisation, and existing from the position of the plumule alone. In monocotyledons, as we know, the plumule is directed parallel to, or away from, the cotyledonous mass, when, of course, on this theory, it remains an undivided mass. But in the dicotyledonous section, the plumule is directed towards the apex of the mass; and as we know in the case of roots against stone walls, or mushrooms under paving-stones, the disposition in the growing force of plants is to go right forward, turning neither to the right nor the left; so in this mass of matter the development of the germ would make easy work of the division; and no doubt often at so early a stage as to give the impression we have been under hitherto, that the division is a primary and essential process.

* Abstract of a Paper read at the Indianopolis Meeting of the American Association for the Advancement of Science, August 1871, reprinted from the *American Naturalist*. By Thomas Meehan.

SCIENTIFIC SERIALS

THE *Monthly Microscopical Journal*, No. 35, November 1871. "On the Form and Use of the Facial Arches," by W. Parker, F.R.S., is chiefly occupied by observations on embryo salmon. "Another Hint on Selecting and Mounting Diatoms," by Capt. Fred. H. Lang, details the method employed by the author for remounting diatoms, either previously badly mounted, or from which it is desirable to select certain forms.—"The Monad's Place in Nature," by Metcalfe Johnson, M.R.C.S.E., has for its object to show a connection between the earlier forms called Monads, and those higher and more complicated organisms at present recognised under the name of Infusoria, Mucedinæ, Confervæ, Oscillatoriæ, &c. The conclusions deduced from some of the experiments are that the author looks upon Monas in its earliest forms to be the starting point whence several products may result, and among the number are Infusoria, Mucedinæ, Englenæ, Oscillatoriæ. He is induced to believe that the Pin-point Monad, when developed under absence of light and only a limited quantity of air, gives rise to the class of plants known as Mucedinæ. Again, he maintains that during the watching of the liquids under experiment the Monads presented various forms, evidently transitional, from the round Pin-head Monad to oval young Paramœcia, until we come to sufficient size to give it a name such as *Kolpoda Cucullus*, &c.—"Infusorial Circuit of Generations," by Theod. C. Hilgard, deals with a similar subject, but in a very different style. It is often very difficult to gather the author's meaning from language such as the following:—"And from each little dot in these 'clouds of life' a separate vorticella can be seen to develop! It is here, indeed, at this first *visible* advent or exordium of animate life, and the resurrection of millions of germs through the spontaneous dissolution of a single one, that the last nubecular *microscopic* perceptions closely resemble the last nebular telescopic as well as the *theoretic* ones of Laplace's cosmogony." The concluding portion of this paper, which is reprinted from *Silliman's Journal*, appears in the succeeding number, and is interesting as a contribution to the "curiosities of scientific literature."

THE *Monthly Microscopical Journal*, No. 36, December 1871.—"Notes of Prof. James Clark's Flagellate Infusoria, with Descriptions of New Species," by W. Saville Kent, F.Z.S. An entirely technical paper, consisting of the diagnostic characters of new species, with those of previously-described ones amended. Eleven forms are figured and described, all of which were found in fresh water at Stoke Newington.—"On Bog Mosses," by R. Braithwaite, M.D., F.L.S., Part II., is occupied chiefly with the anatomy of the leaf and development of the plant.—"On the Conjugation of Amœba," by J. G. Tatem, is a note serving to strengthen the supposition previously advanced by this author, "that these large Amœbæ so frequently met with in the autumn months are actually the incorporation of two individuals in a copulative act," from which free-swimming ciliated germs might eventually issue. "On the Connection of Nerves and Chromoblasts," by M. Georges Pouchet. The inference drawn from an examination of the pectoral fin of a young flat-fish is that there is a reality of connection between the nervous and sarcodic elements, but that the nature of this connection is unknown.

THE *Revue Scientifique*, Nos. 19—25, contains, among others, the following articles, translations, and reprints:—General Morin's eulogy on Piobert and his inventions in artillery; Dr. Carpenter's lectures at the Royal Institution; the continuation of Grehant's course of lectures on Experimental Physiology; M. Lorain on primary and secondary instruction in France; Berthelot on the union of alcohols with bases, and on the history of carbon; Moleschott on the regulators of human life; Saussure on the life and works of Claparède; Valentin on the electric properties of nerves during embryonic life, and during putrid decomposition; a summary of the most important papers read at the Bologna International Congress of Anthropology and Prehistoric Archæology; Contejean on the origin of sedimentary deposits; Mr. Bentham's last anniversary address to the Linnean Society; Fonvielle on aerial navigation; Prof. Huxley's article in the *Contemporary Review* on English Critics of Darwin.

THE twentieth volume (1870) of the *Verhandlungen der k.k. zoologisch-botanischen Gesellschaft in Wien*, although a stout octavo, is hardly equal in bulk or in the variety of its contents to some of its predecessors; nevertheless its readers will find in it an abundant supply of valuable papers on zoological and botanical subjects. As usual, entomological articles are in the majority under the former head, and here Dr. Winnertz leads off with two papers on Diptera, containing descriptions of species belonging to the *Lestreminæ*, a sub-family of Cecidomyidæ, and of the species of *Heteropeza* and *Miastor*—two genera of the same family. Singularly enough these, and a short notice by M. von Bergenstamm on the metamorphoses of *Platypeza holosericea*, are the only papers on Diptera in the volume.—The Lepidoptera also receive but little notice, but on the Rhynchota we have some important papers:—M. P. M. V. Gredler furnishes a list, with notes, of the Heteropterous Rhynchota of the Tyrol, and Dr. F. X. Fieber the characters of twelve new genera and twelve new species of the same group. The forms described by the latter are from various parts of Southern Europe.—M. C. Tschek describes a number of Austrian Ichneumonidæ belonging to the group of the Cryptoides, Dr. G. Mayr a number of new species of ants, and Dr. J. Kriechbaumer four new South European species of humble bees.—A paper on the Orthoptera of the Syrmian valley in Hungary by M. V. Graber, which includes an interesting description of the district, is the only other entomological paper to which we shall refer.—The malacologist will find a list of the land and freshwater mollusca of Galicia by Dr J. Jachno, a monograph of the genera *Emmericia* and *Fossarulus* by M. Brusina, and an important paper on the anatomy of *Tribonophorus* and *Philomycus*—two forms of naked Pulmonata; whilst for the ichthyologist we have the first part of a descriptive synopsis of the fishes of the Red Sea from Dr. C. B. Klunzinger, who also notices the animals observed upon a coral reef in the Red Sea.—M. D. Dybowski describes a new form of Salamander from Siberia under the name of *Salamandrella Keyserlingii*, and Dr. Burmeister gives a description of the pelvis of *Megatherium*. The botanical papers are to a considerable extent of the nature of local lists, but some of these contain a good deal of descriptive matter. Thus in M. Schulzer von Müggenburg's "Mycological Observations in North Hungary" we find many descriptions of fungi; Glowacki and Arnold's "Lichens from Carniolia" contains descriptions of species, as does also the latter's "Lichenological Excursion into the Tyrol," and the contribution to the moss-flora of East by MM. Juratzka and Milde. M. F. Hazslinsky describes the *Sphæriæ* which are parasitic upon the rose; M. Julius Klein's mycological communications contain a description of a new genus of Mucorine fungi, and of some other forms which grew with its representative; and M. Schulzer von Müggenburg, above-mentioned, has also his mycological contributions, which consist almost entirely of descriptive matter. The papers which treat of the higher forms of plants, and those describing the natural history journeys of their authors, are not numerous. We may mention especially a long paper by M. F. Krasan on the periodical phenomena of vegetable life, and an article by Dr. A. Unterhuber on the position of the scales of the fruit in *Ceratozamia mexicana*. This list of papers will be sufficient to show how much there is in the proceedings of the Vienna Zoologico-Botanical Society to interest both the zoologist and the botanist.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, December 6.—Mr. J. Prestwich, president, in the chair. Prof. Giovanni Capellini, of Bologna, was elected a Foreign Correspondent of the Society. 1. "On the presence of a raised beach on Portsdown Hill, near Portsmouth, and on the occurrence of a Flint Implement at Downton." By Mr. Joseph Prestwich, F.R.S., President. The author noticed a section observed by him in a pit ten miles westward of Bourne Common and five miles inland in a lane on the north side of East Cams Wood. It is situated at an elevation of 300 feet above the sea level, and shows some laminated sands with seams of shingle, overlying coarse flint-shingle with a few whole flints, which the author regarded as a westward continuation of the old sea-beach which has been traced from Brighton, past Chichester, to Bourne Common. A flint flake was found by the author at the bottom of the superficial soil in this pit. The author also noticed the occurrence of a flint implement of the type of those of St. Acheul in a gravel near Downton in Hampshire. This gravel capped a small chalk-pit, and its elevation above the River Avon was about 150 feet. Two gravel terraces occur between this pit and the river, one 40 by 60 the other 80 by 110 feet above the level of the latter. Mr. Codrington stated that, according to the Ordnance Survey, the level of the pit at Cams Wood was not more than 100 feet above the sea, so that it was at about the same level as the gravels of Titchfield and elsewhere. Mr. Evans remarked that the flint flake from Cams Wood presented no characters such as would prove it to be of Palæolithic age. He was, on the contrary, inclined to regard it as having been derived from the surface. He commented on the height at which the Downton implement had been discovered, which was, however, not so great but that the containing gravels might be of fluviatile origin. Mr. Gwyn Jeffreys thought that if the beds at Cams Wood were marine, some testaceous remains might be found in them. If these were absent, he should rather be inclined to regard them as fluviatile. Mr. J. W. Flower contended that the gravel at Downton could not be of fluviatile origin. He thought, indeed, that the gravel was actually at a higher level than the present source of the river. If this were so, he maintained that the transport of the gravel by fluviatile action was impossible. He further observed that gravels precisely similar, also containing implements, had now been found, as well in the Hampshire area as elsewhere, the transport of which, in his view, could not possibly be attributed to any existing rivers. At Southampton they occur 150 feet above the River Itchen and the sea, and considerably inland; at Bournemouth, on a sea cliff 120 feet in height; and at the Foreland (at the eastern extremity of the Isle of Wight), on a cliff 82 feet above the sea, and far remote from any river. If, therefore, these deposits were effected by fluviatile agency, it was evident that all traces of the rivers were afterwards effaced by some great geological changes, or, in the alternative, some great geological change, not fluviatile, must have caused the deposit. Upon the whole he was disposed to conclude with the French geologists as well as with many eminent English authors that the accumulation of all these superficial drifts was, as the late Sir Roderick Murchison had said, sudden and tumultuous, not of long continuance; and thus it was such as would result from some kind of diluvial action, rather than from the ordinary long-continued action of water. Mr. Judd pointed out, in contravention to Mr. Jeffreys' views, that in the Fen district, over large tracts of deposits of undoubtedly marine origin, not a trace of marine shells could be found. Mr. Prestwich, while willing to concede that the implement-bearing gravel-beds had been deposited under more tumultuous action than that due to rivers of the present day, was still forced to attribute the excavation of the existing valleys and the formation of terraces along their slopes to river-action. He showed that Mr. Flower's argument as to the present level of the source of the river was of no weight, as the country in which it had its source was formerly, as now, at a much higher level than the gravel at Downton. As to the absence of marine shells at Cams Wood, he cited a raised beach in Cornwall which, in company with Mr. Jeffreys, he had examined for a mile without finding a trace of a shell, though for the next half-mile they abounded. There was the same difference between the raised beach at Brighton and at Chichester. He was obliged to Mr. Codrington for his correction as to the level at Cams Wood, though the pit was at a higher elevation than the one to which Mr. Codrington had alluded.—2. "On some undescribed Fossils from the 'Menevian Group of Wales.'" By Mr. H. Hicks. In this communication the author gave descriptions of all the fossils hitherto undescribed from the Menevian rocks of Wales. The additions made to the fauna of the Lower Cambrian rocks (Longmynd and Menevian groups) by the author's researches in Wales during the last few years now number about fifty species, belonging to twenty-two genera, as follows:—Trilobites, 10 genera and 30 species; Bivalved and other Crustaceans, 3 genera and 4 species; Brachiopods, 4 genera and 6 species; Pteropods, 3 genera and 6 species; Sponges, 1 genus and 4 species; Cystideans, 1 genus and 1 species. By adding to these the Annelids, which are plentiful also in these rocks, we get seven great groups represented in this fauna, the earliest known at present in this country. By referring to the Tables published in M. Barrande's excellent new work on Trilobites, it will be seen that this country also has produced a greater variety, or, rather, representatives of a greater number of groups from these early rocks than any other country. The species described included *Agnostus*, 5 species; *Arionellus*, 1 species; *Erinnys*, 1 species; *Holocephalina*, 1 species; *Conocoryphe*, 2 species; *Anopolenus*, 2 species; *Cyrtotheca*, 1 species; *Stenotheca*, 1 species; *Theca*, 2 species; *Protocystites*, 1 species, &c. The author also entered into a consideration of the range of the genera and species in these early rocks, and showed that, with the exception of the Brachiopods, Sponges, and the smaller Crustacea, the range was very limited. A description of the various beds forming the Cambrian rocks of St. David's was also given, and proofs adduced to show that frequent oscillations of the sea-bottom took place at this early period, and that the barrenness of some portions of the strata, and the richness of other parts, were mainly attributable to these frequent changes. Mr. Gwyn Jeffreys suggested that the term Polyzoa might be adopted in preference to that of Bryozoa, as being the more ancient term, and that the name *Proserpina* should not be applied to the new genus of Trilobites, as it had already been appropriated to a tropical form of land-shell.

Royal Geographical Society, December 11.—Major-Gen. Sir H. C. Rawlinson, president, in the chair.—A paper was read by Mr. Keith Johnston, "On the Rev. Thomas Wakefield's Map of Eastern Africa;" the subject being limited to the form of Speke's Lake Victoria Nyanza, which Wakefield's native travellers had decided to consist of at least two lakes.—Capt. R. F. Burton followed with a paper on "Lake Ukara or Ukarewe," in which he argued from the new information gleaned by Mr. Wakefield at Mombaz, and Captain Speke's own data, that Victoria Nyanza consisted of many separate lakes, and that it was a "Lake Region," and not a single lake.

Sunday Lecture Society, December 17.—"On the Optical Construction of the Eye," by Dr. Dudgeon. The early part of the lecture was occupied with a description of the optical construction of the eye. In order to ascertain the precise focal length of aqueous humour, the lecturer immersed his eyes in water, which, being of the same refractive power as the aqueous humour, extinguishes it as a lens. He then ascertained what power of lens was required to restore perfect vision under water, which he found to be affected by an artificial lens, whose focus was exactly 1½ inch under water. He constructed a pair of spectacles fitted with air lenses, formed by very concave watch-glasses placed back to back, and united round their edges by a ring of wood or vulcanite. In this way he formed air lenses which had a focus of 1½ inch in water, but which offered no obstruction to vision in the air. With these spectacles perfect vision both for near and distant objects below the water was obtained, and on coming to the surface these spectacles allowed of perfect vision in the air. He then explained the construction of the eyes of fishes and amphibia, which have no anterior aqueous lens, but only a nearly spherical crystalline lens. He next explained the mechanism of the accommodation of the eye from distant to near vision. He showed that this was not effected by any increase of the convexity of the anterior surface of the crystalline lens, as is generally supposed, but by a slight rotation of the crystalline lens from without inwards, whereby the focus of the crystalline lens was shortened to the degree necessary to throw the image of a near object accurately on the retina. Finally, he pointed out that some of the principal discoveries of modern physicists already existed in the eye. Thus, the principle of achromatic lenses by the combination of two lenses of different refractive power was seen in the eye when a water lens was combined with the crystalline lens; the discovery of Descartes, that an elliptical surface of a lens obviated spherical aberration, was also found in the eye; and Herschel's discovery that a combination of the meniscus with the double convex lens prevented spherical aberration also obtained in the eye.

Photographic Society, December 12.—A paper was read by Lieut. Abney, R.E., F.R.A.S., on albumen applied to photography. He first referred to the use of albumen as a substratum for collodion films. Taking different proportions of albumen and water, and iodising part of each, he found that with the best collodion process the iodised substratum as a whole gave neither increase nor diminution of sensitiveness, whilst with the uniodised substratum the sensitiveness was slightly diminished. He next pointed out the cause of blisters in developing dry plates, and traced them to the expansion of the albumen; the substratum rising from the glass at the smoother portions. He lastly touched upon the uncombined sulphur always present in albumen, as much as 1·2 grains being found in a whole sheet of paper, whilst but ⅔ grain of metallic silver was found in prints of the same area. He argued from this that silver prints must fade, apart from the imperfect washing, unless the sulphur be removed. He recommended the makers of albumenised paper to try to do this, first forming albumenate of potash by the addition of potash to the albumen. The unprecipitated part contained the sulphur. This might be removed and the albumen once more dissolved by the addition of acid.—A paper on M. Dagrou's microphotographic despatches was also read, detailing the methods of preparation; as many as 50,000 messages were received in Paris during the Siege upon these films, conveyed to the capital by pigeons.

MANCHESTER

Literary and Philosophical Society, November 14.—E. W. Binney, F.R.S., president, in the chair. The president said that, on Friday the 10th inst., he observed at Douglas in the Isle of Man, a splendid display of the aurora borealis. At 8 P.M. it appeared as an arch of a greenish colour, extending from west to east, through the tail of the Great Bear. Afterwards, at ten o'clock, the same kind of arch was observed with another higher up, which ranged west and east through the Pole star. At this time numerous streamers and flashes of light of a green and yellowish-white colour flashed up from near the horizon to the zenith, from east, south, and west; those towards the west had a reddish hue. The sky was beautifully clear, and the light from the aurora was greater than ever previously observed by him.—"On the Origin of our Domestic Breeds of Cattle," by William Boyd Dawkins, F.R.S. There are at the present time three well-marked forms inhabiting Great Britain. 1. The hornless cattle, which have lost the horns which their ancestors possessed through the selection of the breeder. The polled Galloway cattle, for instance, are the result of the care taken by the grandfather of the present Earl of Selkirk, in only breeding from bulls with the shortest horns. The hornless is altogether an artificial form, and may be developed in any breed. 2. The *Bos longifrons*, or the small black or dark brown Welsh and Scotch cattle, which are remarkable for their short horns and the delicacy of their build. 3. The red and white variegated cattle, descended from the urus, and which have on the whole far larger horns. These two breed freely together, and consequently it is difficult to refer some strains to their exact parentage. The large domestic cattle of the urus type are represented in their ancient purity by the Chillingham wild oxen, as they are generally termed, but the exact agreement of their colour with that specified in the laws of Howel Dha proves that they are descended from an ancient cream-coloured domestic ox with red ears. The animal was introduced by the English invaders of Roman Britain, and was unknown in our country during the Roman occupation. The *Bos longifrons*, on the other hand, was the sole ox which was domestic in Britain during the Roman occupation, and in the remote times out of the reach of history it was kept in herds by the users of bronze, and before that by the users of polished stone. This is proved conclusively by the accumulations of bones in the dwelling-places and the tombs of those long-forgotten races of men. The present distribution of the two breeds agrees almost exactly with the areas occupied by the Celtic population and the German or Teutonic invaders. The larger or domestic urus extends throughout the low and fertile country, and indeed through all the regions which were occupied by Angle, Jute, Saxon, or Dane; while the smaller *Bos longifrons* is to be found only in those broken and rugged regions in which the unhappy Roman provincials were able to make a stand against their ruthless enemies. The distribution, therefore, of the two animals corroborates the truth of the view taken by Mr. Freeman, that the conquest of Britain by the

English was not a mere invasion of one race by another, but as complete a dispossession as could possibly be imagined. The *Bos longifrons* lingers in Wales, after having once occupied the whole country, just as its Celtic owners still linger, while the urus is an invader just in the same sense as their English possessors. The *Bos longifrons* is of a stock foreign to Europe, and the urus was most probably domesticated in some other region by those Neolithic people. Both these animals have probably been derived from an area to the south and east of Europe, and were introduced by the Neolithic herdsmen and farmers at a very remote period.

Dublin

Royal Dublin Society, November 20.—Prof. R. Ball, M.A., in the chair. Mr. Maurice Cole exhibited and explained a working model of an improved seed sowing machine.—Prof. Edward Hull, F.R.S., read some notes of a recent visit to Vesuvius.—Dr Emerson Reynolds exhibited a new apparatus for gas analysis, and Mr. A. G. More exhibited some specimens of well-stuffed birds from the museum of the Society.

Royal Irish Academy, November 30.—Rev. J. H. Jellett, president, in the chair. The Secretary read a paper by M. Donovan on Earl Stanhope's alleged imperfections of the tuning fork; also for Dr. Whitley Stokes a paper on a fragment of Cormac's glossary.—Mr. G. H. Kinahan read a paper on and exhibited sketches of what appeared to him a new type of Clochán, observed in the county of Mayo, South of Louisburgh. The structure was composed of large flags inclining inwards to form sloping sides and roof, the very apex of which was covered by horizontal flags. He also exhibited a sketch of a form of cross observed in the same neighbourhood, and which was unlike anything he had ever seen.

Paris

Academy of Sciences, December 11.—M. J. Boussinesq read a paper on a remarkable property of the points where the lines of greatest slope of a surface have their osculatory planes vertical, and on the difference which generally exists at the surface of the earth between the lines of the ridge or the thalweg, and those along which the slope of the soil is a minimum.—M. Becquerel presented a third memoir on the discoloration of flowers by electricity, and on the cause of the phenomenon, in which he shows that electricity acts in this case by destroying the envelopes of the cells containing the coloured materials. Heat produces the same effect. The author remarked upon some general applications of these facts.—A paper on the diffusion and deleterious influence of mercurial vapours, by M. Merget, was read. The author disputed the conclusions of Faraday, founding his opposition upon experiments and observations which show that the vaporisation of mercury is a continuous phenomenon not even interrupted by the solidification of the metal, and that the vapours emitted by it are capable of great diffusion, nearly in accordance with the dynamic theory of gases. M. Dumas called attention to some observations on this subject by M. Boussingault.—M. C. A. Valson presented a note on the part played by space in the phenomena of solution, in which he discussed the contraction produced by the solution of various salts in water.—A note on different acoustic phenomena observed during balloon-ascents, by M. W. de Fonvielle, was read. The author remarked upon the fact that certain acute but very feeble sounds are often heard in balloon ascents, and accounts for the phenomenon by the reverberation of the balloon itself.—M. Serret presented a note by M. de Tastes on a new propeller, consisting of a plate or fan worked in the manner of the tail of a fish or whale. M. A. Barthélemy presented a memoir on the vibrations communicated to mercury and liquids in general, in which he described and figured the curious effects produced by these vibrations in vessels of various forms.—M. Delaunay read a note on the cold of the 9th December, containing some interesting observations on the range of this extreme cold over the Continent of Europe; and M. C. Sainte-Claire Deville presented a second note on the precocity of the cold in the present year.—M. P. P. Dehérain presented a memoir on the intervention of the nitrogen of the atmosphere in vegetation, in which he demonstrated by experiment the absorption of the atmospheric nitrogen by decomposing organic matters, and suggested that by this means nitrogen may be absorbed by the soil.—M. Wurtz presented a note by MM. C. Friedel and R. D. Sylva, on the action of chlorine upon chloride of isopropyl; and a note by M. E. Grimaux on derivatives of chloride of tollylene.—A note was read by M. Dubrunfaut on the combustibility of carbon, in which he maintains that carbon is combustible only in gases containing water; and another by M. F. Jean on the quantitative determination of glucose, recommending a process depending on the precipitation of metallic silver by protochloride of copper, prepared from the protoxide precipitated by glucose.—The deposits of phosphate of lime in France formed the subject of three papers, namely, a note on the composition of that recently worked in the Departments of Tarn-et-Garonne and of the Lot, by M. A. Bobière; an account of the deposits of Saint-Antonin and Caylux, in the former department, by M. Trutat; and a short note on the organic origin of the deposits in the Quercy, by M. Malinowski. M. Trutat described the structure of the deposits, and noticed the remains of certain mammalia found in them.—M. Daubrée communicated a note by M. P. Fischer on the existence of Lower Tertiary strata in Madagascar. These beds, belonging apparently to the great Nummulitic formation, occur on the west and south-west coast of the island. No nummulites have been found in them.—M. E. Blanchard presented a note by M. A. Milne-Edwards on the structure of the placenta in the Tamandua. The author describes this placenta as differing in various respects from those of other Edentata, and remarked that the diversity in the fœtal envelopes of those mammals would lead to the supposition that either the characters derived from them are not so important among the Edentata as in other groups, or the forms united in the Edentata are less nearly related than is generally supposed. He is inclined to the latter opinion.—M. Duchartre communicated a note by M. J. de Seynes on *Penicillium bicolor*, Fr.; and M. Robin presented a note by M. Rabuteau on the physiological properties of various chlorides.

BOOKS RECEIVED

English.—Nature; or, the Poetry of Earth and Sea: From the French of Madame Michelet (T. Nelson and Sons).—The Mountain: From the French of J. Michelet (T. Nelson and Sons).—Beautiful Birds in Far-off Lands: M. and E. Kirby (T. Nelson and Sons).—Text Books of Science; Theory of Heat: J. Clerk Maxwell (Longmans).—A Manual of Zoology: H. A. Nicholson; 2nd edition (Blackwood).—Comparative Metaphysics; Part II.: S. H. Hennell (Trübner).

Foreign.—(Through Williams and Norgate)—Handbuch der vergleichenden Anatomie: E. O. Schmidt.—Mineralogische Mittheilungen, Jahrg. I., Heft 1: G Tschermak.

DIARY

THURSDAY, December 21.

Royal Society, at 8.30.—Contributions to the History of Orcin. No. II. Chlorine and Bromine Substitution Compounds of the Orcins; Note on Fuedsol: Dr. Stenhouse, F.R.S.—On some recent Discoveries in Solar Physics: and on a Law regulating the Duration of the Sunspot Period: W. De La Rue, F.R.S., B. Stewart, F.R.S., and B. Loewy.

Linnean Society, at 8.—On the Anatomy of the American King-Crab (*Limulus polyphemus*, Latr.): Prof. Owen, F.R.S.

Chemical Society, at 8.

London Institution, at 4.—The Philosophy of Magic. 1. The Magic of Modern Conjurers: J. C. Brough, F.C.S.

FRIDAY, December 22.

Quekett Microscopical Club, at 8.

THURSDAY, December 28.

Royal Institution, at 3.—On Ice, Water, Vapour, and Air. No. I. Prof. John Tyndall, F.R.S.

London Institution, at 4.—The Philosophy of Magic. 2. The Magic of the Theatre: J. C. Brough, F.C.S.

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Errata.—P. 123, col. 2, line 36 from top, for "or D1 . . . or D2," read "on D1 . . . on D2."

THURSDAY, DECEMBER 28, 1871

TECHNICAL EDUCATION IN HOUSE CONSTRUCTION

REFERRING to the recent sad events at Londesborough Lodge, and the disclosures made in the medical press, showing how the whole internal air of this house was tainted with sewer gas for want of ordinary care, the *Times*, in an able article which appeared on December 9, has the following telling passage : " What a satire on the universal diffusion of knowledge, on the lectures of the Royal Society, on hundreds of scientific and educational institutions, and all our new inventions and discoveries ! Here is the simplest thing in the whole world, which wanted only common sense, and nobody seems to have thought of it—nay, we are not sure that our architects and builders will be thinking of it next year. It is far too simple and too deadly an affair."

We purpose to deal with this subject ; and in doing so to show briefly how it is that with every apparent advantage our houses are still not altogether safe to live in.

In the first place, the whole subject of house-drainage has been thoroughly discussed, and simple rules have been laid down, which any one with ordinary technical skill can apply to any conceivable case.

The question has been treated in published reports by the Health of Towns' Commission, by the Metropolitan Sanitary Commission, by the General Board of Health, by the Barrack and Hospital Improvement Commission, by the Local Government Act Office, and recently, by the Army Sanitary Commission, for application in India. These official documents, extending over a period of nearly thirty years, contain all the principles on which wholesome house-conveniences can be constructed ; and besides all this, engineering, architectural, and medical journals have never ceased to advocate attention to the requirements of healthy house construction. The Legislature, on its side, has been anxiously engaged in defining and granting every necessary power for the efficient carrying out of town-drainage works ; but hitherto these powers have stopped short with the house drain. All between the head of the house drain and the interior of our bedrooms has been left to chance, or to the imperfect knowledge or no knowledge of such officials as we have seen defending the deadly arrangements of existing houses, or to plumbers' journeymen or apprentices. The whole experience shows that every official has considered his duty fulfilled when he had ensured an outlet for the refuse water of the house. As to the subsidiary traps, and such like things, they have been introduced without regard to scientific considerations ; so that, instead of proving an advantage, they have, in some instances, increased the evil. Now, it must henceforth be recognised that house drainage is not a question of hydraulics merely, it is in a higher sense a question of pneumatics ; but even in this extended sense it is far from being a difficult art, as some would have us suppose. It is by no means a " refuge of despair," as some have asserted. It is a great and beneficial necessity. Because carelessly-fitted water-pipes are burst by frost, and our houses are deluged every winter, are we to have a crusade against water supply ? Bursting of water-pipes and the influx of foul air from sewers are indications of want of ordinary common sense ; or, at all events, of very ordinary technical skill. And the real future question before us, is not whether we are to abolish household drainage and water supply, but whether some public control in these matters ought not to be exercised over the proceedings of plumbers' apprentices and other similar persons, so that when we rent or buy a house, we may be assured that typhoid fever, or some other pestilence, is not included in the contract. Every such contract should, however, ensure three things, viz., that water-pipes are protected from frost ; that the house is thoroughly drained ; and that no sewer-air can, under any circumstances, enter the house. Now all these things can be assured.

It is a mere truism to say that there are plenty of non-conductors of heat with which water-pipes can be efficiently surrounded. Why should water-pipes be left uncovered under flooring or in walls, as at present ? Surely any local authority could deal with so simple, and, at the same time, so important a question as this.

As regards efficient drainage-pipes, traps, and the like, there are great manufacturing interests involved in the production of these, and any one who will cast an eye over the advertising columns of our architectural and engineering contemporaries, will see how much ingenuity and wholesome competition there exists in the production of the most scientific forms of apparatus of this class. But the missing link in the whole of these drainage arrangements is how to prevent foul air entering the house. In an ordinary second or third-class house in London, there are three or four water-closets, the main pipe from which enters the drain, either directly or through an inefficient trap. It may be safely stated that at all times there is more or less pressure of sewer air on the pan or trap of the closet, which must lead to an infiltration of foul air into the house. But nobody appears to have applied the long-known remedy for this, viz., to take off the pressure by a small leaden pipe carried from the upper end of the soil-pipe to the open air.

It is not, however, from the soil-pipe that most of the danger arises. Houses of the same classes have generally what is called a safe under the water-closet, from which safe a pipe passes directly to the drain. Next there may be a bath with its outlet pipe, its overflow, and the pipe of its safe, all connected with the drain. There may be three or even four sinks all connected with the drain, and then every cistern has its overflow, also connected with the drain. As these various open pipes are distributed all over the house, we can easily understand how, while fulfilling the function of removing waste water, they may, in conformity with the laws of pneumatics, distribute the most deadly poison among the unconscious sleeping inmates of every bed-room.

Foul sewer air returns into a house for the following reasons, viz. :—1. A wind-pressure exercised on the open mouth of a sewer perhaps many miles away ; or a similar pressure exerted on an ordinary gulley grate. 2. By pressure of foul air into the house from the superior specific gravity of the atmosphere outside. 3. The draft of chimneys, when doors and windows are shut, as during the night. This draft must be supplied, and will supply itself from every one of these small pipes, perhaps a dozen or more in a house, if it cannot be supplied more easily elsewhere.

The principles to be kept in view in dealing with defects such as those stated are obvious enough.

The general drain system of every street or district should be studied as regards its pneumatic relations, and means should be adopted for relieving the pressure within the system by ventilating outlets in safe positions. By placing charcoal strainers at all these outlets, sewer air would be deprived of its destructive qualities before passing into the streets. In special cases provision would have to be made for preventing the tide or strong winds from entering the mouth of the main sewer.

Then as regards the household drains. There is nothing easier than to ventilate the soil-pipes through charcoal filters in the manner stated. And as regards the numerous small pipes of sinks, baths, &c., not one of these ought on any account to communicate directly with a sewer. They ought all to be collected and allowed to discharge their contents in the open air over a trap communicating with the house drain, so that reflux of sewer gas into the house would be simply impossible.

Much evil has in times past arisen from imperfect drains within houses. Properly there should be none such. All connections of water-closets, sinks, baths, &c., with the house drain, should take place outside the house walls, and where from bad construction drains have been laid within houses and cannot be altered, they should be replaced by glazed earthen pipes laid in concrete, every joint made perfectly air-tight.

Cess-pits and traps ought never to be permitted within walls. The trapping should be all outside.

From want of attention to these long-known principles most of our houses are sick, and require separate diagnosis and treatment. They can all be cured if we only could find an authority to undertake the cure.

Were it not that in many instances we should have men of straw to deal with, we should feel disposed to advocate the application of Lord Campbell's Act to these cases. But as the recovery of damages would be a remote contingency, why should not Local Boards of Health, with their highly-paid health officers and surveyors, be required to see not only that all the details of water supply and drainage in new houses are safe, but that unsafe houses are made safe by their proprietors, or condemned as unfit for habitation?

After all is done, however, the chief remedy must be sought in technical training on all house questions in the application of which scientific principles are involved.

It may be safely stated that there is no technical subject of greater importance than this, and our recent experience has shown that there is no subject on which more training is necessary than to build a comfortable healthy dwelling.

SUTTON'S VOLUMETRIC ANALYSIS

Volumetric Analysis. By F. Sutton. Second Edition. (London: J. and A. Churchill.)

THE present volume is almost the only representative of a considerable branch of chemistry. We are surprised that Volumetric Analysis has not come into more general use amongst chemists, for the saving of time in most instances is very great, whilst for accuracy it frequently surpasses gravimetric analysis. Since the last edition of this work was published (1863), chemistry has made great advances; in volumetric analysis there has been a gradual extension and development, although nothing very new or startling has taken place during this period. This edition is a far more handsome volume than the last, the type and engravings being everything that can be desired. The author states in his preface that the new system of atomic weights has been adopted; the nomenclature also has been changed to a great extent, although we are sorry to find that the system adopted is by no means perfect. Thus we read of "the carbonates of lime, baryta, and strontian" (p. 26), whilst in a later part of the book such terms as "hydric chloride," &c., are met with. These of course are extreme cases; would it not have been better to have adopted some definite system throughout the book? We regret to say that the larger portion of the book is disfigured by a great number of small errors; for instance, the cross references in many cases are wrong, thus at page 80, the reader is referred to § 80, 2, for the determination of chlorides by Liebig's method, the paragraph referred to is an article "on the examination of raw phosphates and phosphatic manures." Again, we are told on p. 116 to refer to § 71 for the titration of phosphate, but this paragraph describes the estimation of sulphuretted hydrogen. We have noticed so many errors, some in formulæ, some in equations, and again in grammar, that, though making every allowance for printer's errors, we must conclude that the edition has been carelessly revised. There is one paragraph we should wish to call attention to, the first on p. 132, which we confess we have not been able to understand clearly. The number of new processes introduced is not large, nor are they of very great importance. We think, however, that methods such as the estimation of nitric acid by indigo might have been omitted, and that, for instance, the iron process for phosphoric acid might have been introduced. If Mr. Sutton would give, as far as possible, the precise cases for which each process is most suitable, we think the value of the book would be much increased. His long experience in these matters would render this addition of great importance, and would save much trouble.

Fifty-four pages of the volume are occupied by a description of the processes of water analysis (furnished by Mr. W. Thorp); this consists of a lengthy description of Frankland and Armstrong's process, which has undergone considerable modification, and a much shorter description of Wanklyn and Chapman's process. We look upon this part of the book as very valuable, for water analysis has now become quite a study, and such a clear and concise statement as that in the present volume will be found of great service to any one engaged in this work.

The last section of the book, consisting of seventy-four pages, is "On the Volumetric Analysis of Gases," contributed by Prof. H. McLeod. We cannot praise this portion of the volume too highly, the engravings are excellent, many of them we believe being from the original drawings of the author. We do not think that any student could do better than take this as his guide to gas analysis. It is the most clearly written and practical account that we have seen in the English language, and we should be glad to see it still further extended by the author.

On the whole Sutton's "Volumetric Analysis" has certainly improved on the first edition, but with more care its value would have been much increased.

MORELET'S TRAVELS IN CENTRAL AMERICA

Travels in Central America, including Accounts of some Regions unexplored since the Conquest; from the French of the Chevalier Arthur Morelet. By Mrs. M. F. Squier. Introduction and Notes by E. G. Squier. (London: Trübner and Co., 1871.)

IN that portion of Central America which lies between Yucatan on the north and the city of Guatemala to the south, and bounded on the east by British Honduras, is a considerable tract of country which has remained almost unknown to Europeans since the Spanish conquest, and in which the traditions of the neighbouring States place vast aboriginal cities and wonderful enchanted lakes. To explore this region was the object of the adventurous expedition of M. Arthur Morelet, a French gentleman of leisure and extensive scientific acquirements. M. Morelet's natural history collections were deposited in the Museum of Paris, and described in the *Comptes Rendus* of the Institute; a new crocodile was named after him (which he pathetically declares to be the only result of the journey as far as fame to himself is concerned), and an account of his travels was printed for private circulation in his own country. In the volume before us a portion of this is now translated for the benefit of the American and English public. Although the work records no important or striking discoveries, it is a valuable and interesting contribution to the geography and natural history of an almost unknown district.

M. Morelet's journey was divided into two portions. The first was devoted to a visit to the ruins of the ancient city of Palenque, near the great river Usumasinta, in the western portion of the district. The existence of these ruins was not known till 1750, but they have been sufficiently described in the works of Dupaix, Stephens, and others. Notwithstanding the traditions of immemorial antiquity which hang around them, the author attributes their origin to the Toltecs, who, in the middle of the 7th century were in possession of Anahuac, where civilisation peaceably developed itself. Later, about the year 1052, they abandoned this region, and emigrated in a south-easterly direction, that is to say, into the provinces of Oaxaca and Chiapa. It is easy enough, therefore, he thinks, to arrive at the conclusion that Palenque was founded at this time, and was consequently contemporaneous with Mitla.

The second and more important portion of M. Morelet's expedition had for its special object a visit to the great lake of Itza, situated in the province of Peten. Although nominally within the territory of the Republic of Guatemala, and but a comparatively short distance from the British settlement of Belize, he was unable to obtain at any of the seaport towns of Yucatan any exact information as to the exact locality of, or the means of access to, this mysterious region. Proceeding from Palenque up the Usumasinta River, his route then lay eastwards for upwards of a fortnight through virgin forests of great magnificence, abounding in insects of all kinds, and in many rare and curious birds, and with a floral vegetation of great interest and beauty. The author describes in particular the *Aristolochia grandiflora*, with a flower often not less than twelve to fifteen inches in diameter, the calyx resembling the figure of a swan suspended by its bill, but when full-blown assuming the form of the conventional cap of liberty, turned up with a violet velvet lining, and worn by the Indian children as a helmet.

The great lake variously referred to by chroniclers as that of Itza, of the Lacandones, and of Peten, is described by M. Morelet as having a circumference of upwards of twenty-six leagues, and a depth in most cases exceeding thirty fathoms. It is not fed by any river, or even brook, of importance, and has no outlet; how its waters are kept fresh is not described. Its shores are defined by a girdle of broken calcareous hills, which are more or less silicious. On an island situated near its south-western shore is the Indian town of Flores, the only one of importance in this vast, almost uninhabited, district. Its description, and the illustration, convey an idea of great beauty:—

"I was impressed with the magnificence of the landscape which presented itself from the eminence where the modern church is situated, and which was once occupied by the ancient temples of the Itzaes. The sky was clear, the waters of the lake of the loveliest azure, and the islands and bluff shores, indented with little bays, hemmed in by silvery belts of sand, were green and refreshing to the sight. The island of Peten itself is oval in shape, rising by a gentle slope from the water, and terminating in a platform of calcareous rocks. It is not large; one may make the circuit of it in a quarter of an hour. Its surface is covered with small stones, which are doubtless the remains of ancient edifices."

The necessaries of life, both as to food and clothing, being very few in number, the inhabitants of Flores have little inducement to labour, and pass their days in luxurious idleness or nocturnal festivities, and their character is what might be expected from their habits of voluptuous ease, though without any strongly developed vices. As to the natural history of the district, the author describes as the most abundant mammalia three species of deer, the tapir, the peccary, a species of rabbit, an armadillo, the agouti, which commits great ravages on the crops, and several rodents. Among the birds he mentions particularly a small heron (*Ardea exilis*), two swallows, and a humming bird. Among the reptiles are a number of species hitherto undescribed, including a new turtle (*Emys areolata*) and the *Crocodilus Moreleti*, the capture of which nearly cost him his life. There are fifteen different kinds of fish in the Lake of Itza, which are almost without exception peculiar to it. Considering the isolation of the lake from all other water systems, this fact is of great interest to the student of the geographical distribution of animals, and of the origin of species. The flora is not described in detail, indeed throughout the book few plants are specifically named, unless of striking beauty or producing edible fruits. A suspicion of the accuracy of the author's knowledge of natural history is excited by the occurrence of such phrases, unless they be due to incorrect translation, as "invertebræ (*sic*) *and* insects," speaking of a gasteropod as a "shell-fish," and describing the *Tillandsia* as "a variety of moss." Another serious defect in the book is that the map which accompanies it does not correspond with the text in the spelling of the names, nor always even in the natural features of the country.

From Flores M. Morelet proceeded in a southerly direction to the City of Guatemala, passing along the watershed which separates the streams flowing into Honduras Bay on the east from those which find their outlet in the Gulf of Mexico to the west. A halting-place on the route is the station of Campamac, laid down on the maps as a place of some importance, but which he found to consist of "half-a-dozen worm-eaten posts stuck in the ground in the midst of the forest, and supporting a thatched roof; a small clearing in front, and faint traces of a path leading to it in one direction, and from it in another." A little farther south, on approaching the Indian town of Cahabon or Cajabon, the traveller emerges from the dense virgin forests which have clothed the country since he left Flores, and enters on the wide open savannahs which characterise the southern portion of Guatemala. The Indians of this district belong to a different race from the Mayas of Peten; they are of a darker colour, with less regular features and less symmetry of form; with low foreheads, high cheek bones, and the top of the head rising to a point in a manner apparently artificial. The civilisation introduced by the Dominicans appears to be gradually decaying; and European vices, added to their own national indolence, are rapidly reducing their numbers, and deteriorating their character.

The reader will find in M. Morelet's narrative much valuable information as to the manners and customs of the inhabitants of an almost unknown territory, and with regard to the physical features and natural history of a country extremely rich in natural productions; interspersed with those personal incidents and tales of romantic adventure which add so much to the charm of a book of travel.

OUR BOOK SHELF

The Ornithology of Shakespeare. Critically examined, explained, and illustrated. By James Edmund Harting, F.L.S., &c. (London: Van Voorst, 1871.)

THE man who wrote the line, "One touch of Nature makes the whole world kin," demands that some notice should be taken in these columns of any one of his numerous commentators who may attempt to set forth that side of our versatile poet which turns towards natural history. Mr. Harting's attempt is eminently successful. We last met with him (not long since) "on the lone sea-shore," we now find he is equally at home in the library, and if he does not convince us that Shakespeare was a greater ornithologist than has lived since, proof at least is adduced that he was, in his knowledge of birds and their ways, inferior to no one of his time. Books have been written to show that our immortal bard was a soldier, a lawyer, and what not—his reputation as a keen and accurate observer of the feathered race is now fully established. How, indeed, could it be doubted? Did not the "swan of Avon" appreciate "the temple-haunting martlet" and the delicate air which it loved? Did he not "tune his merry note unto the wild bird's throat" while celebrating equally "the clamorous owl that nightly hoots," and "the plain-sung cuckoo grey?" But here we must stop. It is always the reviewer's business ("'tis true, 'tis pity, and pity 'tis, 'tis true") to point out defects. We may mention one. Mr. Harting has forgotten to notice the correct interpretation of the expression "russet-pated choughs," and urges the claim of the jackdaw to be the bird so distinguished. Now, as he truly says, the daw has a grey head, and to make Shakespeare term grey "russet" is, in our eyes, a crime. Without doubt the poet had in his mind the real Cornish chough, and the expression is quite accurate. "Russet-pated" is having red *pattes* or feet (*cf.* the heraldic *croix patée*); not a red *pate* or head—a feature equally inapplicable to chough or daw, while the red feet of the former are as diagnostic as can be. We are bound to say, however, that such a slip as this stands alone. Mr. Harting's book in general is not only readable, but exact and instructive, while its illustrative woodcuts are well chosen, well drawn, and well engraved.

Thoughts on Life-Science. By Edward Thring, M.A. (Benjamin Place), Head-Master of Uppingham School. Second edition; enlarged and revised. (London and New York: Macmillan and Co.)

THE first edition of this book by the accomplished and efficient head-master of Uppingham School appeared with the pseudonym "Benjamin Place" on its title-page; this second and much-enlarged edition bears the author's own name. The title may be apt to mislead some as to the nature of the contents; it is not a work on Biology. The author apparently means by "Life-Science" the science of those phenomena which are the manifestations of the higher kinds of life, as opposed to those sciences which deal with "matter animate and inanimate." "The world open to man's intelligence," he divides into two parts: "On the one side there is matter animate and inanimate, which as matter is capable of material investigation, and which is below man. On the other side there is life as displayed in feeling and thought, and belief founded on the facts of life. The science of this is Life-Science." Mr. Thring believes that man cannot live by science alone; that there is a kind of knowledge, a circle of belief, a region of activity, quite outside and independent of science strictly so-called, and which is of far more importance to the great bulk of humanity than any amount of scientific knowledge. To Mr. Thring, in the present "displacement of traditional ideas, it has seemed no useless task to look steadily at what has happened, to take stock, as it were, of man's gains, and to endeavour, amidst new circumstances, to arrive at some rational estimate of the bearing of things, to examine the instruments and means at our disposal, to examine our strength; so that the limits of what is possible, at all events, may be clearly marked out for ordinary persons." "This book is an endeavour to bring out some of the main facts of the world." Mr. Thring puts forward many statements regarding the inadequacy of language as a vehicle for thought, and on the imperfection of human intelligence itself at the present stage of man's progress, which claim the consideration of all those who are inclined to deny them; and much of what he says, as to the sphere and power of scientific research, deserves to be pondered by all earnest seekers after truth, and, indeed, has almost always been admitted by the highest intellects, who have tried to explore "the great ocean of undiscovered truth." Mr. Thring's style is characterised by a rugged force, and a certain novelty of expression and even of construction, which will render his book interesting to many readers, and which are frequently the outcome of his intense earnestness and the thoroughness of his convictions, as well as of impatience with those intolerant scientific specialists who imagine the little group of phenomena that comes within the ken of their limited vision to be the universe. We heartily commend the book to the attention of our readers.

LETTERS TO THE EDITOR

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

Dr. Carpenter and Dr. Mayer

WITH reference to Dr. Tyndall's communication of last week, in which I most unexpectedly found a private note of my own placed before your readers, I should be obliged by your allowing me to state :—

1. That the idea of "Correlation," as originally entertained by Mr. Grove, and applied by myself to physiology more than twenty years ago, most unquestionably included that of the *quantitative equivalence* of the convertible forces, as will appear from the following passage in my memoir of 1850 (Phil. Trans. p. 731) :—"The idea of correlation also involves that of a certain *definite ratio* between the two forces thus mutually interchangeable, so that the measure of force B, which is excited by a certain exertion of force A, shall, in its turn, give rise to the same measure of force A as that originally in operation" And further I urged the *precise relation* observable between the vital activity of plants and cold-blooded animals, and the amount of heat they receive from external sources, as a ground for the belief that heat has the same relation to the organising force as it has to electricity (pp. 747-750).

2. In crediting Dr. Mayer therefore with the independent (and in my own case the previous) enunciation of the "Correlation" doctrine, I most certainly meant to include the notion of *quantitative equivalence.* Whether the quantities be or be not expressed in number seems to me a matter of secondary importance. WILLIAM B. CARPENTER

University of London, Dec. 26

The "North British Review" and the Origin of Species

THE writer of the article on the "Origin of Species," which was published in the *North British Review* for June 1867, has corrected in your periodical for November 30 an unimportant error which occurs in a certain paragraph of that article. There is, however, it appears to me, a much more serious error in the same paragraph, which vitiates his arithmetical calculations throughout, and leads him to an erroneous conclusion.

The paragraph in which this error occurs is quoted at length in Mr. Mivart's work on "The Genesis of Species." It may therefore be worth while to point out the oversight alluded to.

The error arises from the writer's assuming that in a race which remains constant in numbers, only one individual out of each family, *i.e.*, out of the offspring of one female, will on an average survive to produce young. This assumption is not true ; for since only one half of the race, namely the females, bring forth young, it follows that two out of each family must, on the average, survive to have offspring, namely, one male and one female. Each of these will transmit its peculiarities to its descendants.

I will now quote the writer's words, putting within brackets the necessary corrections.

He says, "A million creatures are born ; 10,000 survive to produce offspring. One of the million has twice as good a chance as any other of surviving ; but the chances are 50 to 1 against the gifted individual being one of the 10,000 survivors." Further on he says, "Let us consider what will be its influence on the main stock if preserved. It will breed and have a progeny of, say 100 ; now this progeny will, on the whole, be intermediate between the average individual and the sport. The odds in favour of one of this generation of the new breed will be, say, $1\frac{1}{2}$ to 1, as compared with the average individual ; the odds in their favour will therefore be less than that of the parent, but owing to their greater number the chances are that about $1\frac{1}{2}$ of them would survive [*about* 3 *of them*, for without any advantage two would on an average survive.] Unless these breed together, a most improbable event, their progeny would again approach the average individual ; there would be 150 [300] of them, and their superiority would be, say in the ratio of $1\frac{1}{4}$ to 1 ; the probability would now be that nearly two [$6 \times \frac{5}{4}$, or nearly 8] of them would survive, and have 200 [750] children with an eighth superiority. Rather more than 2 [15] of these would survive ; but the superiority would again dwindle, until after a few generations it would no longer be observed, and would count for no more in the struggle for life than any of the hundred trifling advantages which occur in the ordinary organs."

The writer thus concludes that the advantage derived by inheritance from the sport will ultimately die out. The true conclusion is, that the advantage never dies out, but only becomes distributed through the whole race ; and, moreover. that the sum of the advantages of all the favoured individuals, when added together, is greater than the original advantage, and becomes greater and greater every successive generation, though it tends to a limit at which it never actually arrives. Thus, representing the original advantage by unity, the advantage in the next generation is $1\frac{1}{2}$, in the next $1\frac{7}{8}$, and so on.

If now the same kind of sport arise independently, (*i.e.* not by inheritance from some previous sport) say once in every generation, and is preserved, say once in every fifty generations, the advantages derived by inheritance from these sports will accumulate and become distributed throughout the whole race. Hence in the course of an immense number of generations they must produce a decided effect upon the character of the race.

Thus, though any favourable sport occurring once, and never again, except by inheritance, will effect scarcely any change in a race, yet that sport, arising independently in different generations, though never more than once in any one generation, may effect a very considerable change. These conclusions are opposed to those which the writer of the article is endeavouring to establish.

Leeds Grammar School A. S. DAVIS

Prof. Tait on Geological Time

AS I have lately found, under the signature of Prof. Tait, in the well-known *Revue Scientifique*, several statements that would doubtless have been challenged had they appeared in any English scientific journal, and of which the following are specimens :—"Sir W. Thomson has already demonstrated, by three complete and independent physical proofs, the impossibility of admitting the existence of such periods"—"Each one (of Sir W. Thomson's arguments) would suffice to upset at once the pretensions of Lyell and Darwin"—"Professor Huxley's attempt has completely failed ;" and as in the new edition of Juke's Geology Sir W. Thomson's demonstration is stated at some length, while an adverse argument used by Jukes is omitted, I venture to ask that you will allow me a few words on the subject, since I treated the matter at length two years ago in *Scientific Opinion*, and, so far as I am aware, my arguments remained unanswered.

1. Does not the conclusiveness of all Sir W. Thomson's arguments depend upon the assumption of the universality of the principle of dissipation of energy? But to assume this is to assume that uniformitarianism is false. The whole question is therefore begged in the premisses, as must be the case in mathematical arguments.

2. As Mayer categorically denies the universality of the said principle, by what right does Sir W. Thomson entitle it a "principle of natural philosophy," and therefore state that uniformitarians are "directly opposed to the principles of natural philosophy"? As in the opinion of the French Academy, and of many eminent English and German savants, Mayer is one of the first physicists in Europe, I think it cannot be assumed with Prof. Tait that, "as regards method, Mayer and his supporters are little in advance of the Middle Ages," though undoubtedly Mayer is very different from Sir W. Thomson.

3. By what process does Sir W. Thomson discover "universal principles?" His universal principle regarding the origin of life "true through all space and all time," affords an opportune answer to this question. I would simply refer to Mr. Ray Lankester's article on that principle (NATURE, No. 97, p. 368), and ask if any one can discover a more satisfactory foundation for the *universal* principle of dissipation. From long study of Sir W. Thomson's reasonings, I conclude that he will reject any evidence for spontaneous generation, in consequence of the "universal principle" he has assumed on that question.

4. In Section A of the last British Association, Sir W. Thomson supported his argument regarding the form of the earth (controverted in your pages by Mr. Croll) by referring to existing mountains five miles high (see *Athenæum* report). His audience must have understood that these mountains are primæval, as otherwise the argument would have had no meaning. But as this is the reverse of the truth, I cannot help saying that Sir W. Thomson appears to consider himself entitled, not merely to invent principles, but also to invent facts. I know no conclusions of

science that might not be "briefly refuted" by such a method; but I think it would be fair to employ the words, "particular opinions of Sir W. Thomson" in place of "principles of natural philosophy," and "imaginary consequences of these opinions" in place of "facts." If this were done, all would admit that Sir W. Thomson's arguments are conclusive demonstrations; granted the premisses, the conclusions certainly follow. But geologists have simply to assume the contrary premisses, and they may mathematically demonstrate the reverse. Agree to beg all the difficulties of a question, and a certain conclusion may easily be obtained. This fact was recognised in the Middle Ages, and Mayer has not got rid of it.

P. W. STUART MENTEATH

42, Rankeillor Street, Edinburgh

[The remarks of Prof. Tait are contained in his opening lecture for Session 1869-70, which was sent to us with permission to make what use of it we chose. As the matter of Geological Time had been very fully discussed in this country, we did not insert the portions bearing on it. We believe that the portion which we did insert induced the editor of the *Revue* to apply to the author for the whole MS. As to the queries in the above letter we may note,

1. The Dissipation of Energy is a necessary consequence of the second law of Thermodynamics.

2. If "Mayer categorically denies its universality," so much the worse for his own credit, and for that of "the French Academy and the eminent English and German savants" who support him.

3. It is not for us to say what Sir W. Thomson would, or could not, do.

4. So, after all, Mayer seems to be no better than Sir W. Thomson.—ED.]

In Re Fungi

IT may allay the alarm of your correspondent "W. G. S." as to the decay of fungology in England, as far, at least, as one of the cases which he quotes is concerned. to be informed that so careful and critical a student of fungi as Mr. W. G. Smith confirmed the determination referred to, and on the faith of the abnormal specimen, included this rare and very critical species without any hesitation among the Middlesex fungi in the "Middlesex Flora," p. 408. Your correspondent "W. G. S." has missed the point of the paragraph from the *Journal of Botany* which he criticises. The specimens of this fungus collected by Mr. Wooster at Whitehall Gardens have a regular and normally developed pilcus, and were in striking contrast to the "abnormal specimens" (W. G. Smith, *l. c.*) from the Goswell Road.

F. L. S.

A Shadow on the Sky

I DO not know how common is the phenomenon described by Mrs. Charlotte Hall in NATURE of Nov. 9 (p. 25), but her communication leads me to report a much less striking appearance of the same kind, which I witnessed Feb. 20, 1870, in this neighbourhood. I was taking an early walk, and had mounted to the top of a ridge commanding an eastern view, about fifteen minutes after sunrise. The sky was veiled in a dark white. Above me, a little to the south and east, hung a ball of vapour in mid-air, warmed into smoke-colour by the rays of the sun, and yet so dense as to cut off these rays, and cast a rectilinear shadow of dark blue against the white coat of the sky. The shadow was sharply defined, and the whole effect was not unlike the nucleus and tail of a comet. In a few moments the shadow faded out, and, shortly after, the ball itself was dispersed. The moon, in its third quarter, was visible somewhat past the zenith, and surrounded with vapour. Twelve hours later we had a violent rainstorm.

N. J.

New York

Coal Measures of Ireland

IN the new edition of Jukes's "Manual of Geology," by Prof. Geikie, at page 592, it is stated, on the authority of Mr. E. Hull, that "in Leitrim, Fermanagh, and Tyrone, there are true representatives of the Yoredale series of England." I, however (as also the late Mr. Jukes), contend that no comparison can be drawn between the coal bearing rocks of Ireland and those of England. Furthermore, as Mr. Hull is unacquainted with these Irish rocks, having only seen a few isolated patches of them, I protest against his being quoted as an authority on the question, more especially as in the paper to which Mr. Geikie referred, "On the Geology of the Ballycastle Coal Fields, &c.," Mr. Hull states that while in the counties above mentioned there are true coal measures, in the provinces of Leinster and Munster there are none—a statement quite contrary to facts, as all the sections of these rocks in Leinster, Munster, and Connaught are identical, and probably, as suggested by the late Mr. Jukes, were once connected, as the lowest bed of coal occurs everywhere at a nearly equal height above the limestone. Furthermore, the intervening strata are nearly identical, there being a certain thickness of argillaceous beds below, next the limestone, and a mixture of arenaceous and argillaceous beds above.

Naturally it may be expected in all places where a sea gradually became shallow, that limestone would be succeeded by fine argillaceous beds, the latter by shore beds, more or less coarse and arenaceous, and eventually by land beds, such as coal, fire-clay, clunch, and the like.

Similar sequences are not uncommon, both on a large and small scale. On the large scale in the passage rocks from the limestone to the coal-bearing rocks of most countries, and on a small scale in the north of Ireland and in Scotland, where a bed of limestone will be succeeded by a shale, the shale by a sandstone, and the latter by a clay or coal.

If we examine into the thickness of the English and Irish rocks, the difficulty of a comparison is apparent. In the latter country the greatest thickness of the rocks called coal measures never exceeds 3,500 feet, this series of strata including all the rocks above the limestone; whilst in Lancashire, according to Mr. Hull's sections, the Yoredale beds alone exceed 5,000 feet in thickness.

Moreover, if any value is to be attached to palæontological evidence, we find that from the base upwards in the Irish rocks there are fossils which in England are considered to be characteristic of the true coal measures. The latter fact would seem to suggest that while in Ireland the upper part of the limestone was being deposited, in England the millstone grits and Yoredale rocks were accumulating, whilst subsequently, in both countries, true coal measures were deposited; those in Ireland being unfortunately very poor in coal, although containing very similar fossils.

In the northern extremity of Ireland, and in Scotland, the measures are very similar, and in certain places apparently identical, as pointed out years since by Sir R. Griffith. This, therefore, is no new fact, as Messrs. Hull and Geikie would suggest to their readers.

G. HENRY KINAHAN

Recent Changes in Circumpolar Lands

SOME years ago I wrote a paper for the Ethnological Society on some changes of surface affecting Ancient Ethnography. Since this was printed many facts have accumulated. These have led me to a tentative generalisation on the subject, which I should like to have discussed in your pages.

The question of the upheaval and subsidence of different areas of the earth's surface, as it is going on at the present moment, is of very great importance in geology, and yet few subjects have been more neglected. A few facts have been here and there collected; but even the best authorities treat the matter in a jejune fashion. According to them the areas of upheaval and subsidence are scattered over the earth's surface in an irregular manner, without any definite law or rule. I believe that with very slight local exceptions there is a very distinct law which governs the subject.

Putting aside altogether the southern hemisphere for the present, I wish to prove that the area of upheaval is confined to the land bordering the Polar Sea, and to the Polar Sea itself; that it is perfectly continuous all round the earth, and that it is greatest near the Pole, and gradually diminishes until it disappears about the 57th parallel, leading to the conclusion that the focus of upheaval is the Pole itself.

Of course, my observations are entirely confined to what is taking place *now*, and are not to be confused with the facts of any other period, historical or geological.

Commencing with Scandinavia, we have the remarkable testimony of Pliny, Mela, Solinus, and others, to the fact that Scandinavia was considered by the Roman geographers, whose authorities were bold and expert seamen, to be an archipelago. Ptolemy speaks of the Scandian Islands. The very name Scandinavia is evidence that those who used it looked upon it as an island. This implies that a great deal of dry land must then have been under water. In 1834 Sir Charles Lyell wrote his Bakerian lecture, in which he brought forward overwhelming evidence to prove that Scandinavia was then being gradually upheaved. Celsius, who wrote in the 17th century, had affirmed it, and calculated the rise at forty inches in a century. In 1807 Von Buch wrote that all the country from Frederickstadt, in Sweden, to Abo, in Finland, and perhaps as far as St. Petersburg, was slowly rising. Other authorities concurred, and lastly Sir Charles Lyell, who had approached the subject as a sceptic, was fully convinced after an exploration of the ground. At Stockholm he found striking proofs of change since the Baltic acquired its present tenants, Testacea found there seventy feet above the sea level being identical with those found in the adjacent sea. At Soderleige, a little farther south, and in a bed ninety feet above the sea level, besides the shells were found several buried vessels, made of wood, and joined with wooden pegs. In another place an iron anchor and nails were found. At Upsala brackish water plants were found in meadows where there are no salt springs; a proof that the sea had only recently retired. At Oregrund, forty miles to the north, the land had risen five inches and a half since 1820, and at Gefle were low pastures, where the inhabitants' fathers remembered boats and even ships floating. Experienced pilots in the Gulf of Bothnia estimated the fall of the waters at two feet in thirty years. Since Sir Charles Lyell's lecture both the Russians and the Swedes have made experiments all proving the same fact.

To the east of Scandinavia we have Finland, exhibiting all the characteristics of a recently-emerged land. It is a mere congeries of lakes and swamps, separated by moss and sand. The level of the lakes is constantly falling. In 1818 Lake Sovando was suddenly lowered; its waters escaped into Lake Ladoga, and much of its bottom was exposed. Similar traditions about low meadows but recently crossed by boats and ships to those existing in Sweden prevail here also, and there seems good ground for believing that in the days of the Norsemen the White Sea and the Gulf of Finland were joined by a considerable strait. Farther east, again, we have the experience of Murchison and his companions, who found on the banks of the Dwina and Vaga recent shells still retaining their colour, and of the same species as those found in the Arctic Sea. In Spitzbergen, Mr. Lamont reports (see vol. xviii. of the "Quarterly Journal of the Geographical Society") that he discovered recent bones and drift wood several miles inland and high above high-water mark, skeletons of whales thirty to forty feet above the sea level. The seal fishers told him the land was rising, and that the seas thereabouts were now too shallow for the right whale, which had forsaken the Spitzbergen coast. This is confirmed by Malmgren (see Petermann's *Mittheilungen*, 2, 1863). Farther east we have the Tundras between the Karen Sea and the Gulf of the Obi presenting bare desolate flats that look as if they had only recently emerged. Middendorf describes the surface of the great Siberian Tundra as coated with fine sand like that now being deposited by the Polar Sea. Von Wrangel has many useful remarks to prove my position. He tells us that Diomed Island, mentioned by Laptev and Schalaurov, is now joined to the mainland; the coast of the Swatoi Ness, which they describe as very indented and ruinous, is now straight. The Bear Islands are mere heaps of ice and stones, evidently but recently covered with water; and shoals and banks now occupy what was tolerably deep water in 1787 when Captain Sarypchew was there.

Herdenstrom, in 1810, found large birches scattered about the Tundra, 3° to the north of any known Siberian forest; probably drift wood such as Wrangel himself found drifting in the Polar Sea. Whales have now almost deserted the Siberian shores, where in the eighteenth century they were common. This is, no doubt, due to the shallowing of the water, as is the case in the Spitzbergen Sea. The shores of the Polar Sea, from the Lena to Behring's Straits, are for the most part low and flat. In winter it is hard to say where land ends and sea begins. A few versts inland, however, a line of high ground runs parallel with the present coast, and formerly, no doubt, constituted the boundary of the ocean. This belief is strengthened by the quantity of drift wood found in the Upper Level, and also by the shoals that run out, and will, no doubt, become dry land (*Vide* Wrangel's Introduction). "At several places along the coast we found old weathered drift wood at the height of two fathoms above the present level of the sea, whilst the lower drift wood lay at a level, indicating a change of level." Moving farther east again across Behring's Straits, we find Captain Beechey describing the coast as a high cliff, now separated from the sea by low flats with bones, &c., on them. I cannot speak with the same confidence of the vast archipelago that bounds America on the north, nor about the northern shores of America, my researches having been confined to Asia, but evidence must abound in the Arctic voyages. Drift wood and bones of whales are mentioned on high ground by several of them. If it be permitted to quote the works of M. Reclus as an authority, and I believe it to be a most sound book, he says, page 628, numerous indications of the phenomenon (*i.e.* of the upheaval of the circumpolar land of North America) have been recognised in the Arctic islands, scattered off the coasts of the Continent. At Port Kennedy Mr. Walker found shells of the present period at a height of 557 feet above the sea; a bone of a whale lay at a height of 164 feet. Again, page 651, after saying that Southern Greenland is being depressed, he continues, "On the north of Greenland, from lat. 76°, and in Grennell's land, &c., the directly contrary phenomenon is taking place." Hayes discovered on all the coasts the existence of ancient sea-beaches which had gradually risen to the height of 100 feet.

I have thus shown good ground for entertaining the notion that the land at present rising about the Pole is a continuous area, and is not rising merely in detached masses as M. Reclus's and Mr. Murray's maps (Geographical Distribution of Mammals) would lead us to suppose. I believe, further, that this area, bounded on the south by about the 57th parallel of latitude, *is the only area in the Northern Hemisphere which is at present undergoing upheaval.* I should feel grateful to any of your correspondents who would point out where there is another area (of course excepting local disturbance immediately round a volcanoe); or would direct me to any authorities throwing light on the question I have advanced, which for anything I know may be an old theory, or even an exploded heresy.

Not only is the land around the Pole rising, but there is evidence to show that the nearer we get to the Pole the more rapid the rise is. This has been shown most clearly in the case of Scandinavia by Sir Charles Lyell, who most carefully guaged the rise at different latitudes from Scania, where the land is almost stationary, to the northern parts of Norway, where the rise is four feet in a century. While in Spitzbergen and the Polar Sea of Siberia, if in the memory of seal fishers and others the water has shallowed so fast as to have excluded the right whale, we may presume that the rate of emergence continues to increase, until it reaches its focus at the Pole, as it certainly diminishes until it disappears towards the south between the 56th and 58th parallels of latitude. The subject is one of paramount importance to those who are trying to work out the history of the earth, and I once suggested at the British Association that it should be made the work of a special report, but I was snubbed. I appeal with more confidence to you, sir, to help me to ventilate it. The question of the *subsidence* of other areas, and of the correlated climatic change, I will reserve for another letter. HENRY H. HOWORTH

Derby House, Eccles

THE ENGLISH GOVERNMENT ECLIPSE EXPEDITION

MANY of the readers of NATURE are no doubt interested in the fate of the Eclipse Expedition of 1871. I will therefore give a sketch of their doings to the present time.

The P. and O. steamer *Mirzapore*, having the party on board, left Southampton on Oct. 26, and, after a rather rough voyage, reached Malta on Nov. 4; left again the same evening, and arrived at Port Saïd on the 8th; entered the Canal at once, and anchored at Suez on the 10th. Here she remained till the 12th, awaiting the arrival of the Brindisi mails; then left for Galle, where she arrived the 27th. On leaving the Channel a strong S.W. breeze was encountered, which soon increased to half a gale. The ship, though a roller, is a good sea boat, and made good progress; but the bad weather continued with little abatement until the *Mirzapore* was well in the Mediterranean, and nearing Malta. The sea then became calmer, the sun shone out, and the passengers, many of whom had not before emerged from their cabins, now came out as gay as possible, ready to make an impression at Malta. Our astronomers, who had not been exempt from the common fate of those who try the sea without a special education, now quickly roused themselves to make use of the opportunities for overhauling their instruments, and practising themselves for the work before them. The officers of the ship kindly gave every assistance, and those instruments that could be used on so unsteady a platform as a ship's deck were brought up from the hold, in which they had lain safely during the gale in the Bay, mounted on temporary stands, and used most diligently to investigate the changing phenomena with which we were surrounded. Classes also for mutual instruction were formed, so that each observer, on being detached in India, might —no matter what his special forte, whether spectroscope or polariscope—be able to impart instruction to the volunteers that we hope to obtain in India to aid in the good work. Our party numbered ten, viz.: Mr. Lockyer, chief, Messrs. Abbay, Moseley, Friswell, Capt. Tupman, R.M.A., and Commander Maclear, R.N., spectroscope observers; Dr. Thomson and Mr. Lewis, polarisers; Mr. Holliday, artist; and Mr. Davis, photographer. At Suez we were strengthened by the addition of Signor Respighi, from Rome, who has so distinguished himself by his observations of the solar atmosphere. The other passengers took great interest in the doings of the "Wise Men of the East," as they called us, and at their request, the day before arrival at Malta Mr. Lockyer gave a lecture on the advances that had been made of late years in solar physics, and on the object of this expedition.

Observations were made, as opportunities were given by clear sunrise and sunset at sea, on the alterations that take place in the absorption bands as the sun rises from the horizon; and here may be mentioned the interesting result, that whilst in the open sea the bands at sunrise and sunset were, with slight variations, the same as observed by Lieut. Hennesey (paper read before the Royal Society May 21, 1870) whilst passing through the Suez Canal and down the Red Sea, the lines attributed to aqueous vapour near C and D were weaker, and although the colour of the hills about Suez was of a delicate purple, especially at sunset, the violet end of the spectrum could hardly be seen.

In the Indian Ocean, when the air was close and filled with moisture, and the N.E. monsoon blowing, the absorption bands near the horizon became very strong, and it was very interesting during the afternoon to fix a telescope with spectroscope attached, so that the horizon bisected the field; the spectrum of the air above the horizon then gave the absorption bands, but they were very faint in the light reflected on the water from the upper part of the sky, and they could be seen lengthening and shortening as the ship rolled towards or from that side. On pointing the spectroscope at the sky above, only the ordinary solar spectrum could be seen.

The Canal was entered on the 8th of November about 3 P.M., and the ship anchored at Suez at noon on the 10th. The *Mirzapore* is one of the largest vessels that has passed through the Canal, and though she got through safely, it must not be supposed that she did not touch at all; in fact, the Canal is so narrow that too little room is left to allow for the time that so long a ship (400 feet) requires to answer her helm, especially at slow speed; and though the helm was shifted, and in some cases the engines reversed, as soon as the bow deviated from the straight line between the piles marking mid-channel, she could not be prevented touching several times. The narrowness also occasions delay when two vessels have to pass, one having to haul close in to the bank, and make herself as small as possible while the other goes by. But it is a grand work, and we have fully experienced the advantage of it, in avoiding the trans-shipment of our instruments, and the rough handling they would have experienced crossing the desert. We anchored in the Bitter Lakes on the evening of the 9th. The cause of the name they bear was shown by the fact, attested by our engineer, that the water was much salter than in the canal on either side.

On arrival at Galle we were delighted to find that Admiral Cockburn had brought his flag-ship the *Glasgow* to meet us, and convey our Indian party to Beypoor and Baikul. He has kindly placed all his accommodation at our disposal whilst he visits Ceylon. All our instruments were embarked yesterday, and we leave this morning for Beypoor, where we hope to arrive on the 2nd. The colonial steamer *Serendib* left yesterday with the parties for Jaffna and Trincomalee.

I can now give you the last dispositions of our party. In consequence of M. Janssen taking his station on the Neilgherries, we shall occupy two stations in Ceylon: Jaffna, where will be Captain Fyers, R.E., Captain Hogg, Captain Tupman, R.M.A., and Mr. Lewis; and Trincomalee, Mr. Moseley and Mr. Ferguson.

In India, Baikul or Ootacamund will be our head quarters, occupied by Messrs. Lockyer, Davis, Maclear, and Dr. Thomson; at Manantawhaddy, Messrs. Abbay and Friswell; at Poodacottah, Mr. Holliday and M. Respighi.

I hope I shall be able to tell you of the success of our efforts. J. P. MACLEAR

Galle, Ceylon, Nov. 28

The following provisional arrangements have been made in order to save time after arrival at Galle. Observers are warned that they are liable to alteration on receipt of information from the Indian and Ceylon authorities:—

1. The expedition will be divided into six parties as follows: (1) Lockyer, Thomson, Maclear; (2) Respighi, Holliday; (3) Tupman, Lewis, Ferguson; (4) Abbay, Friswell; (5) Moseley; (6) Davis.

2. Each party will be under the charge of the observer just named in each party, who will be held responsible for the instruments, &c., detailed for the use of observers. He will also be the channel of communication with the local authorities, and will make arrangements for the observations to be made by local volunteers.

3. Special instructions will subsequently be issued for the observations, and stations will be named. Each observer will be responsible to the chief of the expedition alone for these observations.

4. The observers in charge of each party will hand in to the treasurer a receipt for the instruments, &c., detailed for each party.

5. The observer in charge of each party will make a list of the cases containing the instruments, &c., and will arrange for their transfer from the *Mirzapore*, and for their future transit.

6. He will be held responsible for the repacking of the instruments after the eclipse, and for their transmission to Galle or Bombay.

To this we are able to append the following official instructions:—

The Ceylon party to be as follows:—Captains Fyers, Hogg, and Tupman; Messrs. Moseley, Lewis, Ferguson, jun., and Fœnandez.

Observing Stations to be as follows:—1. Jaffna and station south; 2. A position as far north of Trincomalee as possible, and a station south.

Instruments to be detailed as follows:—recording Dublin spectroscope, Capt. Fyers; tube Dublin spectroscope, Mr. Ferguson; analysing spectroscope, Mr. Moseley; camera, Capt. Hogg; polariscope, Mr. Lewis.

Mr. Fœnandez should observe on the central line. He should instruct two observers to make drawings of the Corona on a plan similar to his own near the southern limit of totality.

The recording spectroscope to be used to determine coronal lines in the red end of the spectrum to, and including, F. A high power should be used, and the prism should be adjusted for the minimum deviation of the central ray of this portion.

The tube spectroscope should be used in a similar manner for the other part, including F. Intensities referred to F to be most carefully noted.

Care to be taken that observers are not interrupted for two hours after totality.

Instruments to be returned to Galle, and shipped in P. and O. steamer, consigned to J. Browning, 111, Minories, London, E.C. All observations, photographic plates, drawings, &c., to be sent to Mr. Lockyer within a week of the eclipse. Observers to keep exact duplicates in case of loss.

The following resolutions were passed by the Government of India in the Home Department—under date 27th July and 21st October:—

"Colonel Tennant has already been authorised demi-officially to provide the astronomical instruments and photographic apparatus that he will require for his observations, and the Governor-General in Council understands that he is now in communication with Prof. Airy and Mr. Huggins on the subject. The cost of these appliances has been included in the estimate appended to Colonel Tennant's memorandum.

"In addition to these instruments, Colonel Tennant will require the aid of qualified observers, and it has been ascertained that the Superintendent of the Great Trigonometrical Survey is prepared to place the services of Mr. Hennessey and Captain Herschel, belonging to his department, temporarily at the disposal of Colonel Tennant for this purpose without prejudice to their proper duties. The Governor-General in Council approves of this arrangement, and is pleased to direct that the Survey officers above-named shall suffer no loss of their allowances while so employed, and that they shall have their travelling expenses paid out of the allotment of Rs. 15,000 sanctioned on account of the Eclipse observations. Colonel Tennant will arrange with Major Montgomerie beforehand when the officers in question should join them.

"The Governor-General in Council is further pleased to direct that Colonel Tennant shall receive from the Surveyor General's Department all the aid that he may require as regards photographic assistants, chemicals, &c.

"Lastly, the Governor General in Council is pleased to direct that the report of the result of Colonel Tennant's observations, and his accounts, shall be submitted by him to this department."

"From the correspondence received with the above despatch, the Governor General in Council has learnt that an expedition is being sent out from England under instructions from the Eclipse Committee of the British Association, and he is desirous that the Government of Madras will afford the expedition such assistance as it may require in the furtherance of its operations. Such assistance will probably consist in the provision, on a moderate scale, for three or four persons, at each place selected, of tents, means of subsistence, and locomotion, and in the erection of temporary observatories of a simple form. It may also be desirable to depute one or two persons to each party from the Public Works Department to assist the observers.

"Information has also been received that the French Government has deputed M. Janssen to visit India with the same object, and the Governor General in Council desires that the Government of Madras will afford every facility and assistance to that gentleman also.

"The Financial Department will be moved to sanction any reasonable expenditure that may be necessary to enable the Government of Madras to give effect to these instructions."

ARCTIC EXPLORATIONS

A SHORT paper of mine on the above subject appeared in NATURE of the 7th December, in which I stated some reasons for my belief that Smith Sound possesses no apparent advantages over Spitzbergen as a route by which to reach a very high northern latitude or the Pole itself. In fact I think the advantages are all the other way; and I shall endeavour to show one or two more reasons than I have already given for this belief.

Kane's and Hayes' ships were stopped by ice in Smith Sound before they reached lat. 79°, and this, I think, can readily be accounted for by the peculiar contour of the coast-line, as may be seen by the accompanying rough outline, taken from a copy of Dr. Hayes' chart in the Royal Geographical Society's Map-room.

The width of Kennedy Channel (a continuation of Smith Sound) is at 80° north lat. about 40 miles, but between latitudes 79° and 80°, Smith Sound expands to a width of something like 100 miles, this expansion being chiefly formed by a large bay on the east side. The south point of this bay, which I have marked A in the accompanying chart, runs far to the west in lat. 78° 30′ (thus changing the direction of the Sound from nearly true north and south to N.E. and S.W.), and approaching within 30 miles of the west shore at the point B.

If there is, as I believe, a set or drift of current southward, the ice will first be pressed with great force—as Kane found to his cost—against that side of the bay of which A is the south point, and then it will be driven across to the west shore somewhere near B at the narrowest part of the Sound in a closely-packed and continuous stream of heavy floes hitherto found impenetrable.

Should this idea be correct, and there is something more than theory to support it, this obstruction will be a constant and not an occasional one as long as there is a supply of ice to the north.

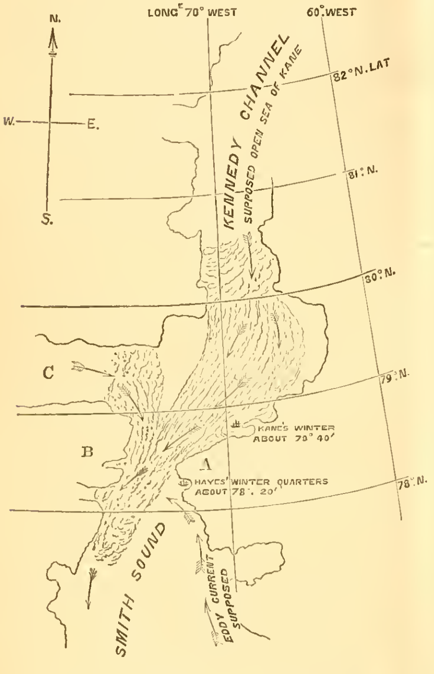
If there is a large opening extending far to the west at the place marked C, we have another probable opposing element; for if the set of current runs eastward through it, we shall have an important addition to the Smith Sound supply of ice, in making the barrier of the "*pack*" more formidable. The opinion I express as to the direction of the currents is not wholly hypothetical, for we have proofs of an *almost constant* current (it is sometimes reversed by strong winds) setting southward down Baffin's Bay and Davis Strait; and this current can only be fed by Lancaster and Smith Sounds and other openings to the west and north.

The only hope of an "easy" passage up Smith Sound to a higher latitude than 78° 40′ is the existence of Kane's "great open Polar Sea," for if such sea does exist, there would be no ice to the northward to keep up the supply of this commodity in Smith Sound, which would in the summer months be cleared of its winter covering by the southerly drift I have already mentioned, and the Sound would, and probably will be, consequently free from ice in August. But this is opposed to both Kane's and Hayes' experience, whatever their expressed opinions about the large open sea may have been.

That Kane's man Morton saw a very considerable extent of open water is not to be doubted, also that it may be quite true that he saw no ice to the northward, although he put down a point of land (whether correctly or not it is difficult to say) seventy miles distant in that direction. Every one, however, must be aware—for it is not necessary to have been in the Arctic seas to acquire such knowledge—that when the temperature of the air is lower than that of the water, a vapour or haze is formed by condensation, which, although by no means dense when looking through a small extent of it, becomes so much so when the observer has to look through eight or ten miles of it, that any low object, such as floe ice, would be quite invisible at either of these distances, and the haze itself would give the appearance of a distant water horizon.*

The opinion that this open sea was of limited extent is, I think, further confirmed by what Mr. Morton states as a proof (as he thought) of its being "boundless" or very large. Morton says "that he remained for three days watching the open sea rolling in waves at his feet, and, although there was a strong breeze or gale blowing from the north all the time, not a single piece of ice" floated past to the southward.†

My interpretation of the above fact is quite the opposite to that of Morton, for I believe there was a barrier of fixed ice at no great distance to the north, hid from his view by the cause I have named, which prevented any ice driving south at the season of the year when Morton was there, I think in June.



I offer these opinions with much diffidence, for we have been recently told that all great Arctic authorities now agree as to the Smith Sound route being the best. When the subject was brought prominently to notice in 1865, the "great authorities" did not agree, there being about as many opinions on one side as on the other.

At that time, without the slightest pretence to being an "authority" in the matter, I looked rather closely into the figures on which the facts favourable to the Smith Sound route were founded, and finding these figures in several important instances erroneous, the facts themselves lost much of their value. JOHN RAE

* I use the term "water horizon" in opposition to "ice horizon," which exhibits a bright line easily recognisable by those who have once seen it.

† As I quote from memory, I give to the best of my belief Morton's meaning, if not his words.

THE TYPHOON OF 2nd SEPTEMBER, 1871

THE Typhoon in China of the 2nd September last, detailed accounts of which reached England by the last mail, and which included in its area of most active violence the island and vicinity of Hong Kong, affords to those interested in such natural phenomena an opportunity of observing their varied characteristics, that may possibly never occur again. The great centre of its efforts having been in a situation where elaborate observations could be taken regarding it both at sea and land, a vast amount of information has been collected on the subject, which throws more light upon these singular "freaks of nature" than has ever before been arrived at.

In treating on the subject, I shall in the first place point out the course which—after careful investigation—I believe the typhoon to have followed, and afterwards I shall state the evidences that I adduce in support of the theory which I have adopted. Before commencing, however, it may be as well briefly to illustrate the plan engraved. The names *Formosa*, *Siam*, *Onward*, *Mikado*, *Woodbine*, and *Anna Henderson* are those of six vessels which were on their way to and within a short distance of Hong Kong during the typhoon, and extracts from whose shipping reports are now before me. A portion of the continent of China is to the north of the plan. The town of Macao and the islands of Hong Kong, Lantao, and Lema are in their respective positions.

The course which was taken by the typhoon was nearly allied to a parabolic curve. I have not attempted to trace its source farther eastward than the position indicated by 22° 30′ N. lat. and 116° 10′ E. long., where it overtook the *Formosa* (see *a* in map), or to follow it beyond the point indicated by 21° 15″ N. lat. and 115° 45′ E. long., where it struck the *Onward* (see *f* in map) on its return from the West. This portion of its course is marked in the plan by a succession of dotted lines. Consequently my observations are confined to the proceedings of the typhoon within these limits. After passing the *Formosa*, it swept over Hong Kong, crossed the mouth of the Canton River, and continued its headlong career to the town of Macao. Approaching this point, however, it was met by a strong northerly gale, and turned towards the south, but again encountering opposition in the shape of a south-west gale, it returned towards the east, upsetting the *Mikado* and driving the *Onward* before it. Throughout its entire course it consisted of a comparatively narrow belt of wind.

So much for the statement of my theorem. Now for its proofs.

I assume that only three conditions are necessary to substantiate my argument:—

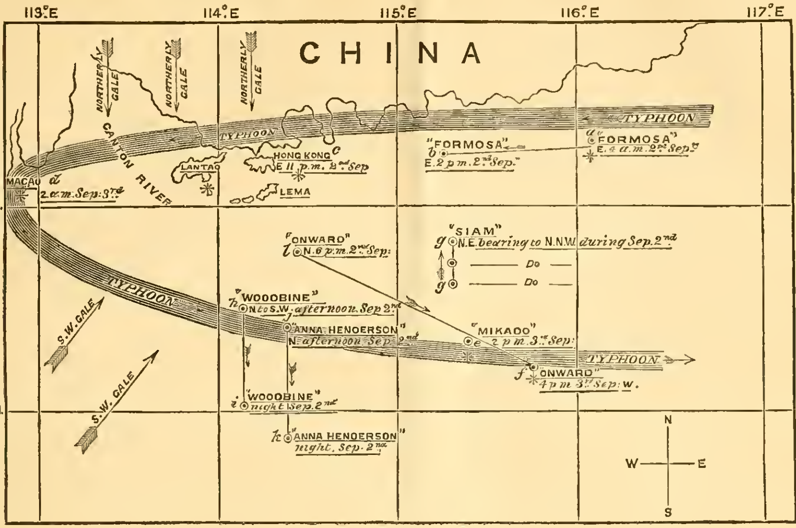
1. I must prove that the typhoon reached the various positions which I have indicated in the order actually laid down.

2. That it reached them at successive intervals of time.

3. That its greatest observed efforts were exerted on or in the vicinity of the line adopted by me, and not at any appreciable distance to the right or left of it.

4. That the two opposing gales, which I have described as occasioning the alteration in the course of the typhoon, did actually exist.

The first and second of these four conditions appear to be so intimately connected, that I think I cannot do better than consider them together. The earliest observations of the typhoon were made by the *Formosa*, which experienced its full force in the situation indicated in the plan between *a* and *b*. Both positions are accurately determined. The following is an extract from the shipping report: "On September 2, the barometer 29·30, experienced very heavy typhoon; during the typhoon the ship suffered some damage. At 4 A.M. on 2nd inst., barometer 29·25, blowing very heavy from east; at 12 noon, the same day, the wind moderated; at 2 P.M. on same made some



sail." The second series of observations was taken at Hong Kong (*c* in plan). Here I may quote from the register kept at Junk Island, near Hong Kong, during Saturday, September 2, and Sunday, September 3:—

September 2

Hour	Wind	Force	Barometer
1	N.N.W.	6	29·58
2	,,	,,	29·54
3	,,	,,	29·52
4	,,	,,	29·50
5	,,	,,	29·48
6	,,	,,	29·46
7	,,	,,	29·45
8	,,	,,	29·44
9	N.N.E.	,,	29·42
10	,,	7	29·40
11	N. by W.	7	29·39
Noon	,,	8	29·38
1	,,	9	29·35
2	N.	9	29·30
3	,,	9	29·29
4	N. by E.	10	29·28
5	,,	10	29·27
6	N.N.E.	10	29·22
7	,,	10	29·19
8	N.E. by N.	11	29·16
9	E.N.E.	12	29·16 *
10	,,	12	29·15 *
11	E.	12	29·17 *
Midnight	E. by S. ½S.	12	29·18
September 3			
1	E. by S. ½S.	12	29·18
2	E.S.E.	11	29·25
3	,,	10	29·30

—and so on, the barometer rising, as the gale decreased.

It will, of course, be remarked that the *east* wind was the veritable typhoon. This is clear from the fact of the barometer reaching its lowest point, and the force of wind being the highest registered, at or about the hour when the vane pointed to the east. Now, to proceed in the same direction that the typhoon is following as far as the town of Macao (*d* in plan). No register, unfortunately, was preserved—at least, that has transpired—of the direction of the winds at Macao during September 2 and 3, but the barometrical readings were as follows:—

Date	Hour	Reading
September 2	12 Noon	29·705
,,	3 P.M.	29 605
,,	5 ,,	29·555
,,	6 ,,	29·485
,,	7 ,,	29 475
,,	8 ,,	29·425
,,	9 ,,	29·405
,,	10 ,,	29·285
,,	11 ,,	29·185
,,	11·30 ,,	29·135
,,	12 Midnight	29·065
September 3	1 A.M.	28·785
,,	1·30 ,,	28·485
,,	2 ,,	28·385 *
,,	3·30 ,,	28 885
,,	4 ,,	29·035

Still, although no record has been preserved of the direction from which the wind came on this occasion, it is evident, from the nature of the injuries inflicted upon Macao, that it was the turning point or apex of: he typhoon. The effects bore a strong analogy to those of a cyclone or whirlwind, as will be seen from the following extract from the *Overland China Mail* of September 15:

"No less than three vessels, the *Vistula*, French *Edouard et Marie*, and a Dutch barque, have been wrecked in the roads. . . . . Baron de Cercal's house on the point has been unroofed; the clock tower top has been blown down; and the façade of the San de Lorenzo Church has been torn off by the force of the wind." Continuing still farther round the course indicated by the

* At this time, between 9 and 11, the typhoon struck the island.

* At this time the typhoon struck the island.

dotted lines in the map, and omitting to take notice of the *Woodbine* and *Anna Henderson*, we arrive at the *Mikado*, whose situation (marked *e* on the plan), although not so clearly specified in the report as might be desirable, must, nevertheless, have approximated to that laid down, if we take into consideration the direction from which it was sailing (from Saigon to Hong Kong) and the time at which it arrived in harbour, viz., about four and twenty hours after the typhoon had passed over it. The shipping report is as follows:—"On midnight the 1st inst. (September) the barometer falling, wind increasing from the northerly, barometer falling rapidly. On midnight of the 2nd instant, the weather *indicating* a typhoon, began to take in sail; the wind continued increasing, the barometer still falling; at 8 A.M. on the (3rd)* instant took in the main topsail; at 11 A.M. till 2 P.M. blowing a very heavy typhoon, the ship lying on her beam end, the barometer 29·34. . . . . . At 3 P.M. weather began to moderate, and the ship began to righten. . . . . At 8 P.M. on same day the weather again moderated, and we then commenced to make sail to Hong Kong; the wind *rounded* to E.S.E." (showing that it had been westerly or north-westerly during the gale). But the fullest and most minute account of the typhoon appears in the narrative of the *Onward's* adventures during its occurrence; and here, fortunately, I am able to repose the utmost confidence in the statements adduced, owing to a personal acquaintance of several years with the Captain and officers of that vessel. There is not the remotest difficulty in determining the position of Captain Whyte's vessel during the 2nd and 3rd September, the bearings and distances being quoted on all important occasions. The report runs thus:—"Current setting to S.W. ½ W., 34 miles daily. September 2, at 6 P.M. (barometer 29·83), N.E., head of Lema Islands, bore N. by W. ½ W., 15 miles distant; tacked ship and stood to eastward, wind at N. with a heavy easterly sea coming away, with all appearances of bad weather; midnight (barometer 29·70) wind N. increasing to a gale; reduced the ship to two topsails; 4 A.M. (barometer 29·59), wind still at N., gale still increasing with heavy sea from the eastward; 8 A.M. (barometer 29·39), strong and increasing gale, furled all sails, and secured them with double gaskets, and made every preparation for a hard gale. September 3, at noon (barometer 29·15, still falling), wind N.W., blowing most terrifically with a fearful cross sea, ship pitching heavily, putting bowspit and jibboom under water at times, and filling the decks with water; 4 P.M. (barometer 29·3), wind W., blowing harder than ever with thick rain; at 6 P.M. (barometer 29·10), wind W.S.W. blowing still most terrifically with a most fearful cross sea running; at 8 P.M. (barometer 29·20), wind S.W. inclined to moderate, sea still very heavy; midnight (barometer 29·39), wind at S., both wind and sea greatly down with all appearance of better weather; 6 A.M. (barometer 29·60), wind S.S.E., moderate breeze, made sail and squared away for port." The run of the ship from 6 P.M. September 2 till 4 P.M. September 3, I have represented by the line *l f*, as although the course taken was *supposed* to be easterly, the *strong current* setting in a S.W. direction would certainly bring it down to the point *f*. Thus the ship in endeavouring to escape the typhoon ran right into it! Now what may be gathered from all these facts? That a terrific gale from the east struck the *Formosa* in the position indicated by *a* on the 2nd September at 4 A.M.; that it passed over Hong Kong (at *c* in map) between 10 and 11 the following night; that it reached Macao (*d* in map) at 2 A.M. on the morning of the 3rd, exhibiting such peculiar phases of character as would lead one to believe that it was revolving on its axis; that (after changing its direction) it overtook the *Mikado* in the position indicated by *e*, at 2 P.M. on the 3rd September; and that finally it swept over the *Onward* in the position indicated by *f*, still coming from the west, at 4 P.M. the same day.

Hence I conceive that my *first two conditions* are proven.

The third is as easily disposed of. That the typhoon did not spread itself out to any great extent in a northerly direction is clear from the fact of Canton not having experienced its fury. There was a smart gale blowing on Saturday and Sunday; but the barometer did not descend below 29·40, and the typhoon was described there as being "insignificant." That it was not felt so far south as 21° N. lat. is evident from the shipping reports of the *Woodbine* and *Anna Henderson*, which make no mention of it. They speak of gales blowing hard from the N. and S.W., and culminating upon the *evening* of the 2nd of September; but it is apparent, from the tone of their descriptions, that they did not encounter the veritable typhoon. The *Woodbine's* report is as follows:—"2nd of September, about thirty miles from Lema Island, when encountered a heavy typhoon from N. to S.W., with heavy sea." The *Anna Henderson* says:—"Wind veering to N.; on the 2nd increased to a gale, splitting several sails; at 7 A.M. on same day blew away the main topsail, the gale continued up to 6 P.M., than began to moderate." Their courses after receiving the shock of the northerly gale are represented by *h i* and *j k*, and these cannot be far from the actual ones taken, as the positions *h* and *j* are determined from observations quoted in the shipping reports, and the ships having been small, with wind and current both dead against them, *must* have been *driven* in the directions indicated. Fortunate for them that it was so, for by this accident they escaped the typhoon altogether. With regard to the interior edge of the typhoon, it would be impossible to ascertain how far it extended; but that there was a region of comparative calm within its circumference is easily proved. The *Siam*, from Newchwang, a port in the north of China, when in 21° 30′ N. lat. and 115° 15′ E. long., experienced a gale, which, during the 2nd of September, went right round the compass, clearly showing that the ship was in the centre of the typhoon. But that the *Siam* did not feel the full force of the gale or anything like it is equally clear from the trifling notice taken of its effects. The date of this vessel's arrival in port leads us to believe that it scarcely altered its position during the gale; probably as the wind veered round it drifted northwards, as indicated at *g g* in the map. The shipping report states:—"1st of September, in lat. 21° 30′ N., long. 115° 15′ E., when experienced *another* heavy *typhoon** from N.E. veering to N.N.W., and round to S.S.E., with very heavy cross sea, and much rain; on the 3rd inst. it began to moderate, wind from S. to S.S.E." I think therefore we may fairly gather that the typhoon's influence did not extend in any great degree to the right or left of the course laid down for it in my map.

Hence condition *three* is proven.

The fourth condition scarcely requires demonstration. The truth of it is apparent from the report of the wind at Hong Kong up to 3 P.M. on the 2nd of September, and that of the ship *Woodbine*, which occupied the most westerly position of any of the vessels, from whose accounts I have gathered my information.

It seems therefore reasonable to assume that the typhoon of the 2nd of September did take the course indicated by me, which is nearly that of a parabolic curve. Should such be the case, it goes far to prove that these eccentric phenomena have not a circular form, as has hitherto been imagined.

One of the most interesting facts that has been elicited from these investigations is, however, the indication that a space of comparative calm does exist within the circuit

* I have altered this from 2nd to 3rd as the typhoon could not have been "indicated" after it had actually *occurred!* The figure 2 was evidently a misprint.

* This shows in how qualified a sense the word "typhoon" must be taken in reading the *Siam's* report.

of a typhoon, a theory which has always been advanced, but, so far as I know, has never hitherto been substantiated by any actual observations. The case of the *Siam* is a strong argument in favour of the truth of such a theory, for in point of fact it may be said to have scarcely felt the effects of the typhoon at all.

Should any of your readers be disposed to sift the various evidences which I have adduced, the papers are in my possession, and access can be had to them at any time.

FRANK ARMSTRONG

NOTES

WE have received full intelligence of the English Eclipse Expedition from Mr. Lockyer, under date Galle, November 29. At that date the expedition had been detailed into various parties for service at different stations in Ceylon and the mainland; the instructions to these several parties are reprinted in another column. Mr. Lockyer, Dr. Thomson, and Captain Maclear were to observe at Ootacamund, Mr. Davis being detached to photograph at Gunote; Messrs. Abbay and Friswell were to go to Manantawaddy, Signor Respighi and Mr. Holliday to Poodacottah; while Captains Tupman and Fyers and Messrs. Moseley and Lewis were to proceed to Trincomalee. The Indian and Cingalese authorities and the officers of the *Mirzapore* and *Glasgow* had exerted themselves to the utmost to assist the expedition, and the Ceylon party acknowledge great obligation to Captain Fyers, the Surveyor-General. In another column will be found an account of the voyage out.

WE hear with great satisfaction that Mr. Edgar Leopold Layard, C.M.Z.S., has received the appointment of H.B.M. Consul at Para. Mr. Layard has already done good service to science in Ceylon and South Africa, and will now have the pleasure of investigating the fauna and flora of a third and not less interesting region. Before leaving England we understand that Mr. Layard will publish a new and revised edition of his work on "The Birds of the Cape Colony," which is now nearly ready for the press.

WE are informed that Mr. Leighton is preparing for publication a conspectus of all the Lichens hitherto discovered throughout the world, with diagnoses, &c., and also a second edition of the Lichen Flora of Great Britain, Ireland, and the Channel Islands, which will combine an Introduction, Glossary, and Index, and which, it is hoped, will be ready for the press early in 1872. The Glossary, &c., will be printed separately, so as to enable possessors of the first edition to purchase separately.

MR. T. K. SALMON, of Guildford, is making preparations to start on a collecting expedition to the highlands of the Columbian republic. Mr. Salmon's head-quarters will be at Medellin, in the State of Antroquia, whence he will explore the Cordillera of Quindin, and upper valley of the Cauca. Mr. Edwin Gerrard, jun, of College Street, Camden Town, acts as his agent, and will be happy to receive subscriptions in aid of the expedition.

WE are glad to hear that the well-known naturalist, Mr. W. T. Blanford, of the Indian Geological Survey, is appointed a member of the British expedition for the survey of the boundary between Persia and Beloochistan. Commencing on the coast of Mekran the party will pass northward to Seistan and Herat. In Seistan they will enter a most interesting region, of which the geology and zoology are quite unknown. The river Helmund, and Lake of Seistan, in which it loses itself, will certainly present many features eminently worthy of scientific investigation, of which no one is more qualified to take advantage of than the ex-geologist of the Abyssinian Expedition.

THE recent death of Dr. Seemann, who for nine years has conducted the *Journal of Botany*, has caused a change of editorship. A new (2nd) series will be commenced in 1872, under the management of Dr. Trimen, of the British Museum, for the last two years a sub-editor, with Mr. Baker, of Kew, who will continue to be associated with Dr. Trimen in the conduct of the new series. We are also requested to state that unavoidable circumstances will delay for a few days the publication of the January number.

THE Edinburgh papers record the death of Mr. J. B. Davies, assistant-keeper of the natural history section of the Museum of Science and Art in that city. Mr. Davies was appointed to his position in the museum, while it was in its old place in the College, by Edward Forbes during the brief period that gifted naturalist occupied the Chair of Natural History; and in the discharge of his duties he was as much distinguished by the extent and accuracy of his knowledge as by his readiness to assist all students of his science, and by his courteous bearing. In addition to his appointment in the museum, Mr. Davies held the lectureship on zoology in the Royal High School, was assistant-secretary to the Royal Physical Society, and an Associate of the Botanical Society. He was the au hor of a little manual of practical natural history termed "The Naturalist's Guide."

THE following have been elected office-bearers of the Edinburgh Botanical Society for the ensuing year:—President, Prof. Wyville Thomson, LL.D.; Vice-Presidents, Dr. M'Bain, R.N., Prof. Dickson, Mr. Buchanan, Dr. T. A. G. Balfour; Secretary, Prof. Balfour; Foreign Secretary, Prof. Douglas Maclagan; Treasurer, Mr. P. N. Fraser; Auditor, Mr. Tod; Artist, Mr. Neil Stewart; Assist. Sec. and Curator, Mr. John Sadler.

IN connection with the Gilchrist Education Trust, arrangements have, we understand, been made for the delivery at the Lambeth Baths of a series of lectures, chiefly of a scientific character. The names of Prof. Huxley and Dr. Carpenter are mentioned among the probable lecturers.

MM. DELAUNAY and Ch. St. Claire-Deville have presented to the French Academy of Sciences some further interesting notes of the cold of November and the early part of December. M. Delaunay remarks that the cold advanced, as is usually the case, from north-east to south-west. The minimum temperatures were recorded at Gröningen, in Holland, on Dec. 7 (-10° C. = 14° F.); at Brussels ($-12^\circ.6$ C. = 9.5° F.) on the 8th; and at Paris ($-21^\circ.3$ = -6° F.) on the 9th. This extremely low temperature appears to have been limited to a very small tract of country between Paris and Charleville. On the same day the temperature was above the freezing-point in Scotland as far north as Nairn, and in the greater part of England, falling only at Greenwich as low as $-2^\circ.3$ C. ($=28^\circ$ F.). The severity of the frost was considerably mitigated at Paris on the 10th and 11th; but on the latter date it was again as low as $-22^\circ.6$ C. ($=-8^\circ.5$ F.) at Haparanda, on the Gulf of Bothnia, -15° C. ($=5^\circ$ F.) at Stockholm, and $-14^\circ.1$ C. ($=6^\circ.5$ F.) at St. Petersburg.

SOME of our readers will recollect the controversy which took place in the "Proceedings of the Zoological Society" and the *Athenæum*, some six months ago, respecting a tortoise's skull in the British Museum, upon which Dr. Gray had established a new genus and species, *Scapia falconeri*. Mr. Theobald maintained that this skull (received by the British Museum from the executors of the late Dr. Falconer) had originally belonged to one of the two typical specimens of Mr. Blyth's *Testudo Phayrei*, in the Indian Museum, Calcutta, and that consequently *Scapia falconeri*, Gray = *Testudo phayrei*, Blyth. Dr. Blyth maintained the contrary. We understand that the director of the Indian Museum has recently claimed the skull in question, and that it is now on its way back to Calcutta, so that the authorities of the British Museum must have given up their view of the question.

At a recent meeting of the Manchester Literary and Philosophical Society, Mr. John Hopkinson, B.A., D.Sc., detailed some experiments on the subject of the rupture of iron wire by a blow, the results of which are—1. That if any physical cause increase the tenacity of wire, but increase the product of its elasticity and linear density in a more than duplicate ratio, it will render it more liable to break under a blow. 2. That the breaking of wire under a blow depends intimately on the length of the wire, its support, and the method of applying the blow. 3. That in cases such as surges on chains, &c., the effect depends more on the velocity than on the momentum or *vis viva* of the surge. 4. That it is very rash to generalise from observations on the breaking of structures by a blow in one case to others even nearly allied, without carefully considering all the details.

We learn from the *Lancet* that all the English universities have now accepted the draft scheme for a Conjoint Examination Board, as proposed by the College of Physicians and the College of Surgeons of England, and that it only now remains to submit the matter to the standing counsel of the two latter bodies for their opinion as to the practicability of carrying out the scheme without in any way violating the provisions of their respective charters. It is pretty well known that in the case of the Royal College of Physicians no difficulty at all is apprehended. It is probably so with the College of Surgeons, but of this we have never had positive assurance.

Attention has been called to the present disgraceful state of the fine mausoleum erected to the memory of Sir John Soane, in the cemetery of St. Giles-in-the-Fields, King's Cross. The tomb of the founder of the first art museum and architectural library in England is surely deserving of preservation. At present, however, its balustrades are broken, its marble capitals chipped, the inscription wilfully defaced, and the entrance filled with brick rubbish. We commend this state of things to all art students.

In the current number of *La Philosophie Positive*, Nov.—Dec., 1871, M. Littré calls attention to the reorganisation of public education in France. "If we are ever," he says, "to have a public system guided by a sound general method, we must begin tentatively and experimentally with private effort;" he then adds, "As for ourselves, it is intended among the writers in this review to compose six treatises, one for each of the fundamental sciences, mathematics, astronomy, physics, chemistry, biology and sociology. They should be so subordinated one to another that each science should form an introduction to the next above it in the scale; they should also be so far restricted to what is of essential importance that the entire course might be mastered in a time compatible with the necessities of life; and complete enough to raise the student to the main level of positive knowledge." There has been a good deal of discussion, especially in this country, about the scientific value of Comte's classification of the sciences. Perhaps a practical experiment like the above is the best criterion of the question, and the wonder is that it has not been applied before.

After unexpected delays, the new Coast-Survey exploring vessel, the *Hassler*, left Boston on December 4, bound for California *viâ* the Straits of Magellan. The *personnel*, which is under the scientific direction of Prof. Agassiz, and the plans of this expedition, have already been given.

Harper's Weekly gives the following account of the labours of Prof. E. P. Cope, of Philadelphia, mainly in the valley of the Smoky Hill Fork of the Republican River in Kansas, where, under the protection of an escort of seventy-five infantry, commanded by Captain Butler, and detailed by order of General Pope, he spent seventeen days in the diligent prosecution of his labours. As is well known to American palæontologists, this region is one of the richest of the world in fossil remains of reptiles and fishes. Of these a large number of specimens were obtained by Prof. Cope, many of extraordinary magnitude, and some of them entirely new to science. More or less complete series were obtained of the bones of animals previously known only by a few fragments, thus supplying much better information as to their affinities and position in the system. Nearly the entire skeleton of a large fish, provided with teeth of immense power, was exhumed. This animal is to bear the name of *Portheus molossus*; and its remains occurred in such abundance as to demonstrate that it must have been a characteristic and very formidable inhabitant of the cretaceous seas. Another discovery was that of a reptilian form related to or intermediate between the tortoises and serpents. The ribs of this animal were long and attenuated; but instead of being united in the carapace, as in the tortoise, remained separate possibly united by membrane. If built at all on the chelonian pattern the expanse would have been at least twenty feet. This is to be called *Protostega gigas*. During his explorations in 1870 Prof. Marsh ascertained the existence of a species of pterodactyl, or flying lizard, in the cretaceous strata of the West, and additional specimens of the same or another species were found by Prof. Cope during the expedition just referred to. The most gigantic reptiles met with by him this year were species of *Liodon*, *Polycotylus*, and *Elasmosaurus*. Of these *Liodon* was found most abundantly, and one specimen will probably prove to be the largest of all known reptiles. *Elasmosaurus* had the most massive body, and must have presented an extraordinary appearance, in consequence of the great length of its neck.

We have already referred occasionally to investigations prosecuted during the past summer on the great lakes of North America, into the fauna and physical condition of the deeper waters; and we find in the last number of *Silliman's Journal* a more detailed account of that portion of the work carried on in Lake Superior upon the U.S. steamer *Search*, under the direction of Gen. Comstock, of the Lake Survey, as reported by Mr. Sydney J. Smith, the zoologist of the expedition. The deepest water met with was 169 fathoms, the bottom being there covered, as in all the deeper portions of the lake, with a uniform deposit of clay or clay mud; and not the slightest trace of saline matter was detected in the water in any part of the lake. The temperature, everywhere below thirty or forty fathoms, varied very little from 39° F, although in August it varied at the surface from 50° to 55°. The fauna at the bottom was found to correspond to these physical conditions. In the shallow waters the species vary down to thirty or forty fathoms, after which the deep-water fauna begins, and the species appear to be uniformly distributed. The list of species is meagre, and the deep-water region is characterised rather by the absence of many of the shore species than by the presence of any peculiar class. The same crustaceans and marine forms met with in 1870 in Lake Michigan were also found here abundantly, together with the same species of *Pisidium*; and some of the crustaceans have so far been undistinguishable from those found in Lake Wetter, in Sweden. The detailed account, of which that in the *Journal of Science* is an abstract, appears in the report of the Chief Engineer of the army to the Secretary of War just presented to Congress (Report of American Secretary of War, vol. ii. p. 1020).

M. Raoult states, in a paper read before the French Academy of Sciences, that cane sugar becomes transformed into grape sugar under the prolonged influence of light. Having dissolved 10 grammes of white sugar in 50 grammes of pure water, and boiled the solution for a few minutes, he placed equal portions in two white glass tubes, which were then hermetically closed. One was deposited in a dark place, while the other was exposed to light. Five months afterwards the tubes were opened, and the contents of that which had been exposed to light gave the reaction of glucose.

A CORRESPONDENT of the *Madras Times* states that on the night of the 21st of October a remarkable meteor was seen at Trevandrum. It first became visible in the northern part of the sky, proceeding at a rapid rate and in a straight line, at an elevation of from 35° to 40°. It was visible for about four seconds.

A BRAHMIN astronomer at Surat has "predicted" that a terrible earthquake will be felt in some parts of the Bombay Presidency either in December or January next.

INDIAN papers state that during the first six months of this year as many as 183 tigers and cubs, 393 panthers and leopards, 203 bears, 281 wolves, and 188 hyenas, were destroyed in the Central Provinces at a cost to the Government of about 9,000 rupees (900*l*.). What a chance for any enterprising Zoological Museum!

WE do not look for zoological statistics in the annals of trading companies; but there is one report that does afford material, that of the Hudson's Bay Company. There we see year by year the varying number of fur-bearing animals, given in a kind of Registrar-General's return of deaths. This year we do not see the details, but we learn there has been a great dearth of martens. A more serious ethnological fact is the great losses by small-pox among the Indians of the Saskachewan district, being no less than 3,000. Throughout the Hudson's Bay district the Canadian Government is employed in regulating the Indians, but this no less forebodes their extinction; the more particularly as a railway is advancing to the Pacific, and steamers are to be placed on the Saskachewan river and Lake Manitoba. Martens that are not killed and Indians that die mean reduced dividends to the Hudson's Bay shareholders and traders.

ON the Chilian map is to be placed Augol, just made a city. It is situated in lat. 37° 42′ S., long. 72° 17′ W., about three miles south of the head waters of the river Verzaro, and twenty-eight miles from Nacimiento. It was founded Dec. 6, 1862, and is a fortified place on the river Pecoiquen.

AT Santiago, in Chile, a zoological garden is to be formed in the Quinta Normal, or Normal Garden.

FROM recent accounts in the *Panama Star and Herald*, it appears the Panama pearl fisheries are now carried on by negroes, whose villages remind the traveller of Western Africa. The value of the fishery is about 30,000*l*. a year, but signs of exhaustion are now showing themselves. This is greatly attributable to the use of diving machines. A gentleman who owns one of the islands, having regulated his fisheries in the Ceylon manner, found that after two years' repose he got a larger crop. It is therefore suggested to regulate the Panama fisheries by law.

COAL has been discovered at Neblinto in Chile. That country is already largely engaged in the shipment of coal.

MUSCAT is now to be divided on the maps into two states, Muscat and Sohar, a once famous name.

THE diamond capital of Adamanta, at the Cape, is likely to become a permanent town. Its present settled and floating population is 20,000.

THE collector of Tinnevelly, in Madras, reports that he has come to the conclusion, after his inspection of the Government Pearl Fisheries, that the oysters migrate every year when young.

THE miners of Copiapa in Chile have undertaken an exploration by subscription of the rich mineral resources of their Cordillera.

*NUMERIC RELATIONS OF THE VERTEBRATE SYSTEM**

THERE are five (not four only) complete neural rib arches to to the cranium of all vertebrate animals, to wit: (1) The condylar or sensitive belt with the condyle plates for side ribs, and the lower arch of the transversely bipartite occiput for its vault piece; (2) the petrosal or acoustic, containing the auditory nerves in its side beams (easily detected by removing the ear drum of Felines, &c.), and overarched by the interior belt of the occipital squama; (3) the parietal belt originally containing the true gustative of fixed tastes (sour, sweet, salt and bitter, the glosso-pharyngeal), in an incision; from which it is, however, soon crowded out by the internal carotid artery and the overlapping "acoustic rib blade." The next (4) is the optic or frontal, visibly succeeded, in fishes, by (5) the ethmoidal or olfactory vertebra. The rest of the cranium is formed by its "extremities" or prehensile appendages.

The same numeric law which pervades the entire vegetable kingdom reoccurs in the human fabric in a very marked manner.

The number of "radiating elements" in a coil or whorl, or of whorls in a cycle, or in cycles generally speaking, as in pine cones and flower buds, &c., are the following:—

1, 2, 3, 5, 8, 13, 21, 34, 55, 89, 144, &c., progressing by the summation of the last two numbers.

The bands or parallel coils of flowers or scales in pine cones, sunflower discs, &c., embody these numbers successively, as they grow steeper and steeper, alternately on the right and left. The vertical bands, or columns, give the number of parts of the cycles involved.

The explanation heretofore given by me is this, that one element generates the other.

The elements are radial; they are bilateral rays, with a rift, so to speak, on the opposite side. It is there where, in a like manner as the seed-leaves of flowering plants produce prolific "ovules," new radial organs are developed from the preceding ones—laterally at alternate heights and towards the wider spaces.

This process, referred to the radial organs of plants in an early stage, will yield:—

1. The numbers of parts in question, successively.
2. The peculiar law of interpolations or of "divergence," viz.: by a number of interstices represented by the second preceding one of each cyclar number.
3. It will conclude the cycles, if it be supposed that the activity of each junior member depends on that of its progenital one; as in all cases of simple branch developments.

These numbers occur in like manner in the human frame, as follows:

Inclusive of the terminal (ossified or gristly) coccygeal element, we have exactly thirty-four spinal vertebræ.

Classifying nerves by their work, or "function," we find—

3 pairs of cervical nerves (neck).
5 pairs of brachial nerves (arms).
8 pairs of pedal nerves, composed of 3 crural (lumbar) and 5 ischiadic (sacral) ones.
13 pairs of nerves to the rump.
5 specific ones of the cranium.

34 in all; whereas the number of the spinal vertebræ, which inclose the spinal cord is exactly 21.

There are five pairs of "extremities," organised after a common plan: (1) the lower, (2) the upper, (3) the temporal (bearing the lower jaw for a "member"), (4) the palate-facial, with the upper jaw for its "member," (5) the opercular or hyo-tympanic one, forming the gill-lid in fishes or the tympanic ossicles in man; and the digital extremity of which is gradually converted into the (hand-like) crimped (external and internal) cartilages of the ear.

The five pair of hæmal arches of the cranium, *i.e.* the gill arches of fishes, are gradually transformed into the gristles of the gullet, &c.

The main variation consists in the varying, but "cyclar" number of "rays," fingers, &c.; the varying cyclar number of their joints (1, 2, 5, 8, 13 respectively in a dolphin, with five carpels instead of eight, as in a man) and the varying cyclar number of "loose" ossicles, such as *carpals, tarsals, teeth*, &c. The number of spinal vertebræ is also variable, but not that of the cranial ones.

* Abstract of a Paper read at the Indianopolis Meeting of the American Association for the Advancement of Science, by Dr. T. C. Hilgard. Reprinted from the *American Naturalist*.

The vertebral blocks, as well as the ribs, are the product of the primitive axial series of (invertebral) discs, which, when completely arrayed, each bear five branches, viz., two pair of hæmal arches, two pair of neural arches, and a fascicle of parallel cleets, so to speak, which being cemented together, both in the front and rear, by the superficial ossification of the discs at either end, are fused into the block pieces, as found, *e.g.* in the young hog; the cementing slab covering the big neural rib head likewise, and not only the pentagonal prismatic block. The first disciform ossification we find in the corals, forming cribrose ethmoidal discs, such as the closely set "sigillate impressions" of the Astræa, and afterwards left behind as the coccyx, *e.g.*, of Byathophyllum.

*SIEMENS' DYNAMO-ELECTRIC LIGHT**

A SERIES of experiments was made last week at Sheerness, with a view of ascertaining the applicability of Siemens' dynamo-electric light to torpedo services in time of war. This scientific combination is produced, as its name signifies, by the application of excessively rapid motion generated from the fly-wheel of a steam-engine to a very powerful set of ordinary galvanic "coils" in connection with soft-iron magnets. The leather strap from a four-horse power engine, encircling a small gun metal pinion, causes it to revolve with the extreme velocity of 1,600 revolutions per minute, inducing motion in an electric "bobbin" at the side of an apparatus consisting of several sets of strong insulated coils. A stream of electricity consequently passes through them. This stream is conducted to a second series of coils, larger and more powerful than the first, which are also in combination with a pinion revolving 800 times per minute, thus intensifying the stream as it passes through them to a very considerable degree. Both negative and positive currents are now alternately given off from another "bobbin" at the side of the second series of magnetic coils, to the train of insulated wires, which conveys them to the position from which the dynamo-electric light is to be exhibited. Here there is a delicately contrived apparatus for containing the carbon points, between which the light is to be generated, adjusted at the top of a tripod somewhat similar in construction to that of a surveying instrument. At the back of the two carbon points, and "slotted" vertically to admit of their holders passing through it, is a concave reflector of white polished metal, which collects the rays of light into a focus, and transmits them in any required direction by means of an adjusting hand wheel below. A minute aperture in the centre of the reflector, precisely behind the junction of the two carbon points, throws a representation of the flame upon a piece of opal glass in a frame fixed at the back of the reflector; and through the agency of another small hand wheel which causes the carbon points to approach or recede from each other, the flame can be reduced or intensified at pleasure, by simply turning the wheel, care being taken at the same time to keep a watchful eye upon the picture produced, as the withdrawing of the points to too great a distance from each other will extinguish the light. It should have been remarked before that ample means are taken by lubricating the electrical apparatus to counteract the evil effects which might otherwise arise from the excessive friction consequent on the rapidity of motion in the several parts.

The object of instituting the series of experiments which were made on Monday was to ascertain if it was possible to throw such a stream of light upon an enemy's working parties engaged in interrupting communications with a line of torpedoes at night, as would render them sufficiently conspicuous to be fired at and consequently driven off. The place selected was the new fort at Garrison Point, Sheerness. The engine and "coils" were erected in the enclosure of the fort, while the instrument itself was placed in one of the massive embrasures piercing its sides. No sooner was steam got up and the order given to turn ahead, than the burring noise of the machine indicated that electricity was being rapidly generated, sparks and stars of vivid blue light being given off at the various joints. Another instant, and a vivid stream of light shot across the sea to a number of ships lying in the offing at a distance of about two miles, lighting them up with the brilliancy and distinctness of broad moonlight. The effect was magnificent. Clouds of mists, rendered visible by the intensity of the rays shooting through them, rolled across the broad field of bright light from time to time, not, however, interrupting the view in their progress. By shifting the direction of the rays laterally, each object in turn came within the compass of the portion of horizon rendered clear. In fact, it was sufficiently apparent that no objects of any appreciable size, such for instance as an enemy's boats, could come within a mile or more of one of Siemen's dynamo-electric instruments in operation without being rendered conspicuous to any battery in the vicinity, and consequently involving to themselves the most imminent danger. Hence the result of the experiments may be pronounced a success; whether, however, a corresponding effect might not be obtained by a succession of parachute lights thrown from a rocket or mortar is quite an open question.

* Reprinted from the *Times*.

PHYSICS

Note on the Spectrum of the Aurora

ON the evening of November 9 there appeared one of the most magnificent crimson auroras ever seen at this place. When first observed, at about a quarter before six P.M., it consisted of a brilliant streamer shooting up from the north-western horizon; this was continued in a brilliant red, but rather nebulous mass of light, passing upward and to the north. Its highest points were from 30° to 40° in altitude. A white aurora, consisting of bright streamers, appeared simultaneously, and extended round to the north-east.*

The crimson aurora was examined with the spectroscope at six o'clock. The instrument used was a single glass-prism spectroscope, made by Duboscq, of Paris. On directing the slit toward the brilliant streamer above mentioned, a bright spectrum was observed consisting of five well-marked lines. A millimetre scale attached to the instrument was then illuminated with a gas flame, the auroral lines being readily measured, even when the numbers on the scale were bright enough to be read distinctly. The sodium line was used to adjust the scale, being equally divided by the division 100; the width of the slit was about one millimetre. As thus arranged, the five auroral lines, beginning at the red end, had the following positions:—Scale-Nos. 90, 110·5, 130, 138, 149. The brightness of the lines was, following the above order, 3, 1, 5, 2, 4, the second line from the red end of the spectrum being the brightest. The line marked 90 and the one marked 110·5 were sharp and well defined; the others were all nebulous on the edges. Before the measurements were completely verified by a second comparison, the crimson aurora entirely vanished, having endured less than half an hour. In the white aurora which remained, the spectroscope showed four of the five lines given; the crimson line alone was absent. The measurements are exact to half a division of the scale.

To determine the approximate wave-lengths of these lines, comparison was made both with certain metallic lines and with the lines of the solar spectrum. On the scale of this instrument the metallic lines employed read as follows:—

Kα 63, Liα 79, Srβ 80, H(c) 82, Caα 91, Srα 96, Caβ 113, H(f) 146·5, Srδ 163, Csβ 165, Csα 167, Rbα, & β 200, Kβ 218.

The Fraunhofer lines measured as follows:—

a 70·5, B 76, C 82, D 100, E 124·5, b 130, F 146·5, G 189.

Direct interpolation was used in comparing the wave-lengths of the auroral lines with those given above, the wave-lengths of the Fraunhofer and elemental lines being taken from Gibbs's tables (Amer. Jour. of Science and Arts, II. xliii. 1; xlvii. 194). This method was believed capable of giving results as close as the instrumental measurements. In this way the wave-lengths of the five auroral lines were obtained, as given in the following table:

Line.	Scale number.	Wave-length.	Auroral lines.	Other measurements.
B	76	687		
C	82	656		
(1)	90	623	623	627 Zöllner.
D	100	589		
(2)	110·5	562	562	557 Angström.
E	124·5	527		
(3)	130	517	517	520 Winlock.
b	130	517		
(4)	138	502	502	
F	146·5	486		
(5)	149	482	482	485 Alvan Clark, Jr.
G	189	431		

* Professor Newton informs me that he observed an equally brilliant red patch of auroral light in the north-east, five or ten minutes earlier. Since the lower end of the red streamers was much lower than that of the white, it would seem as if the red were seen through the white, the red being most remote.

In this table, column 1 gives the auroral and the Fraunhofer lines; column 2, the number of these as measured upon the scale of the spectroscope used; column 3, the wave-lengths of these lines obtained as above stated; column 4, the wave-lengths of the auroral lines, given by themselves; and column 5, the wave-lengths of what I assume to be the same lines, with their wave-lengths as measured by the observers mentioned.

The point of particular interest in this observation is the fact that the line (4) of wave-length 502 is not laid down in any authority accessible to me as having been observed in the auroral spectrum. Indeed, no previous observer, so far as I know, has seen any auroral line between the Fraunhofer lines *b* and F. Professor C. A. Young (*Journal of Science and Art*, III. ii. 332, Nov., 1871) gives two lines—Nos. 56 and 57, or 1866·8 and 1870·3 of Kirchhoff—observed by him in the sun's chromosphere and also by Rayet in the eclipse of 1868, one of which may coincide with this fourth auroral line.

New Haven, Nov. 13 GEORGE F. BARKER

SCIENTIFIC SERIALS

THE *Geological Magazine*, Nos. 86—89, August to November 1871. This valuable magazine continues to furnish us every month with important and interesting articles upon subjects belonging to the various departments of geology. In the first number now before us we find an interesting paper on volcanoes by the editor, Mr. H. Woodward, and a particularly valuable article by Mr. J. W. Judd on the anomalous mode of growth of certain fossil oysters. In the latter, which is illustrated with a plate, the author notices those oysters from various secondary deposits, in which the shell has acquired throughout the peculiar sculpture of some ammonite, *Trigonia*, or other shell, to which its lower valve has adhered during growth.—In the September number the most interesting paper is Mr. Woodward's description of a new Arachnide from the Dudley coal-measures. This animal, to which the author gives the name of *Eophrynus Prestvicii*, is most nearly allied to the existing genus *Phrynus*, and the specimen is remarkable for the beautiful preservation of the casts of both surfaces.—Among the contents of the October number we must call particular attention to Dr. Murie's article on *Sivatherium*, in which the author discusses the characters of that most remarkable animal, which he regards as most nearly allied to the Saiga antelope, the latter being placed by him at the central point of ramification of the hollow-horned ruminants, and leading from the ruminants towards the Pachyderms through the Tapir. This valuable memoir is illustrated with two plates, one representing the skeleton of the animal, the other giving an ideal restoration of the living aspect of the male, female, and young of this gigantic ruminant.—The November number opens with a biographical notice (with a portrait) of Sir Roderick Murchison, followed by a shorter one of Mr. Charles Babbage. The other articles contained in it are on relics of the Carboniferous and other old land-surfaces, by Mr. Woodward; on the prospects of coal south of the Mendips, by Messrs. Bristow and H. B. Woodward; on the futile search for coal near Northampton, by Mr. S. Sharp; and on the Foraminifera of the Cretaceous rocks, by Messrs. T. Rupert Jones and W. K. Parker.

THE *Journal of Botany* for November commences with an interesting contribution to historical botany; in a paper read by the late Robert Brown before the Edinburgh Natural History Society in 1792 on "The Botanical History of Angus" never before printed. It was probably his first contribution to botanical science, having been written when he was about eighteen years old. Prof. Thiselton-Dyer contributes some observations on "Fungi parasitic upon *Vaccinium Vitis-Idæa*," and Mr. A. W. Bennett "Further observations on Protandry and Protogyny," in continuation of his previous researches on this subject. Mr. T. A. Briggs has a note on an undescribed species of *Rubus*, and the remainder of the number is filled up with short notes, abstracts, extracts, and reviews.

The number for December opens with the commencement of a paper by Mr. J. G. Baker "On the Botany of the Lizard Peninsula." Although this district is well known to botanists as the habitat of many very rare and local plants, yet no detailed account has yet been published of the flora of this portion of Cornwall. From the idea that many plants very common in other parts of England would find their limit short of this south-western extremity of the island, a list is here given of every flowering plant observed during a three days' visit, accompanied by general remarks on the peculiarities of the flora, both in what it includes and in what is absent from it. The only other original paper of importance in the number is a new arrangement by the Rev. J. C. Leefe of the English species of the extremely difficult genus *Salix*.

Journal of the Royal Geological Society of Ireland. Part 1, vol. iii. new series (vol. xiii.), has just been published. It contains besides the Report of Council for 1870-71, J. Emerson Reynolds on two remarkable Crystals of Galena; G. H. Kinahan, additional notes on Foliation, and supplementary notes on some of the Drift in Ireland; R. G. Symes, on the Geology and extinct Volcanoes of Clermont, Auvergne—plates i. ii. iii.; W. H. Baily, on the genus *Pleurorhynchus*, and a new species—plate iv.; M. H. Ormsby, Analyses of some Granite Rocks from India, and of their constituent minerals (1668); Edwd. T. Hardman, Analysis of Trachyte Porphyry from Tardree near Antrim, and on the Analysis of a Limestone compared with that of the same rock where it is in close proximity to a Doloritic Dyke; R. C. Tichborne, note on the Geological Formation of some of the Tiroxides.

Journal of the Chemical Society, October.—This number does not contain any papers originally communicated to the Society. The abstracts of foreign papers, however, occupy nearly 100 pages, and comprise many subjects of interest. The importance of the work done this way by the Chemical Society can scarcely be estimated; the journal must now be of great value not only to the chemist, but also to the physicist, physiologist, and to the chemical manufacturer, for many papers in these subjects are abstracted fully. An abstract of M. Berthelot's paper on the heat evolved in the formation of organic derivatives of nitric acid is very interesting. It is shown that in the formation of nitroglycerine, a very small amount of heat is evolved, as compared with that evolved in the formation of gun-cotton or nitrobenzine. This will account for the ready decomposition of the former, and the formidable effects produced by its decomposition. Amagat has experimented on the compressibility and dilatation of sulphurous and carbonic anhydrides; he finds that they first begin to act as perfect gases at a temperature of 250° C. Several of the abstracts contained in this number have already been noticed in these pages. One of them deserves especial notice, by Friedel and Ladenburg, on silicopropionic acid; this body is the first in which the group CO_2H contained in organic acids has been replaced by the corresponding group SiO_2H. Amato has obtained a curious compound, glucosophosphoric acid, the sodium salt of which has the composition $C_6H_{11}O_5Na_2PO_4$. Waage has published a paper on the use of bromine in chemical analysis, in which he points out that this reagent can be substituted with advantage for chlorine in many instances; it is very useful in decomposing pyrites, the whole of the sulphur being easily oxidised. We find an abstract of Bischof's paper on Fire Clays, which appears to deal very practically with this most important subject.

THE part just issued of the *Transactions of the Linnean Society*, completing vol. xxvii., contains three papers, of two of these, "Revision of the Genus *Cassia*," by Mr. G. Bentham, the president, and "Contributions to the Natural History of the *Passifloreæ*," by Dr. M. T. Masters, abstracts have already appeared in our columns. The remaining paper, "Notes on the Reptiles, Amphibia, Fishes, Mollusca, and Cretacea, obtained during the Voyage of H.M.S. *Nassau* in the years 1866-69," by Dr. Cunningham, contains descriptions of several new species collected and named by the naturalist to the expedition, and notes on the habits, localities, and characters of many other species. All these papers are illustrated by plates.

The first part of Volume xxviii. is also published, consisting of only a single paper, Dr. Triana's monograph of the *Melastomaceæ*. After some general remarks on the order, and on each of the genera comprised within it in French, follows an enumeration of the species, with the synonymy, references to type specimens in the principal herbaria, and fresh descriptions of new or badly-described species. It is illustrated by seven plates.

THE *Bulletin of the Royal Academy of Sciences of Belgium* for September and October, 1871 (Tom. xxxii., Nos. 9 and 10), contains but little scientific matter.—M. J. C. Houzeau communicates a description of a method of measuring directly the distance of the centres of the Sun and of Venus during the transits of that planet.—M. P. J. Van Beneden describes a new Sirenian from the Rupelian stage. The remains of this animal were obtained at Elsloo, near Maestricht, and consist of a portion of the cranium, one dorsal vertebra, and a series of seven caudal vertebræ. These are described and figured by M. Van Beneden under the name of *Crassitherium robustum*; he regards it as more nearly allied to the *Stelleræ* than to the Manatees and Dugongs. M. Van Beneden also notices the occurrence at Basel near Rupelmonde of a nearly complete skeleton of a Sirenian in brick-clay, and remarks upon the constant association of remains of *Squalodon* with those of Sirenians wherever the latter have been found in Europe. He also notices some points in the osteology of living Sirenia.—M. E. Van Beneden gives us a note on the preservation of the lower animals, in which he recommends the employment of solutions of osmic acid and picric acid for the preservation of the more delicate forms of animal life, such as the Medusæ, Ctenophora, &c. According to him these processes are most successful.

SOCIETIES AND ACADEMIES

London

Royal Society, December 21.—"Contributions to the History of Orcin.—No II. Chlorine and Bromine-substitution Compounds of the Orcins." By John Stenhouse, F.R.S.

"Note on Fucusol." By John Stenhouse, F.R.S.

Mathematical Society, December 14.—Dr. Spottiswoode, president, F.R.S., in the chair. Mr. K. Freeman, of St. John's College, Cambridge, was elected an ordinary member, and the following gentlemen foreign members of the Society:—Dr. Clebsch, M. Hermite, Prof. Cremona, Dr. Hesse, and Prof. Betti. Dr. Sylvester explained the methods he had employed in his paper, "On the theorem that an arithmetical progression which contains more than one contains an infinite number of prime numbers." The communication was limited to the case of arithmetical progressions proceeding according to the common difference, 4 or 6. The method employed appears to differ fundamentally from Dirichlet's method (Berlin Transactions, 1837). [In the account of Dr. Sylvester's previous communication to the Mathematical Society, given in NATURE, Nov. 23, p. 75, at line 18 from the commencement of the paragraph, for *intention* read *induction*, and at line 20 from the foot of the page, for the words *the magnitude* read *the order of the magnitude*.] Profs. Cayley and H. J. S. Smith took part in a discussion on the subject.—Prof. Clifford next spoke with reference to a paper he is preparing for the society.—Prof. Cayley then drew attention to the question of the determination of the surfaces capable of division into infinitesimal squares by means of their curves of curvature. It was shown by M. Bertrand that in a triple system of orthotomic isothermal surfaces each surface possesses the property in question, of divisibility into squares by means of its curves of curvature. But in such a triple system each surface of the system is necessarily a quadric. There is nothing to show that the property is confined to quadric surfaces, and the question of the determination of the surfaces possessing the property appears to be one of considerable difficulty, and which has not hitherto been examined.—Mr. S. Roberts exhibited a thread model of a homographic transformation of the developable surface which circumscribes a system of compound quadrics. The surface is generated by planes touching an ellipse at a constant inclination, and its equation is obtained by writing $p^2 z^2$ for r^2 in $\phi(x^2, y^2, r^2) = 0$ representing the plane parallel of an ellipse.

Anthropological Institute, December 18.—Dr. Charnock, president, in the chair. Lord Dunraven, Dr. John Best, and Mr. J. Kempe were elected members. A paper was read by Mr. Joseph Kaines on the "Anthropology of Auguste Comte." The sources of the paper were to be found in chapters on "Biology" and "Fetishism" of M. Comte's *Philosophie Positive* and in the *Politique Positive*. The paper itself aimed to show that the differences between man and the rest of the animal kingdom were not so great as they were usually represented, nor in fact were they so numerous in their resemblances. Treating man as the head of the zoological series, it argued that his dominion over animals was from primitive times (and is now) a moral dominion rather than intellectual, and it concluded, that in so far as external nature was used by man for moral ends, it was rightly used, and that the intellect found its true work in directing his affective nature to moral purposes and relationships.

Linnean Society, December 21.—Mr. G. Bentham, F.R.S., president, in the chair. "On the Anatomy of the American King-Crab (*Limulus polyphemus*, Latr.)," by Prof. Owen, F.R.S. The author, referring to anatomies of existing species of animals elucidating the type of structure of large extinct groups—as that of *Apteryx* in reference to the *Dinornithidæ*; of *Protopterus* in relation to the notochordal, protocercal Cycloganoids of palæozoic beds; of *Nautilus* as the representative of the constructors of extinct chambered and siphonated shells; of *Orbicula*, *Discina*, and *Terebratula* in like relation to extinct *Brachiopoda*—stated that, in reference to the Trilobite Crustacea, he had once doubted whether *Serolis* or *Limulus* would reflect most light on the internal structure of those ancient forms of the class. But, in the 14th lecture of the Hunterian Course of 1843, published in April of that year, appreciating the importance of the character by which the Xiphosures and Trilobites agreed in differing from *Malacostraca*, viz., in the numerical formula of segments, he decided to take *Limulus* in hand. Isopodal tendencies in Trilobites indicated, however, their more generalised character, and continued palæontological research led to the postponement of the original purpose, until the subsequent discoveries of a palæozoic group of Crustacea, due mainly to the labours of Salter, Huxley, and Woodward, decided the author no longer to delay the present communication, in view of its more special bearings upon the *Merostomata* of the last-named carcinologist. Of the external characters of *Limulus* but little was left to describe. The author accepted the evidence of the homologies of the three divisions of the body adduced by Dana, Spence Bate, and Woodward as outweighing that which influences V. der Hoeven. The "cephalothorax" of the latter author was the "cephalon," the second division was, not the "abdomen," but the "thorax," of the later carcinologists. The determination by the latter of the articulated appendages of the foremost division of the body of *Limulus* was also adopted. But as that division includes not only the brain, organs of sense, mouth, and manducatory instruments, but also the stomach, liver, major part of the heart, and genital organs, together with a long tract of the ventral ganglionic neural chords or centres, the author proposed to speak of it as the "cephaletron," the succeeding division as the "thoracetron," for the spine-shaped part he adopted Spence Bate's term of "pleon." In the description of the cephaletron, its modifications enabling it to act effectively as a burrowing digger or spade were dwelt upon, and the modifications of the hind border which articulates with the thoracetron were pointed out, showing that whilst by coalescence it was part of the foremost division in all its formal characters, more especially its upper pair of entapophysial pits and under pair of coalesced lamelliform appendages, it belonged to the series of lamelligerous segments constituting the thoracetron. The author then proceeded to give a detailed account of the muscular system of *Limulus*, and concluded this third section of the paper, by condensing notes made by Mr. Lloyd, of the Crystal Palace Aquarium, on the movements of living *Limuli* in captivity, and those made by Mr. Lockyer in New Jersey on the *Limulus polyphemus* in its native seas. The reading of this memoir will be continued at a subsequent meeting of the Linnean Society.

Manchester

Literary and Philosophical Society, November 28.—Dr. J. P. Joule, F.R.S., vice-president, in the chair. "Encke's Comet and the Supposed Resisting Medium," by Professor W. Stanley Jevons. The observed regular diminution of period of Encke's comet is still, I believe, an unexplained phenomenon for which it is necessary to invent a special hypothesis, a *Deus ex machina*, in the shape of an imaginary resisting medium. I cannot be sure that the suggestion I am about to make has not already been made, but I have never happened to meet with it; and therefore I venture to point out how it seems likely that the retardation of the comet may be reconciled with known physical laws. It is asserted by Mr. R. A. Proctor, Prof. Osborne Reynolds, and possibly others, that comets owe many of their peculiar phenomena to electric action. I need not enter upon any conjectures as to the exact nature of the electric disturbance, and I do not adopt any one theory of cometary constitution more than another. I merely point out that if the approach of a comet to the sun causes the development of electricity arising from the comet's motion, a certain resistance is at once accounted for.

Wherever there is an electric current, some heat will be produced and sooner or later radiated into space, so that the comet in each revolution will lose a small portion of its total energy. In the experiments of Arago, Joule, and Foucault, the conversion of mechanical energy into heat by the motion of a metallic body in the neighbourhood of a magnet was made perfectly manifest. If then there is any magnetic relation whatever between the sun and the comet, the latter will certainly experience resistance. The question is thus resolved into one concerning the probability that a comet would experience electric disturbance in approaching the sun. On this point we have the evidence now existing that there is a close magnetic relation between the sun and planets. If, as is generally believed, the sun-spot periods depend on the motion of the planets, a small fraction of the planetary energy must be expended. I find, indeed, that a very brief remark to this effect was given in the memoir of the original discoverers of the relation, namely, Messrs. Warren De La Rue, Balfour Stewart, and B. Loewy. At p. 45 of their Researches on Solar Physics they add a small note to the following effect: "It is, however, a possible inquiry whether these phenomena do not imply a certain loss of motion in the influencing planets." As I conceive, no doubt can exist that periodic disturbances depending upon the motions of bodies must cause a certain dissipation of their energy; for if stationary the constant radiation of the sun could not produce any periodic changes, unless the sun were itself variable. Is there not then a reasonable probability that the light of the aurora represents an almost infinitesimal fraction of the earth's energy, and that in like manner the light of Encke's comet represents a far larger fraction of its energy? It is also worthy of notice that the tail of a comet is usually developed most largely at those parts of its orbit where the rate of approach or recess is most rapid, and where the electric disturbance would be correspondingly intense. I do not, of course, deny that the resisting medium may nevertheless exist, or may by other observations or experiments be made manifest. But I hold that so long as other physical causes can be pointed out which might produce the same effect, it is quite unphilosophical to resort to a special hypothesis. Encke's comet ought not to be quoted as evidence of the existence of such a medium until electric disturbance is shown by calculation to be insufficient to account for the observed diminution of period.

LIVERPOOL

Geological Society, November 14.—Dr. Ricketts, president, in the chair. Mr. T. Mellard Reade, C.E., on the "Geology and Physics of the Post-Glacial Period, as shown in the Deposits and Organic Remains in Lancashire and Cheshire." The paper was largely illustrated by maps and sections. The author's views are summarised in the following conclusions:—1. That since the glacial period there are distinct evidences in Lancashire and Cheshire of three periods of depression or downward movement, and two periods of elevation or upward movement. There may also have been a period of elevation and a land surface previous to any of these movements, but posterior to the true glacial times. 2. That the first period of depression, which was the greatest, submerged the land to a minimum of 1,500 feet below its present level—in Wales at least—and was doubtless general. The post-glacial shells of Moel Tryfane and those by the Ribble, indicating ancient beaches, belong to this period. During this time, and the re-emergence of the land, what the author termed the "washed drift sand" was eliminated from, sorted, and reformed out of, the boulder drift, and scattered over the country, but has since been much denuded by atmospheric and aqueous or sub-aërial influences above the 25 feet contour, and by sub aërial and submarine denudation below that line. 3. A re-emergence of the land took place, and a land-pause favourable to growth occurred, during which time the "inferior peat and forest beds," or sub-terrene land surfaces, were formed. At the period of pause the land would be higher than now, but the vertical extent of this movement the author purposed investigating hereafter. 4. A second period of subsidence again followed, and a pause occurred at or about the 25 feet contour line. "The Formby and Leasowe marine beds" were now laid down. 5. A second or latest vertical upward movement followed, elevating the Formby and Leasowe marine beds, upon which now grew the forest trees, the remains of which assist to form the "superior peat bed" extending along the coast margin from the river Douglas to Bootle in Lancashire, and from the Mersey to the Dee in Cheshire, and remains of which are found as high up the river Mersey as Garston and Warrington. 6. The third or latest downward movement now took place, and during this time the river bed at Crossens was silted up, as also the Garston Creek. The drainage was obstructed, and the beds of marine silt intercalated in the peat. The tidal silt overlying the superior peat bed by the Douglas, the Alt, and the Birket, the silt which overlay the peat bed of Old Wallasey Pool, and that in which the vertebræ of a whale, now in Brown's Museum, were discovered at the North Docks, and all the deposits to which the author confined the term recent, belong to this period, in a pause of which we are now living. 7. That the whole of these movements were uniform over a far more extensive area than the author has investigated, he has not the shadow of a doubt. That post-glacial movements were slow is almost universally admitted, and from these the inference is obvious that the time which they measure compared with the historical period is so vast that it is difficult to form an adequate conception of it.

NORWICH

Norfolk and Norwich Naturalists' Society, October 31.—Mr. J. E. Taylor read a paper on "The Origin of the Norfolk Broads and Meres." With regard to the former, Mr. Taylor propounded the theory that the depressions, so-called, were owing to the influence of ice in remote ages, and that the basins thus scooped out had been since filled up by the growth of peat and the soil brought down by floods. His views were supported by an elaborate essay upon the probable condition of the European continent at the close of the glacial epoch, and the alterations effected by "the last geological change in its physical scenery and geography," as illustrated by the deep lakes of "Switzerland, Scotland, Cumberland, &c., hollowed out of the solid rocks by glacier action." He specially referred also to the great similarity in the physical aspect of the Dutch coast as compared with the Broad district of our eastern counties. Broads, he remarked, were distinguished from meres by being always in connection with rivers, and having a chalky bottom, more or less filled in with deposits of mud. Meres, on the contrary, in their physical characters, presented an almost entire separation from rivers and streams, "and the fact that they usually lie in the upper boulder clay, and therefore at a considerably higher level than the broads. The water supply of meres was simply the storage of wet seasons." The number of broads on the Bure and its tributaries, amounting in all to twenty-two, as compared with but four on the Yare, he attributed to the former stream having an average breadth of 150 feet, and the latter of only 100 feet. The formation of Diss Mere he considered due to glacial action, "as the neighbourhood abounded in evidences of such phenomena."—Mr. J. H. Gurney, jun., exhibited a male specimen of White's Thrush (*Oreocincla aurea*), killed on the 10th October last, by Mr. F. Barrett, in a marsh at Hickling, and exhibited by permission of the Rev. J. Micklethwaite, for whose collection it is being preserved by Mr. T. E. Gunn. Mr. Gurney pointed out the distinctions between the closely allied genera of *Oreocincla*, *Turdus*, and *Merula*, and made some remarks on *O. aurea* as a British species. It is, he said, the *Turdus Whitei* of Egton, and of Yarrell's "British Birds," so called after the well-known naturalist of Selbourne, and has been killed in six or seven instances in this country, the specimen exhibited being the first recognised as occurring in this country. It is found in China, and is said to have been met with in Siberia.—Mr. Barrett exhibited specimens of *Zygæna exulans*, a Swedish moth recently taken in Scotland.

DUBLIN

Royal Irish Academy, December 11.—Prof. Henry Hennessy, F.R.S., vice-president, in the chair. Prof. Robert S. Ball read two notes on applied mechanics. In the first note it was demonstrated that in whatever manner a figure moves in a plane, a number of points, lying on the circumference of a circle, are any instant in points of inflexion of the curves which they describe, and that the points of the circle are at points the tangent to which meets the curve in four consecutive points. These theorems embrace what are known in mechanics as the parallel motions. The second note contained an elegant geometrical construction by which the consecutive points of contact of two curves are determined.—The Secretary then read a paper by Mr. Hodder M. Westropp, in which the writer stated that he had abandoned his former theory that the Ogham inscriptions had a Danish origin, and now suggested that after all the learned interpretations that had been attempted of their meaning, they were nothing more than notches made to mark the number of cattle possessed by the owner of a plot of land at the annual division which took place under the ancient Brehon laws of Ireland. It was simply a rudimentary scoring of numbers, such as had taken place amongst all nations in the earliest stages of civilisation. There was no substantial reason for attributing to the Irish, who, even at the time of Giraldus Cambriensis, had scarcely emerged from barbarism the formation of an alphabet, and the attempts to decipher the inscriptions by attributing to them an alphabetic character were simply absurd. Dr. Ferguson, Q.C., said he was sure that if Mr. Westropp knew anything of the circumstances in which these inscriptions were found he would not have put forward such a theory. One of the very examples to which he referred in his paper proved the inaccuracy of his statement that these stones had not been found in connection with gravel. It was quite evident that in his illustrations he had worked from very imperfect copies, for his illustrations misrepresented the inscriptions. This was a case of a wild theory started without a fact being adduced in support of it.

Royal Geological and Zoological Societies of Ireland.—A joint meeting of these societies was held on Wednesday, the 13th of December, 1871, William Ogilby, M.A., F.G.S., in the chair. W. H. Baily, F.L.S., read some additional notes on the Fossil Flora of Ireland. The author first described a new fossil plant from shale in the carboniferous limestone of Whitestone Quarry, near Wexford, under the name of *Filicites plumiformis*. He then gave the results of his examination of the collections made from upper Old Red sandstone strata at Kiltorcan, Co. Kilkenny, which collections had excited considerable attention among the Continental and American botanists, and brought forward some strong facts to prove that the Irish palæontologists had not misled Prof. Heer, as stated by Mr. Carruthers at a recent meeting of the London Geological Society.—Prof. Traquair read some notes on the genus *Phaneropleuron*.

Vienna

I. R. Geological Institution, November 21.—The Director, Fr. Ritt. v. Hauer, read the anniversary report on the progress made by the Institute. The surveyors were occupied in the course of the last year on two different regions; the military frontier, where the geological maps of the country between Brod in Slavonia, and the shore of the Adriatic were finished, and Tyrol, where parts of the crystalline central mountain region and of the northern limestone ranges were surveyed. At the request of private proprietors, the members of the Institute were occupied besides with particular inquiries as to the nature and extent of coal-seams, strata and veins of ores and other useful minerals in almost all parts of the empire, and a very accurate examination of the rocks which are to be perforated by the Arlberg Tunnel, between Tyrol and Varalberg, was made by M. H. Wolf. In the museum of the Institute the larger collections of minerals from the different mining districts of the empire were completely re-arranged, and a magnificent collection of fossil Mammalia, from the tertiary brown coal of Eibiswald in Styria, was exposed under glass. More than forty different persons have contributed by donations to the increase of the various collections. In the Chemical Laboratory more than 100 analyses and assays have been performed for about fifty parties. A new arrangement of the library was finished in the course of the year; with the end of 1870 it numbered 6,500 different works, with about 16,500 volumes; in the first ten months of 1871 the increase amounted to more than 12,000 volumes. The collection of Maps (besides those which were made by the Institute itself) consisted, at the end of 1870, of 2,850 sheets, and has since increased by nearly 300 sheets. The publications of the Institute were enlarged by a new periodical, the "Mineralogischen Mittheilungen," which is edited by Dr. G. Tschermak, the director of the Imperial Mineralogical Museum; they appear separately as well as in the form of a supplement to the "Jahrbuch." The publication of the memoirs ("Abhandlungen") of the Institute, which had been interrupted, was also recommenced this year by the publication of two memoirs: one by Dr. Neumayer, "On the Cephalopods of the Jurassic Beds of Balin, near Krakaw;" the other by Dr. Bunzel, "On the Vertebrata of the Cretaceous Formation of Grünbach in Austria." Of the general geological map of Austria, edited by Fr. v. Hauer, appeared sheet No 3 (the northern Carpathians), and the printing in colours of sheet No. 7 (the Hungarian plain) was finished. Dr. Neumayer noticed the discovery of the salt formation in the valley of Hall in Tyrol, at a point far below the salt mines now being worked. Here the mining work would meet with considerably less difficulty, arising from the great height of the old mine (5,000 feet above the level of the sea) the access to which in winter time is always dangerous, often even impossible.—M. Charl. v. Hauer read a note on a very successful boring for coal in the tertiary basin near Fohnsdorf in Styria. On the northern edge of this basin, many years since, a large seam of coal had been worked. The bore-hole had been opened in the midst of the basin, 300 fathoms from the nearest point of the mine. At the depth of 155 fathoms the coal was reached in two seams, having together a thickness of 5½ fathoms. This discovery is of great importance for the industry of Upper Styria.—Dr. E. Tietze "On the Eocene Formation south of Glina, in Croatia." It consists of three members; the lowest a fresh-water deposit, with Planorbis, and traces of coal; the middle, green sandstones alternating with marly beds, probably identical with the so-called Albarese or Galestro of the Appennine mountains; and the upper, formed of slaty sandstones with fucoids.

DIARY

THURSDAY, December 28.

Royal Institution, at 3.—On Ice, Water, Vapour, and Air. No. I. Prof. John Tyndall, F.R.S.
London Institution, at 4.—The Philosophy of Magic. 2. The Magic of the Theatre: J. C. Brough, F.C.S.

SATURDAY, December 30.

Royal Institution, at 3.—On Ice, Water, Vapour, and Air. No. II. Prof. John Tyndall, F.R.S.

MONDAY, January 1.

Anthropological Institute, at 8.—On the Hereditary Transmission of Endowments: George Harris.—The Adamites: C. Staniland Wake.

TUESDAY, January 2.

Zoological Society, at 9.
Society of Biblical Archæology, at 8.30.—Hebræo Ægyptiaca; or, Hebrew and Egyptian Analogies: M. Francois Chabas.—Some Observations upon the Inscription of Daly (Idalion): S. Birch, F.S.A.

WEDNESDAY, January 3.

Microscopical Society, at 8.—Fossils of the Coal Measures; W. Carruthers, F.R.S.—Fermentation and its results: James Bell.

THURSDAY, January 4.

London Institution, at 4.—The Philosophy of Magic. 3. The Magic of the Mediums: J. C. Brough, F.C.S.

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NOTICE

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THURSDAY, JANUARY 4, 1872

BRITISH PREPARATIONS FOR THE APPROACHING TRANSIT OF VENUS

IN nearly all those countries of Europe in which Astronomy is nationally cultivated, preparations are being made for thorough observation of the first of the coming Transits of Venus, which will occur on December 8, 1874. In Russia, whose territory presents many favourable points for observation of the phenomenon, a committee, organised by Professor Struve, has had under consideration during the past two years the establishment of a chain of observers at positions 100 miles apart along the region comprised between Kamschatka and the Black Sea. The principal astronomers of Germany have held two conferences, each of several days' duration, which have resulted in a decision to furnish four stations for heliometric observation of the planet during its transit: one of these will be in Japan or China, and the others probably at Mauritius, Kerguelen's and Auckland Islands; and three of these, with the addition of a fourth station in Persia, between Mascate and Teheran, will be equipped for photographic observations also. A French commission on the subject sat before the war, and reported to the Bureau des Longitudes that it was desirable for their government to provide for observing stations at Saint Paul's Islands, and Amsterdam, Yokohama, Tahiti, Noumea, Mascate, and Suez. Since the close of the war the subject has been reverted to, and lately the Academy of Sciences applied to the Government for the requisite funds; but these could not be granted till next year, the budget for 1872 having been disposed of.

The British preparations, to which we shall chiefly confine our remarks, are, we believe, in a more advanced state than those of any other country. This forwardness may probably be ascribed to the circumstance that they have from the first been directed by a single mind, and have thus been freed from the inevitable delays of a committee. The Astronomer Royal first called attention to the Transits in 1857 and again in 1864. In 1868 he commenced to shape definite plans, selected the observing stations which were in all respects most suitable for British occupation, and opened communications with the Government upon the financial requirements of the undertaking.

Presuming a general acquaintance with the phenomenon under notice, and its availability for determination of the parallax of Venus, and that of the Sun (a subject that has been well popularised), we merely remark that there are several methods by which observers at opposite points on the earth may measure the parallactic displacement of Venus upon the Sun's disc: (1) by durations of Transit (Halley's method); (2) by absolute local times of ingress and egress (Delisle's method); (3) by heliometric measures of the planet referred to the limbs of the sun; (4) by similar measures obtained from photographs of the sun with the planet on his disc. The first of these has been considered disadvantageous for the 1874 Transit, which is the one that immediately concerns us. The third and fourth are of recent suggestion, and have been regarded as of doubtful accuracy, especially the fourth, whose reliability is still the subject of experimental inquiry. The second was the one which demanded foremost attention. The Astronomer Royal, therefore, as a first step, set down the stations best available for its application. These had to be selected in order to combine a sufficient altitude of the sun with the maximum attainable acceleration of ingress and retardation of egress on one side of the earth, and retardation of ingress and acceleration of egress on the other side of the earth. And after weeding the lists for each phase of such stations as were expected to be provided for by foreign governments, and of those already occupied by established colonial observatories, it was found that there were five stations which it was desirable that England should prepare to equip. These were Woahoo (for observation of accelerated ingress), Kerguelen's and Rodriguez Islands (for the retarded ingress), Auckland in New Zealand (for the accelerated egress), and Alexandria (for the retarded egress).

Now, as at all these places the absolute local time of the phenomenon is required, it is indispensable that the longitude of each be very exactly known. In no one case does a sufficiently accurate determination of this element exist, and provision must therefore be made in each case for obtaining it. This vastly increases the extent of preparations for the instrumental equipment of the stations, and renders necessary a three or four months' sojourn of the observers at each. Of the methods for determining longitude which were open to choice, the Astronomer Royal decided to employ that by vertical transits of the moon, and for observing these he resolved upon supplying altitude instruments with fourteen-inch circles and telescopes of twenty inches focus. For time determinations he proposed three-inch transits, of thirty-six inches focus, with clocks of moderately high class For observing the phenomenon he elected to employ at each station one six-inch equatorial and one four-inch portable telescope. For these an observatory of three rooms was required. With the exception of one altazimuth, two clocks, and two or three four-inch telescopes, which the Greenwich Observatory could furnish, all the specified instruments and the observing rooms had to be specially provided. An estimate for their purchase and construction, amounting to 2,154*l*, was therefore submitted to the Admiralty. Some needful chronometers and meteorological instruments were available from home stores. To the above estimate for material requirements were added others, prepared by Admiral Richards, for the personal expenses, the conveyance, residence, pay, and contingencies, of the observing parties. These amounted, for the Woahoo detachment, to 2,500*l*., for the Rodriguez and Kerguelen's parties to 2,000*l*. each, for the Auckland party to 1,000*l*., and for Alexandria to 750*l*., making a total of 8,250*l*. The grand total of 10,500*l*. was asked of the Treasury in May 1869, and immediately granted.

The construction of the requisite instruments and clocks was forthwith commenced, by Messrs. Troughton and Simms and Messrs. Dent. Three six-inch equatorials happening, however, at the time to come into the market, they were at once purchased; one of the three being that which is known to fame as the "Lee Equatorial," and is the instrument used by Admiral Smyth in the preparation of his "Celestial Cycle." The observatories were put in hand also. They are somewhat substantial structures, formed of a stout wooden framework, covered with weather-boarding and roofed with zinc and roofing-felt. Each instrument has a separate hut. The transit huts are ten feet square, with walls six feet high, and with the shutter openings a little on one side of the centre, so as to leave good room for mounting the clock, &c. The altazimuth huts are planned on a nine-feet hexagon. Their domes are hexagonal pyramids erected on circular frames, which are grooved to run on six-inch rollers. These rollers, six for each dome, are mounted on the wall-curbs. One flap-back shutter gives sky view from the horizon to the zenith. Each hut is made portable by being constructed in sections which are connected together by bolts and nuts. For the transit instruments massive Portland stone piers and foundation slabs have been provided; for the altazimuths stone pier-caps only will be sent out, leaving the piers to be provided on the spot. Every part of each observatory and every packing case has been numbered and marked by stencilling, with a letter to denote the station for which it is destined.

These transit and altazimuth observatories, with their instruments and the primary clocks, are, with trifling exceptions, in perfect readiness for use. The equatorials are generally ready, though their final completion has been interrupted by the loan of portions of them to the observers of the recent solar eclipse. The telescopes will be supplied with the Astronomer Royal's prismatic eye-piece for correction of atmospheric dispersion, which will necessarily be considerable at the low altitudes at which some of the contact observations must be made. The equatorial observatories are not yet constructed; the plans for them are under consideration as we write. The four-inch telescopes, some second-class clocks for use with the altazimuths and equatorials, and the small accessories, have also to be provided.

It is early to speak of the *personnel* of the various observing expeditions. Officers of the army and navy will probably compose a large proportion of the observing corps. Several gentlemen of the Royal Artillery have already commenced practice at Greenwich with the time and position instruments; but, with the object of forming a more accessible school of observation for them, a temporary observatory has been fitted up near to their headquarters at Woolwich.

Photography was not included in the Astronomer Royal's original plans. But from the time that his preparations were first mooted, the probable advantages of photoheliometry of the planet during transit were strongly insisted upon. The plans for photography were advanced from photographic quarters; astronomers of the exact class who were not photographers were somewhat sceptical at the outset concerning its accuracy. They anticipated that uncertainties would attach to the photographic measurements: in the first place from optical distortion of the image formed by the camera-telescope; in the second place, from mechanical distortion produced by unequal shrinkage of the collodion film, which must receive its impression in the wet state, whereas the measurements must be taken when it is dry; and in the third place, it appeared doubtful whether sufficiently accurate scale measurements could be secured to make the results equally reliable with those to be obtained from eye observation of the contacts. No method of secondary accuracy could be tolerated, since the received value of the solar parallax ($8''\cdot95$) is probably much less than 1 per cent. in error. It is considered that an eye-observation of contact, *i.e.* of formation or rupture of the "black drop," can well be made with no greater error than 4 seconds of time. As Venus moves over the sun at the rate of about $2''$ in a minute of time, the 4 seconds correspond to a displacement of $0''\cdot12$ of arc in the direction of motion, or about $\frac{1}{14000}$ of the sun's diameter. Can the measurements from a photograph, with all the above noted chances of error, be relied upon for such small quantities? It is argued that they can. The probable error of a single micrometric measurement of the photographic distance of the images of a double star is cited by Mr. Asaph Hall* to be $0''\cdot12$, and Mr. De La Rue, who is naturally the English referee in such matters, has no hesitation in saying that the measurements from a solar photograph *may* be depended upon, with all due precautions, to the $\frac{1}{16000}$ of the sun's diameter. He is of opinion that the shrinkage of the collodion film takes place only in the direction of its thickness, and he considers that if any optical distortion exists, it may be determined, and the correction for it found, by photographing a scale of equal divisions upon different parts of a plate, and comparing micrometric measurements of the various images. Upon this point he is about to make some crucial experiments with a large scale erected upon the Pagoda at Kew, and photographed from the Kew Observatory with the image in all positions on the sensitive plate. Herr Paschen is also investigating the matter on the part of the German Commission, using for his test-scale a glass plate divided into squares by diamond-ruled lines. Some preliminary trials have convinced him that should it be impossible to get rid of distortion, it will yet be easy to correct for it as accurately as may be desired.

Although the thorough reliability of the photographic method has not yet been satisfactorily established, the doubts concerning it have been in part removed, and it has appeared undesirable to neglect photography in the face of the circumstance that it might be the means of obtaining some useful record of the transit at stations where from atmospheric causes the observations of contact may be lost or vitiated. Moreover, as other nations had decided to employ the photographic method, it seemed incumbent upon Britain to work in harmony if not in actual concert with them; for although there has as yet been no formal proposal for international co-operation, there have been communications between the astronomical authorities of the various countries concerned, which have prevented the formation of very divergent plans. The Astronomer Royal therefore laid the subject before the Board of Visitors of the Greenwich Observatory, at their meeting in June last, and it was fully discussed by them. They resolved that it was desirable to furnish all the English stations chosen for eye observations with the necessary photographic appliances, and an application was shortly afterwards addressed to the Treasury for a grant of 5,000*l.* to defray the expenses of the additional equipment. The money was granted, and the construction of the photo-heliographs—five in number—was forthwith placed in Mr. Dallmeyer's hands. These instruments will be of generally

* *Silliman's Journal*, vol. cii., p. 26.

similar design to one made by the same artist for the Wilna Observatory, which has produced sun-pictures that, so far as the eye can judge, leave nothing to be desired in point of sharpness of definition and freedom from such distortion as the photographed cross-wires can exhibit. The object-glasses will be of about 4in. diameter, giving focal images of the sun about half an inch in. diameter. The focal image will be amplified to about 4in diameter on the photographic plate, and, in applying the enlarging lens, Mr. Dallmeyer is confident that he can entirely destroy the spherical aberration. The camera-telescopes will be mounted on equatorial stands, with latitude adjustment of 80° range; and they will be furnished with driving clocks.*

For the general photographic organisation, the Astronomer Royal has secured the co-operation of Mr. De La Rue, under whose able supervision the instruments above mentioned will be constructed, and by whom the various details of the photographic scheme will doubtless be arranged. Of the five stations already selected for eye observation of contacts, three are well suited for photographic record. These are Rodriguez, Kerguelen's, and Auckland, at all of which the whole transit will be visible. The Hawaiian station and Alexandria, though they are available, are less advantageous than the rest, because only a portion (about half) of the transit will be visible from each, and the photographs, besides being thus limited, must be obtained at low altitudes of the sun. It may become a question whether the heliographs provided with a view to furnishing these two stations cannot be more advantageously located. But before the positions are finally decided upon, it appears desirable that the intentions of other nations should be fully known, or, as would be preferable, that the ultimate distribution of observers of all kinds—telescopic, heliometric, and photographic—should be made the subject of an International Conference.

J. CARPENTER

* There are grounds for hoping that the same artist will construct some precisely similar photo-heliographs for other countries, for use on the Venus Transit. There would manifestly be great advantage in the employment by all photographing observers of instruments whose optical portions at least are of identical material and manufacture.

JUKES'S MANUAL OF GEOLOGY

The Student's Manual of Geology. By J. Beete Jukes F.R.S. Third edition, re-cast, and in great part re-written. Edited by Archibald Geikie, F.R.S. (Edinburgh: A. and C. Black, 1872.)

IF there be any one feature more strongly marked in the present age than another indicative of progress and intellectual advancement, it is the superiority of most (we will not say of all) of the books intended to promote education. School books and class books of all kinds, instead of being merely reprints, as in the days of yore, now really undergo revision every five years or so, or are superseded by new ones; whilst the introduction of natural science teaching into our Universities and public schools has created a demand for text-books to an extent greater even than the supply.

Among the various writers of the day on the science of Geology, Sir Charles Lyell must undoubtedly be placed in the front rank, as having done more than any other man to promote its study, and his "Principles" and "Elements" of Geology still hold the highest places in our estimation; but we must not forget that Phillips, Dana, and Jukes have also furnished us with geological manuals, more elementary in their style and arrangement, and therefore more serviceable for beginners than are Lyell's works. In order, however, to remedy this, Sir Charles Lyell has lately brought out a "Student's Elements of Geology," 8vo. pp. 624 (Murray), being an abridged edition of his larger work. This will no doubt prove a very useful book to beginners as an introduction to the higher class books.

Jukes's "Student's Manual of Geology" was born in 1857, and has already gone through two previous editions, each time, as is the sad fate of such books, growing more corpulent, till the poor student turns pale before the vast array of facts, neatly arranged for him to "cram," in the smallest possible type.

The original design contemplated in 1854 was an article on Geology for the "Encyclopædia Britannica," to have been carried out by the late Prof. Edward Forbes and Mr. J. Beete Jukes conjointly; but the death of Forbes for a time deferred the task. It was afterwards inserted in the Encyclopædia under "M," as "Mineralogical Science," and finally appeared as a separate work in 1857. The first edition is comprised in 610 pp., and is illustrated by 74 woodcuts, chiefly diagrams and sections of rocks, &c.

The second edition appeared in 1862, having grown an inch in the size of its page, and added 154 pages to its bulk, partly owing to the addition of 100 more illustrations, 50 of which are of fossils, or rather groups of fossils.

The idea of these figures of "Fossil groups," as they are termed, seems to have been taken from the admirable series of little woodcuts which illustrate the invertebrate portion of Owen's "Palæontology,"* prepared by the late Dr. S. P. Woodward. They are, however, arranged stratigraphically in Jukes's "Manual," not zoologically, as in Owen's "Palæontology."

The third edition, now before us, is only fourteen pages thicker than the second edition, and contains thirty-one more illustrations; but the bulk of matter is vastly increased by the use of smaller type than in the former editions.

The illness which seized Mr. Jukes, and by which he was removed from among us, had already impaired his health so much as to render it desirable he should be relieved of the labour of completing this edition, and the task was accordingly, by the author's own wish, undertaken by Professor Geikie, Director of the Geological Survey of Scotland.

The eighty pages on mineralogy (forming chapters II. and III.) have been entirely re-written by Dr. Sullivan; Chapter XIII., on trap-rocks, has been re-written by Prof. Geikie, as well as many other parts. Mr. Hull has revised the description of the English Coal-measures. Messrs. Bristow, Whitaker, and Judd have looked over the Mesozoic and Cainozoic chapters, and Prof. Huxley has contributed a new synopsis of the animal kingdom.

By a modification of the former edition, a new part is introduced (Part II.) called "Geological Agencies, or Dynamical Geology," a part of which also is from the pen

* Second Edition, 1861 (Edinburgh: A. and C. Black).

of Prof. Geikie, and now appears for the first time. It treats of the origin of hills, lakes, valleys, caverns, passes, fjords, glaciers, river-deposits, sea-action, coral-reefs, and all the many phenomena which either are themselves the cause, or the effect, of geological agents.

We have such a strong feeling against making a reference-book, especially one intended for the use of students, too bulky to be conveniently handled, and even carried about with one, as is frequently needful, that we have looked most closely into the present edition to see in what way it may be reduced without injury, bearing in mind that it only purports to be "a Student's Manual of Geology."

Candidly, then (without the least disrespect to Dr. Sullivan), we think the two chapters on chemistry and mineralogy (chapters II. and III., occupying eighty-one pages) should have been omitted. For these sciences, although so intimately related to, and constantly extending their aid to geology, are equals in rank and importance as sciences, and the student, if intending properly to master them, must possess such text-books as Williamson's Student's Chemistry and Dana's System of Mineralogy, books of equal importance in these sciences to Lyell's or Jukes's geological works.

As might naturally be expected in a text-book framed by a Geological Surveyor deeply versed in all the intricacies of rock structures in the field, and constantly dealing with stratigraphical questions, the book treats most largely of physical geology, not, however, to the exclusion of palæontology; yet exalting petrological science—at present in its infancy—into a far higher place than it has hitherto occupied in any other similar work. We do not wish it to be understood that we desire to undervalue lithological characters, especially in rocks devoid of organic remains; but we find such conflicting opinions prevalent among petrologists, that we are led to doubt the possibility of teaching much of such a branch of geological science to the student until the nomenclature of the principal rocks is settled by a congress of geologists, mineralogists, and chemists, or by some other authoritative body.

If in a new edition the mineralogy is omitted, we would suggest the introduction of a glossary of geological and zoological terms, which the beginner would, we feel sure, be very grateful to find added to the index, as an addition to the valuable tables of classification contributed by Prof. Huxley.

We heartily recommend the book to both intending teachers and students, who will find it a most complete compendium of geological science, and still one of the best Manuals in our language, as it has now been brought by its editor, Prof. Geikie, fairly "abreast of the onward march of science." H. W.

BREHM'S BIRD-LIFE

Bird-Life. By Dr. A. E. Brehm. Translated from the German by H. M. Labouchere, F.Z.S., and W. Jesse, C.M.Z.S. Parts I.—III. (London: Van Voorst, 1871.)

MR. WILLIAM JESSE, at the instigation of his colleague, is doing his best to make a silk purse out of—well, we do not wish to be rude, so let us say, out of materials of which silk purses are not commonly made; for Dr. Alfred Edmund Brehm has the fatal facility of being able to write endless nonsense on a subject which, in better hands, might be made truly instructive. He is so far from being a true naturalist that he is constantly being misled, confounding analogies with homologies. Take his second paragraph, as Mr. Jesse translates it, and translates it very well too:—

"Birds have much in common with mammals; and it is certain that some striking resemblances between individuals of both classes cannot be denied. Every impartial observer must recognise in the eagle the image of the lion, or rather its true representative in the bird-world; in the owl we see the cat; the raven resembles the dog; the vulture, the hyæna; the hawk, the fox; the parrot, the monkey; the crossbill, the squirrel; the wren, the mouse; the butcher-bird, the weasel; the bustard, the stag or antelope; the ostrich, the camel; the cassowary, the llama; the dipper, the water-rat; the duck, the duck-billed platypus; the diver, the otter; the auk, the seal; and so on. In spite of all these resemblances, which, after all, only apply to the external aspect, the bird is always and essentially distinct from mammals" (p. 2).

What, then, is the use of all this? Even the translator has to append a note stating that the author has not truly explained what he is writing about, and, indeed, it is plain that the writer to whom such ideas as the foregoing occurred has no pretension to be accounted a scientific man. Their association jars upon the feelings and contravenes the knowledge of any student of morphology. We have no wish to shock our readers even with the commonest terms of German philosophy, but is it not clear that to draw a parallel between a raven and a dog, and between a butcher-bird and a weasel, while a fox is likened to a hawk and a water-rat to a dipper, is simply a *subjective* process, depending entirely on the fancy of the beholder? Of what use then are any speculations on "Bird Life" by such an one? To most men the observation of the aspects of nature, as exhibited under divers conditions of country and climate, afford a most instructive education. To Dr. A. E. Brehm it seems to be otherwise. He has wandered in many lands, and has seen in their homes the faunas of both north and south. The only effect this seems to have had upon him is to teach him that he lives. "Movement is life" we read (p. 19), "and life is the power of self-motion." Motion is therefore the chief characteristic of birds. "The bird is, of all creatures, the most versatile in its movements; it runs, climbs, swims, dives, and flies" (p. 20). He is careful to add that all these qualities are not to be found in a single species; but may not just as much be said for the insect or the mammal; or even if the dreams of some geologists be well-founded, might they not all have been found "combined in one creature"? A contemporary of the pterodactyls might, with some appearance of truth, have applied to one of them the description of Milton's fiend, who

O'er bog, or steep, through strait, rough, dense, or rare,
With hand, head, wings, or feet, pursues his way,
And swims, or sinks, or wades, or creeps, or flies.

So far as powers of locomotion go, and by "movement" Dr. Brehm plainly means locomotion, the bird is hardly superior to the insect or the mammal. But to return to the extraordinary hypothesis that "movement is life," and the converse. The most miserable savage that ever plucked a mussel from the rock knows better in this respect than Dr. Brehm; and when the latter tells us, *à propos* of the songs of birds (p. 37), that the "voice is still motion," and we connect the statement with a previous assertion (p. 19), that "worlds roll on through boundless space—and live," we feel certain that we ought to hear the music of the spheres, or some other mystical sweet sounds, if we could only elevate ourselves to his exalted ecstasy.

But we think we need not trespass further on the time of our readers. We will conclude by expressing the hope that when Mr. Jesse and Mr. Labouchere next set about translating a German author they will have better luck in pitching upon a subject—and they will easily find one—for their labours than the rhapsodies of Dr. Alfred Edmund Brehm.

OUR BOOK SHELF

Proceedings of the London Mathematical Society. Vol. iii., Nos. 21—40.

THE papers read before this Society still preserve the high character attributed to them in the notice of vol. ii., which appeared in this journal. That such should be the case is not matter for surprise, when we run our eyes over the list of contributors. The principal authors are Prof. Cayley and Mr. Samuel Roberts. The former furnishes three memoirs on quartic surfaces (pp. 59—69; 198—202; 234—266); sketch of recent researches upon quartic and quintic surfaces; rational transformation between two spaces (pp. 127—180); on Plücker's models of certain quartic surfaces. The latter communicates papers on the order of the discriminants of a ternary form; pedals of conic sections (pp. 88—98); on the ovals of Des Cartes (pp. 106—126); on the order and singularities of the parallel of an algebraical curve (pp. 209—259); on the motion of a plane under certain conditions. Prof. Clerk Maxwell contributes a paper on the mathematical classification of physical quantities. Besides the foregoing communications, the above-named gentlemen have laid other papers before the Society. Memoirs have also been presented by Mr. J. Griffiths, Mr. J. J. Walker, Prof. Clifford, Hon. J. W. Strutt, and other members. Some other highly valuable communications, we learn from the "Proceedings," were made to the Society, but no record has as yet been made of them, their authors not having yet sent their completed papers for publication. The Society, from the number and high character of its memoirs, seems to have met a want, and is, perhaps, the only Society before which many of the communications could have been brought. As generally the papers are worked out in some detail at the meetings, members have an interesting opportunity of seeing how some of our foremost mathematicians employ their divers instruments. The Society has lost by death during the past session, its first president, and one of its earliest warm supporters. A slight sketch of Prof. De Morgan and his works appeared in NATURE close upon his death in March last. The eighth session of the Society's existence has just commenced, and we trust its future work may be as good as that it has already achieved. *Floreat.*

Treatise on Terrestrial Magnetism. (Blackwood and Sons.)

THE first half of this book contains a good deal of information, and some inquiries connected with the question of the secular variations in the magnetic elements. The author, on the supposition that the secular changes in the declination are caused by the action of a single, slowly rotating pole on a needle which at each place is locally influenced in a definite and determinable manner, computes the declination at several places, and shows that it agrees tolerably well with actual observation. The rotating pole he places at a constant distance of 23° 30′ from the pole of the earth's axis, and gives to its rotation a period of 640 years. The latter part of the book, however, is taken up with "an hypothesis." The writer of this book, and many other such writers, would do well to remember the words of Newton, "*Hypotheses non fingo.*" The hypothesis referred to is simply this:—that the sun attracts the electric matter in the earth and carries it round in a sort of tidal wave, this causes an electric current from east to west, which causes the magnet to point to the north, and from which the writer also attempts to deduce some of the other phenomena of magnetism. There seems to us to be some ambiguity in the writer's method of expression, so that we do not clearly gather whether he intends this current to account for the whole magnetic action of the world, or only for the variations of it. A consideration of the character of the variations of the needle is sufficient to overthrow the hypothesis announced by our author. The solar diurnal variation is thus explained by him:—The pole of the ecliptic revolves once a day round the pole of the earth's axis, the needle tends to follow this, and hence the solar diurnal variation. Now, we may point out a circumstance which, apparently, entirely overthrows, not only this hypothesis, but any which attempts to account for that variation by anything of the nature of the movement of a magnetic pole. At Point Barrow the needle points N.E., at Port Kennedy it points S.W., yet at each place the solar diurnal variation follows local time and exhibits precisely the same features. Standing, then, at the centre of the needle, and looking towards its marked end, that end would at both places be observed to be moving towards the left hand of the observer between the hours of 8 A.M. and 1 P.M. But since the needles are pointing in opposite directions, this constitutes a movement of the marked end of the one towards the geographical west, and of the marked end of the other towards the geographical east, and this at times when the needles are under precisely the same circumstances with respect to the sun's influence. Now, no movement of the magnetic pole can account for this, it would necessarily entail a movement of the marked end of both these needles in the same geographical direction. The consideration of this phenomenon shows us that if the solar diurnal variation of the declination is to be attributed to a current, it must be one not round the magnetic pole or the geographical pole, but along the magnetic meridian. But this is not the place for us to discuss this question further at present. It would seem to be, however, rather from the consideration of such phenomena as this in a careful and accurate way, and the attempt therefrom, by induction, to arrive at laws, that we may hope to form a theory of terrestrial magnetism, than from "making an hypothesis," and then attempting to apply it to facts.

J. S.

LETTERS TO THE EDITOR

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

Mayer and De Saussure

IN Prof. Tyndall's account of the labours of Mayer, a paragraph is devoted to the bearing of his principles upon the phenomena of vegetable life. It suggests two points of difficulty to me:—

1. It is said that "Mayer's utterances are far from being anticipated by vague statements regarding the 'stimulus' of light, or regarding coal as 'bottled sunlight.'" Nevertheless the paragraph reads almost like a paraphrase of the following passage from Sir John Herschel's "Outlines of Astronomy" (1833), p. 211:—

"The sun's rays are the ultimate source of almost every motion which takes place upon the surface of the earth. By its heat are produced all winds and those disturbances in the electric equilibrium of the atmosphere, which give rise to the phenomena of terrestrial magnetism. By their vivifying action vegetables are elaborated from inorganic matter, and become in their turn the support of animals and men, and the sources of those great deposits of dynamical efficiency which are laid up for human use in our coal strata. By them the waters of the sea are made to circulate in vapour through the air and irrigate the land, producing springs and rivers. By them are produced all disturbances of the chemical equilibrium of Nature, which by a series of compositions and decompositions give rise to new products and originate a transfer of materials."

In a note in Mr. Herbert Spencer's "First Principles" (2nd Ed., p. 496), which first led me to look at this passage, it is remarked that Herschel "expressly includes all geologic, meteorologic, and vital actions, as also those which we produce by the combustion of coal," in the effects of the solar rays. When, therefore, Prof. Tyndall states that Mayer *revealed* the source of the energies of the vegetable world, it appears to me that Herschel anticipated the revelation twelve years previously. Of course I apprehend that Mayer's merit consisted in seizing at once a physical principle of immense generality, and in applying it to very different phenomena. Herschel began at the other end; but appears equally to have seen the solar energy underlying these phenomena, though in a general way, and without demonstrating numerical relations.

2. De Saussure is credited unreservedly with the observation of the reducing power of the solar rays in the vegetable economy. But he seems to me, as, indeed, he seemed to himself, to have only crowned a theory which other workers had elaborated. Priestley began by ascertaining that air depurated by animals was purified by plants. Ingenhousz showed, what Priestley candidly confesses he missed, that this effect is due "chiefly, if not only, to the light" of the sun. Senebier found that "fixed air" was the ingredient which plants removed from a vitiated atmosphere, and that this underwent elaboration in the tissues, oxygen being set free as the result.* Lavoisier having previously shown that fixed air was a compound of carbon and oxygen, Senebier's results implied the fixation of carbon by plants. This fixation De Saussure actually demonstrated by feeding a plant with carbon dioxide and water alone, and showing that the carbon in the tissues increased. He further found the unexpected fact (and this is what he added to the matter) that the oxygen evolved by plants does not correspond to that contained in the carbon dioxide absorbed, but that it is smaller in quantity.

De Saussure's researches are a beautiful example of quantitative work, but they would have, I imagine, merit of a different order if Priestley, Ingenhousz, Senebier, and Lavoisier had not broken ground before them.

W. T. THISELTON DYER

* Recherches sur l'influence de la lumière solaire pour metamorphoser l'air fixe en air pur par la végétation, 1783.

Phenomena of Contact

IN NATURE of August 24 I objected to, as misleading, the statement by Mr. Newcomb that "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." I asked for references to the facts and experiments by which the statements are justified.

In NATURE of September 28 I find Mr. Newcomb's reply, but without the references which I desired. Mr. Newcomb considers that I controvert the two following propositions :—

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.

If the word "irregular" is cut out, and the word "seen" substituted for "present" in proposition (1) there can be no doubt about its truth. It will be found that all the arguments adduced by Mr. Newcomb to prove this proposition have no bearing either upon the word "irregular" or "present" in contradistinction to "seen."

It appears to me, therefore, quite unnecessary to allude further to this proposition.

With reference to proposition (2), I believe it to be utterly erroneous. I believe that the phenomena of the fine connecting ligament can only be seen in a fair atmosphere, with a good telescope well adjusted to focus, and with considerable magnifying power. When it is remembered that the fine connecting ligament is confined to within about a second of arc of the sun's limb, I think my statement will at least commend itself to practical observers. Mr. Newcomb appears to regard it as a great difficulty in my view of these phenomena, that some of the observers should see the ligament and some not. I am rather surprised at the persistence with which this point is again and again brought forward in his letter. I thought that it had been answered by anticipation in my letter which appeared in your number of August 24. In all my writings upon the subject I have maintained that the phenomena could only be seen under favourable circumstances and with sufficient power; and in my letter of August 24 will be found this statement, which appears to have been entirely overlooked, at least unanswered, by Mr. Newcomb :—"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." In my view those observers who did not see the connecting ligaments failed to see it, either from want of attention to the point as not a contact such as they expected to see, or from the observations having been made under such circumstances that some of the necessary conditions which I have indicated were not satisfied.

The fine connecting ligament is only seen by contrast against the illumination of the sun's disc near the point of contact, and it may well be that some of the observers have pushed the diminution of brightness of the sun's image to such an extent that the contrast was too feeble to attract attention before the apparent contact. To me, and I think to others who will give the matter some consideration, it is clear enough "that an observer with the naked eye, a telescope of low power, would not, in the case of a transit of Venus, see the connecting ligament at all." It is as clear "that without seeing any ligament, the planet, at egress, would appear to touch the limb without distortion, necessarily, earlier than the contact would appear to be established to observers who were watching the transit with good telescopes and with high powers." It appears to me equally clear that, if the brightness of the sun's image be reduced to excess, then the ever-diminishing small portion of the illuminated disc between the sun's edge and the advancing planet, at egress, may be made to disappear, from sheer inability to appreciate so faint a light, before the contact would appear to be established to observers who had not so reduced the brightness of the image. Disturbing causes such as these do exist, and their effects must be recognised. I must apologise for bringing forward such arguments; but, since one observer has published his opinion that the "ligaments, &c.," do not exist in contacts, because he looked at the transit of Mercury with an opera glass and saw nothing of the kind, it would appear necessary to recall attention to a common-sense view of the points at issue.

But whatever may be the opinion of Mr. Newcomb respecting the explanation which I have given of the probable reason why some of the observers have not seen the connecting ligament, he must feel that it will at least be difficult for him to explain away the positive evidence of the numerous observers who profess to have seen the ligament with first-rate telescopes. Some of them, at least, were gentlemen not likely to have forgotten to adjust their eye-pieces to focus, even if such a neglect would have produced the phenomena observed. In cases where the ligament has been seen, it will be found that the earlier lines of contact at egress have been given by observers with the best telescopes and high powers. This is strikingly shown by the Greenwich observations of the transit of Mercury, 1868. It is a result in perfect accord with my views. Very large and systematic discordances will be found to exist between the times of internal contact at the transit of Mercury, 1868, in cases where no connecting ligament was seen at all. This has been passed over in silence by Mr. Newcomb; but it is important. It would be difficult to select from such groups of observers—the French, for example, who saw no connecting ligament—those who saw "the phenomena exactly as we know they are;" and unless this can be done, I am afraid that Mr. Newcomb's somewhat unique argument upon this point might be made as easily to prove the converse as the result he deduces from it. All these observations, in my view, are good; but they are not strictly observations of the same phenomena.

Mr. Newcomb rejects at once the force of the evidence of the observers of the transits of Venus, 1761 and 1769, upon the question of the connecting ligament "till we have better evidence than now exists that their object-glasses were such as Clarke or Foucault would call good." The phenomena connected with the ligament must be far more marked in the case of Venus than in that of Mercury, on account of the large diameter of Venus. To reject therefore by an impossible condition all the evidence in our possession respecting transits of Venus is certainly a bold step; but Mr. Newcomb appears to me to attach far too much importance, so far as irradiation phenomena are concerned, to the improvements effected in modern telescopes. The image of a point of light on the most perfect object-glass which can be conceived is not a point, but a disc, of which the illumination degrades rapidly from the centre, and which is surrounded by concentric rings of light. The law of degradation of the illumination of the central disc has been given by me in the Monthly Notices, November 1865. The result of theory upon these points has been most completely tested by experiment. The existence and regularity of these concentric circular rings is one of the most delicate tests of the perfection of a telescope. Since we have a disc of light corresponding to a point in the most perfect object-glass which can be made, the visible image of the sun formed by such a glass will not terminate with the geometrical image. This result of theory is confirmed by experiment. The optical enlargement found under degrees of illumination similar to those very commonly adopted in observations of the sun is amply sufficient to produce by its destruction near the point of contact the phenomena which so many observers of experience have declared that they have seen. That the optical enlargement is sufficient for the purpose can be seen from the experiments of Dr. Robinson, of Armagh, and from the Greenwich discussions of eclipse observations. This was pointed out in my letter in your number of August 24. With respect to Mr. Newcomb's remark as to the application of this theory of irradiation to a transit of a planet, viz., "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 transit." I have merely to remark that Mr. Newcomb is undoubtedly right when he asserts that the irradiation from the minute thread of light darkened so as to be barely visible is not the same as that of the large disc. *It is simply because such is the case that the phenomena of the connecting ligament appear.* When the planet is well on the disc, the irradiation around the disc will not be disturbed, but as the planet approaches the edge, the irradiation near the point of contact must eventually be disturbed, and this disturbance, or change, gives rise to the phenomena observed—a black drop, connecting ligament, or whatever name you prefer to give to that apparent cutting out of a piece of the sun's edge near the point of contact which must take place. After the disturbance of the irradiation has once commenced, the connecting ligament must at egress increase in breadth; but I do not profess to be able to give the law of the changing form with any degree of exactness.

The experiments of Wolf and André were, as I stated in my letter of August 24, made upon a disc presenting no sensible traces of optical enlargement. The results can therefore have no bearing upon the question of irradiation. These results are undoubtedly valuable in themselves, as showing experimentally the tendency of errors of observations of contacts under feeble illumination. They may throw light upon those observations at which no connecting ligament was seen, but they are useless to disprove or prove irradiation effects.

My authority for stating that the observations of Wolf and André were made upon a disc showing no sensible traces of optical enlargement, is contained in the memoir itself. If Mr. Newcomb is pleased to call the phenomena of "telescopic irradiation" a species of bad definition, there can be no objection on my part to his doing so; but it is not a species of bad definition "which disappears in a fair atmosphere, with a good telescope well adjusted to focus."

With respect to the ligament not being a celestial reality. The contact is not a celestial reality. My views of the reality of the phenomena are that the reality is neither more nor less than the reality of the phenomena presented at the focus of an object-glass when turned upon a star. The irradiation can in my view be got rid of to the same extent and in the same manner that the central disc corresponding to the star's image can be got rid of. You can reduce its dimensions by cutting down the illumination, and the disc will become a point, but only as it vanishes.

If I may be allowed to give one word of warning respecting the preparations for the transit of Venus 1874, it is *uniformity*. Make such arrangements as you think best, but once made stick to them even if not absolutely the best. The observations which are to be compared must be made as early as npossible under the same optical conditions. The whole success or failure of the work will, in my opinion, turn upon the extent to which this necessary condition is approximated to.

E. J. STONE

Royal Observatory, Cape of Good Hope, Nov. 18, 1871

The Origin of Insects

WITH your kind permission I will answer Dr. Beale's questions, published in his letter in your issue of December 21, 1871.

Dr. Beale asks me what part of the nervous system of the maggot is present in the fly? My answer is that I have traced the changes of the one directly into the other; and that Weismann has done the same. There is no time in the pupa state when the nervous system is absent; but it is difficult to demonstrate this, as amongst so much molecular matter it is not easily found, and is very easily crushed and destroyed.

Again, Dr. Beale asks me if I have seen any vestige of the mouth organs of the imago in the larva? I reply that the mandibles and maxillæ exist in the egg twelve hours before the young maggot emerges, together with the fore and hind-head segments; that these have all disappeared when the egg hatches; but that the imaginal discs are already formed at that time. Now, I would ask if it bears the slightest aspect of probability that the larval head segments and mandibles, maxillæ, &c., are formed for nothing, and that the imaginal discs are new formations arising contemporaneously with the disappearance of the larval head segments? Dr. Weismann has shown unmistakably that the abdominal segments of the pupa skin are formed from the abdominal segments of the larval skin. Does it appear in the slightest degree probable that the thoracic and head segments have a totally dissimilar origin? I admit that I have not been able to see the imaginal discs in contact with the head segments of the embryo; but I have found the imaginal discs immediately after the egg is hatched, and they are then too much like the embryonic structures alluded to, to have had any other probable origin. The proboscis is formed from cells laid down within these discs; of that there is not the smallest doubt. Dr. Weismann makes the same assertion, and, although I did not know it to be so at the time I wrote my work on the fly, I acknowledge it is so now, and that in my description of the origin of the proboscis I was wrong. In the Lepidoptera, and in some beetles, imaginal discs may be seen to have their origin in the inner layer of the larval skin.

Again, Dr. Beale says:—"Does Mr. Lowne mean to say, for instance, that he or any one else can adduce any reliable observations to prove that the sexual organs are gradually developed, even from the time when the embryo is enclosed within the egg?" I answer, yes. My own observations confirm those of Weismann on this head, and Dr. Beale will find, on looking again at page 112 of my book on the blow-fly, that he has not correctly quoted my statement. I will also refer Dr. Beale to Dr. E. Bessel's paper, "Studien über die Entwicklung der sexual-Drüsen bei den Lepidopteren," in the "Zeitschrift für wissenschaftliche Zoologie," vol. xvii. I believe future observers will find the sexual organs are always so formed, even as they are in the vertebrata. There is another paper, by Siebold I think, on the same subject in the above-quoted periodical.

Lastly, Dr. Beale asks me to explain what I mean by the sentence occurring at page 116 of my book:—"All the tissues of the larva undergo degeneration, and the imaginal tissues are redeveloped," &c. I apprehend that the redevelopment of all the tissues does not imply also the redevelopment of the insect. That the tissues are all so redeveloped is undoubted, but they are not all redeveloped at once. I have stated in my book again and again that certain organs are redeveloped in a particular manner, and was never under the impression that the whole was a case of alternate generation. I did not know the origin of the imaginal discs in those days.

With your permission I will add a few words in support of the assertion "that the pupa change is analogous to ordinary ecdysis, of which it is a modification." In ordinary ecdysis the muscles undergo degeneration at their points of attachment to the cast skin; in metamorphosis this change is far more marked. In ecdysis in Chloeon, for instance, Sir J. Lubbock (Linn. Soc. Trans., vol. xxiv.) has shown that the wings and thorax are gradually developed through nine successive sheddings of the skin. In the more remarkable metamorphosis of Lepidoptera they are developed in two ecdyses, these two being called metamorphosis. Prof. Owen believed, and the assertion is now widely known, that the larvæ of such insects as the Orthoptera, Neuroptera, &c., exist in the maggot form in the egg; but the observations of Mr. Newport on Meloe, and of Fritz Muller, of Weismann, and many others, go far to prove that this is not so—that the maggot form is intermediate, the half-developed embryo and the pupa or perfect insect, being most alike.

The subject is one of great interest, and therefore I trust you will excuse this long trespass on your pages.

90, Guilford Street BENJAMIN T. LOWNE

In Re Fungi

YOUR sarcastic correspondent "F. L. S." is quite incompetent to reply to my former letter. I did not call in question the correctness of the determination of *Agaricus cartilagineus*, but merely drew attention to the absurdity of the statement that the said determination was made from a mere "mass of mycelium," and that such a statement should come from a journal specially devoted to Botany.

In the original report of the occurrence of *Agaricus cartilagineus* (*Journal of Botany*, vol. iii. p. 28) special reference is there made to the "many-headed pileus;" now some of these "pilei" (not the "mycelium," "F. L. S.,") were forwarded to the Rev. M. J. Berkeley for examination, and from *these materials* he (and not the writer of these lines) made out the plant to be *A. cartilagineus*. Certainly I included the species "without hesitation" in the list of Middlesex Fungi, because I knew the plant referred to had not been determined from a mere "mass of mycelium," but that Mr. Berkeley had examined the perfected parts.

I fail to see why "F. L. S." is so anxious to "allay my alarm as to the decay of Fungology in England," especially as I have never expressed any "alarm" on that head. I do not look upon the *Journal* as such an infallible weathercock as to connect its wrong statement with a national breakdown in Botany; neither do I see how I have "missed the point" of its paragraph. I am more inclined to think that I have *hit it* in a friendly way, and rather hard too. W. G. S.

Mr. Baily on Kiltorkan Fossils

IN your last number Mr. Baily is said to have brought forward at a meeting of the Geological Society of Dublin "some strong facts to prove that the Irish palæontologists had not misled Prof. Heer, as stated by Mr. Carruthers at a recent meeting of the London Geological Society."

At the meeting referred to, Prof. Heer placed the Irish beds at the base of the Carboniferous series, mainly because *Sagenaria Veltheimiana*, a coal measure plant, was found in them.

Into this error I said "Prof. Heer had been led chiefly by the erroneous determination of the Kiltorkan *Lepidodendron* by the Irish palæontologists." I will not burthen your columns with the strange history of the nomenclature of this plant, as I shall have an opportunity of doing this elsewhere ere long. The point before us is this, that Mr. Baily alone has the credit of erroneously determining the Kiltorkan plant to be the same as an already described Carboniferous species. And the proof of this is easily adduced. In 1864, Mr. Baily, in his "Explanation of Sheets 187, &c., of the Irish Survey," figures the fossil, and describes it unhesitatingly as "*Sagenaria Veltheimiana*, Sternb. sp." This he repeated in a paper by the lamented Prof. Jukes in 1866 (*Journ. Geol. Soc. Ireland*, i. pp. 123, 124), as well as in a paper by himself read to the Natural History Society of Dublin in the same year (p. 2). Prof. Heer acknowledged his obligations to Mr. Baily for the Irish specimens he had examined. I have examined specimens so distributed by Mr. Baily, and they were named *Sagenaria Veltheimiana*.

In the volume of the British Association Reports, published in 1869, Mr. Baily says (p. 59) that the *Sagenaria* is named by Schimper *S. Bailyana*. More recently (Nov. 1871), in his "Figures of British Fossils" (p. 84), he names it *Knorria Bailyana*. It is not much to the purpose to say that it is neither a *Knorria* nor a *Sagenaria*, or further that the specific designation *Bailyana* must give place, with some dozen other synonyms, to the original name given by Dr. Haughton in 1855. But it is to the purpose to notice that *Sagenaria Veltheimiana* is *not* a Kiltorkan fossil, though said to be so by Mr. Baily, and that this error, now acknowledged by Mr. Baily himself, was the main foundation of Prof. Heer's argument.

I am not a little curious to know what are the "strong facts" which will overthrow a plain narrative that fully justifies my statement, but at the same time compels me to make it more personal than the truth seemed to me to demand when I made it some months ago. WILLIAM CARRUTHERS

ZOOLOGICAL RESULTS OF THE ECLIPSE EXPEDITION

A STEAMER is eminently unqualified for observations on marine zoology. Owing to the high rate of speed, it is impossible to use a towing net with any success, and to a zoologist it is perfectly tantalising to see swarms of Medusæ, &c., sail past the ship without being able to obtain a single specimen. In Peninsular and Oriental ships the only practicable method is to keep the tap of the baths constantly running through a fine gauze net. In this way quantities of Entomostraca may be obtained. Since we have been in the Red Sea, the water has been splendidly phosphorescent every night, the light being most brilliant where the hot water from the condensers is shed out into the sea, the animals being probably killed by the heat, and emitting in the act one last brilliant flash. If the water be turned on into one of the baths at night, most gorgeous flashes of light are obtained, and the animals causing them may be caught in small vessels and kept for examination. They are at present almost exclusively Entomostraca of the genera *Cypris*, *Cyclops*, and *Daphnis*. When the light is examined spectroscopically, it gives a spectrum in which only the green and yellow are present, the red and blue being sharply cut off. Several species of the Entomnstraca obtained contain a brilliant red pigment, which gives unfortunately no absorption bands when examined with the micro-spectroscope. At Suez I obtained a number of Echinodermata of the usual dark purple tint, a splendid *Comatula* in abundance, two species of *Echinus*, and one or two star-fishes. The colouring matter of these animals is readily soluble in fresh water or alcohol, as is that of the common British feather-star. Though its colour is extremely intense, it gives no absorption bands, but when a strong solution is used, the spectrum is reduced to a red band, all the rest of the light being absorbed. Apparently parasitic on a large flat *Spatangus*, were obtained a number of *red* Planarians, about one-eighth inch long, which gave the characteristic absorption bands of hæmoglobin with great intensity. The existence of hæmoglobin in Planarians is a fact of considerable interest, and I believe quite new. On taking a boat excursion round the shores, where I obtained abundance of large Gasteropods and the Echinodermata mentioned above, I was remarkably struck by the absence of Actinias. Though I was out nearly the whole day, I did not see a single specimen, nor indeed did I observe any large Medusæ. This absence of these latter may perhaps, however, have been due to the set of the wind or tide.

Of the Suez Canal fauna we were able to observe very little, except that the canal perfectly swarms with fish from one end to the other. A good many were taken with hand-lines in two spots, one close to Port Saïd, the other in the middle of the Great Bitter Lake. They were all of one species, a sort of mullet, but there are no books at hand to determine the species. The mud brought up from the bottom of the Great Bitter Lake by the chain cable was absolutely devoid of any traces of life. The *Mirzapore* has been visited on her voyage by various land birds. One hen chaffinch accompanied us from Cape Finisterre to Port Saïd, not leaving the ship when she was anchored at Malta, and was to be seen every day hopping about the deck and feeding. At present the ship is surrounded by vast flights of flying fish. They fly generally up wind, and sometimes go as far as one hundred yards.

It is hoped that this short note may be found of some interest, and that it will be borne in mind that it is impossible to travel about with a library sufficient to determine species on the spot.

H. N. MOSELEY

MELTING AND REGELATION OF ICE

AN observation made yesterday caused me to present to my class, in a lecture on Heat this morning, the following experiment. A piece of wire gauze was laid on a convenient horizontal ring, and on this a lump of ice. A flat board was placed on the ice, and pressure was applied by means of weights put upon the board. I put 12 lbs. upon a piece of ice as large as an apple. This was done at the commencement of the lecture, and before the conclusion I found a considerable quantity of ice on the lower side of the gauze, apparently squeezed through the meshes. The temperature of the class-room was about 15° C. (59° Fah.). The experiment was continued for eight or ten hours, fresh ice being supplied when necessary to the upper side of the gauze, and, in spite of the continual surface melting and dripping away of water, a very large quantity of ice was formed below the gauze. The ice below the gauze was firmly united to that above. I tried with my hands to break away the upper from the lower, and to break either of them off at the place where the wire gauze separated them; but I was not able to do so. The ice that has passed through the meshes has a kind of texture corresponding to that of the network, and the small air bubbles appeared to be arranged in columns.

The phenomenon is a consequence of the properties, announced from theory by Prof. James Thomson, and then exemplified by an experiment; and the explanation depends on the theories put forward by him—the first (1857) founded on the lowering of the freezing point of water by pressure, and the second (1861) founded on the tendency to melt given by the application to the solid ice of forces whose nature is *to produce change of form* as distinguished from forces applied alike to the liquid and solid. The stress upon the ice, due to its pressure on the network, gives it a tendency to melt at the point in contact with the wire, and the ice, in the form of water intermixed with fragments and new crystals, moves so as to relieve itself of pressure. As soon as any portion of the mass is thus relieved, freezing takes place throughout it, because its temperature is reduced below that of the freezing point of water at ordinary pressures, by melting of contiguous parts. The obvious tendency of the ice under the pressure from above is thus, by a series of meltings and refreezings, to force itself through the meshes.

The next experiment that I tried I was led to by that just described. I supported a block of ice on two parallel boards, placed near to each other, and passed a loop of wire over the ice. The loop hung down between the boards, and weights were attached to it. The first wire tried was a fine one (0·007 inches diameter) and a two-pound weight was hung on the loop. The wire immediately entered the ice, and it passed right through it and dropped down with the weight after having done so, but it left the ice undivided, and on trying it with a knife and chisel in the plane in which the cutting had taken place, I did not find that it was weaker there than elsewhere. The track of the wire was marked by opacity of the ice along the plane of passage. This opacity seemed to be due to the scattering of air from the small bubbles cut across by the wire. I have not, however, been able to try a piece of ice free from bubbles; and, from the nature of the experiment, air may very possibly pass in along the wire from the outside. I next experimented with a wire 0·024 inches diameter, weighting the loop with 8 lbs., and obtained a similar result; and, finally, I took a wire 0·1 inch diameter, and, putting a 56lb. weight on a loop of it, I caused it to pass through the ice, and the block remained undivided. This, though it follows from theory, has a most startling effect; and during the passage of the thick wire through the ice, I was able to see the bubbles of air across which it cut rising up round its sides. I made careful trials to cut the ice with a knife in the lamina through which the wire had passed, but found no weakness there.

A string was next tried, but, as might be expected, it did not pass through the ice. I considered that the string was not a good enough conductor to relieve itself of the cold in front and pass it back to the water behind. The capillary action of the string also doubtless takes part in the production of the result. It simply indented the ice and froze into it.

On this point of the necessity for a good conductor, and for a way of relieving itself of the cold, a curious observation was made. In one case a thick wire appeared to have stopped (this requires confirmation) as if it were frozen into the ice. On examination it turned out that the ice was so placed that the water formed by the pressure of the wire had flowed away at the first, and a hole was left behind the wire. On supplying a few drops of water to the place from a small pointed bit of melting ice, the water froze instantly on coming in contact with the wire, and the wire moved forward as usual. By this I was also led to try putting a thick wire over a piece of ice having a hollow at the top, so that the wire cutting into the shoulders bridged across the hollow between them. Looking at the wire, which was in front of a window, I dropped some ice-cold water on it, and saw it *freeze instantly* into crystals on the parts of the wire near to the shoulders on which it was pressing. This is notable as the first experimental confirmation of Prof. Thomson's theory on the production of cold by the application of stress.

I have not yet had an opportunity of trying these experiments at a temperature lower than freezing. The amount of pressure necessary to make the wire pass through the ice would of course be very much increased as the temperature is lowered, and it would finally be impossible to cut the ice without breaking it up like any other hard solid. Indeed I saw in one case in which I had a very great weight (80lb. or so) on a thick wire, the ice cracking in front of the wire; apparently the wire was forced too fast through the ice.

These experiments seem to me to have considerable importance in relation to the sliding motion of glaciers. The smallness of the cause has been raised as an objection to the theory of Prof. Thomson. But no one can see the experiments I have described, particularly the first, where a large quantity of ice is squeezed through the meshes of fine wire gauze under small pressure and in a short time, without feeling almost surprised at the slowness of the glacier motion.

JAMES THOMSON BOTTOMLEY

Glasgow University, Dec. 20, 1871

ELECTROPHYSIOLOGICA:

BEING AN ATTEMPT TO SHOW HOW ELECTRICITY MAY DO MUCH OF WHAT IS COMMONLY BELIEVED TO BE THE SPECIAL WORK OF A VITAL PRINCIPLE

I.

ON a white marble slab let into the front of a house in the Strada Felice at Bologna is an inscription showing that, in this house, then his temporary dwelling-place, at the beginning of September 1786, Galvani discovered animal electricity in the dead frog, and hailing this event as the well-spring of wonders for all ages (Luigi Galvani in questa casa di sua temporaria dimora al primi di Septembre dell' anno 1786, scoperse dalle morte rane La Ellettricita Animale—Fonte di maraviglie a tutti secoli). Animal electricity, well spring of wonders for all ages! Yes, said I, as I copied these words a few weeks ago, and as I went into the house repeating them to myself. Yes, still said I, after seeing what was to be seen within the house. Within the house, indeed, there was much to excite the imagination, and to make me more ready to accept these words as the sober utterance of simple truth. Still the same were the common stairs leading from the open outer door to the landing on the first floor, with its two main doors, one on each side, each one opening to a distinct set of apartments, in one of which had lived the discoverer of animal electricity; and the only change of moment was one which served to call back more vividly the memorable past—a portrait in lithograph of Galvani himself hanging upon the wall facing the stair-head. Still the same was a third and smaller door, at which the portrait seemed to be looking, and beyond which were the stairs leading to the belvedere on the roof so common in Italian houses hereabouts. Still the same were these stairs, the lower flights of uneven bricks, the upper of ricketty woodwork, unmended, scarcely swept, since the time when Galvani went up and down them afire with the discovery made in the belvedere to which they led. Still the same was the belvedere itself—the same walls, blank on one side, pierced on the three others with arched openings, two at each end, three at the front, each opening being built up breast-high so as to form the parapet—the same roof overhead with its bare rafters and tiles—and, running across each opening a little below its arched top and parallel with the parapet, the very same iron bar upon which the frogs' limbs had been suspended by copper hooks in the experiment to which the inscription on the slab outside the house refers, and about which Galvani wrote:—"Ranas itaque consueto more paratas uncino ferreo earum spinali medulla perforata atque appensa, septembris initio (1786) die vesperascente supra parapetto horizontaliter collocavimus. Uncinus ferream laminam tangebat; en motus in rana spontanei, varii, haud infrequentes! Si digito uncinulum adversus ferream superficiem premeretur, quiescentes excitabantur, et toties ferme quoties hujusmodi pressio adhiberetur." So little change was there, indeed, that, forgetting the present altogether, I could only think of this experiment in which the existence of animal electricity was divined, and of those myriad other experiments to which it had led, and by which in the end the truth had been made manifest. So absorbed was I in these thoughts that I even forgot to look through the open arches of the belvedere at the blue Italian sky and the other beauties of the prospect. And when at length I came down, I was more than ever in the mind to assent unhesitatingly to the words, "la ellettricita animale, fonte di maraviglie a tutti secoli"—more than ever convinced that animal electricity would prove to be the key by which to unlock not a few of the secrets which are supposed to be exclusively in the keeping of life—more than ever resolved still to go on seeking for truth in the path along which I was urged to go by this conviction.

Nor was I long at a loss how to begin to carry out this resolution. I wanted to reiterate briefly and more clearly some of the things which I had said before respecting animal electricity, and the way in which this force may do a work ascribed to life in muscular action and nervous action; and at the same time to make use of certain new facts which were not a little calculated to confirm former conclusions. I wanted to show that the same workings of animal electricity may be detected in the condition called tone, and even in growth, and that these processes, no less than muscular action and nervous action, may have to be looked upon as electrical rather than as vital manifestations. A natural way of carrying out the resolution I had formed was, indeed, to do the work ready for me; and therefore the task I have now set myself is to do this work, beginning with an attempt to set forth a new theory of animal electricity, and then proceeding to say something in turn on the way in which this theory sheds light upon muscular action, nervous action, the maintenance of the state called tone, and the process of growth in cells and certain fibres—something calculated to show that in each of these cases animal electricity may have to do much of what is commonly believed to be the work of a vital principle.

1. *On a theory of animal electricity which seems to arise naturally out of the facts.*

A current, to which the name of muscle-current is given, may easily be detected in living muscle. It may be detected by applying the electrodes of the galvanometer, the one to the surface made up of the sides of the fibres, the other to that made up of either one of the two ends of the fibres, and also, though much less clearly, by examining either of these two surfaces singly, provided only the two points to which the electrodes are applied are at unequal distances from the central point of the surface. It may not be detected, if, instead of applying them in this manner, the electrodes are applied so as to connect either the two surfaces made up of the ends of the fibres, or two points equidistant from the central point of the surface made up of the sides, or of that formed by either one of the ends of these fibres. A current may or may not be detected under such circumstances, and when it is detected its direction is such as to show that the surface made up of the sides of the fibre is positive in relation to that made up of either one of the two ends, and that the former surface is more positive and the latter more negative as the distance increases from the line of junction between these two surfaces. In this way the galvanometer makes known the existence of points of similar and dissimilar electric tension in living muscle; and the only inference from the facts would seem to be that there is a current when the electrodes are applied so as to bring together points of dissimilar tension, but not otherwise. The facts are not to be questioned. The inferences arising from them can scarcely be mistaken.

This current is to be detected in living muscle, but not in muscle which has passed into the state of rigor mortis. As muscle loses its "irritability," indeed, it ceases to act upon the galvanometer, and no trace of the current is to be met with after the establishment of rigor mortis. As a rule, too, nothing is to be noticed except a gradual failure of current; but now and then (though not in the frog) there may be a reversal in direction in the last moments preceding the final disappearance.

When muscle passes from the state of rest into that of action, there is also a change in the muscle current to which the name of "negative variation" is given by its discoverer Du Bois-Reymond. Thus, when a galvanometer is connected with the gastrocnemius of a frog so as to respond to its muscle-current during the two states of rest and action in the muscle, the needle, which may have stood at 90°, or thereabouts, during the state of rest, is seen to fall back, and take up a position at 5° or nearer still to zero, during action. This change it is which is spoken of as "negative variation." It is a change indicating, not reversal of the current, but simple weakening; for the idea of reversal, which is readily suggested to the mind by the way in which the needle swings back past zero when the state of action is first set up, is at once corrected by the position which the needle takes up a moment or two later, and also by the fact that when the muscle-current of the *contracted* muscle is admitted into the coil of the galvanometer while the needle is resting at zero—when, that is, the experiment is not complicated by the muscle-current of the *relaxed* muscle being in the coil when the state of contraction is set up in the muscle—the needle is found to move in the same direction as that in which it moved under the current of the *relaxed* muscle, but not to the same distance from zero by a very great deal. So that, in fact, this "negative variation" of the muscle-current is nothing more than a sudden disappearance or failure of this current, and no good is gained by retaining a name which only serves to confuse and perplex.

Substituting the new quadrant electrometer of Sir William Thomson for the galvanometer, tensional changes are detected which are in every way parallel with the current changes which have been mentioned.

With this instrument, it is found that the surface made up of the sides of the fibres in living muscle, and that made up of either one of the two ends of these fibres, are in opposite electrical conditions, the ray of light marking the movement of the aluminium needle passing in the direction indicating positive electricity under the charge supplied by the former surface, and in the direction indicating negative electricity under the charge supplied by the latter surface—passing, that is to say, not in one direction only, as it would do if the needle were acted upon by charges differing, not in kind, but in degree only, but to the right in the one case and to the left in the other. It is found, indeed, not only that the surface made up of the sides of the fibres of living muscle is positive, and that made up of either end of these fibres negative ; but also that the former surface is more positive and the latter more negative as the distance increases from the line of junction between these surfaces. With this instrument, too, it is found that these indications of free electricity fail *pari passu* with this failure of the "irritability" of the muscle, that they have disappeared altogether before the advent of rigor mortis, and also that there is a change which serves to point to discharge, more or less complete, when muscle passes from the state of rest into that of action. Thus—in illustration of this latter fact—if the ray of light on the scale stand at 30° under the charge supplied to the electrometer by either one of the two surfaces of living muscle during the state of rest, it will stand at 5° only, or still nearer to zero, under the charge supplied by the same surface during the state of action. The difference is always marked, and always of the same character ; and, being so, the proof of discharge during action would seem to be as complete as may be, seeing that the instrument only takes cognizance of electrical changes of the nature of charge and discharge.

These, then, are the facts which may be looked upon as fundamental. There are the facts brought to light by Du Bois-Reymond through the instrumentality of the galvanometer—the muscle-current, present in living muscle during the state of rest, suddenly disappearing when the state of rest changes for that of action, gradually disappearing as muscle loses its "irritability," and absent altogether in rigor mortis ; there are the facts which I myself have been able to make out for the first time by means of the wonderfully sensitive new quadrant electrometer of Sir William Thomson—the two opposite charges of electricity, one positive, the other negative, present in living muscle during the state of rest, disappearing suddenly when this state changes for that of action, gradually disappearing before, and altogether absent in, rigor mortis. And this is all that need be said upon this subject at present.

And as in muscular so in nerve tissue, there is the current, in this case called the nerve-current, and there are the two opposite charges, positive and negative, this current and these charges being present during life, disappearing suddenly when the state of rest changes for that of action, disappearing gradually *pari passu* with the "irritability," and absent altogether at the time when rigor mortis has seized upon the muscles ; and in truth every particular in the electrical history of the muscle is repeated with strict exactness in the electrical history of the nerve.

In these two tissues, muscle and nerve, there is no difficulty in arriving at a knowledge of these facts ; in other tissues the case is different. In other tissues, indeed, all that can be said is that faint indications of electricity are to be detected during life only, and that in some of the fibrous structures there are differences between the surface made up of the sides of the fibres and that made up by either one of the two ends, which correspond to those met with in muscle and nerve.

These then being the fundamental points in the history of animal electricity, the question is as to their meaning. To what theory do they point ?

In order to account for this muscle-current and nerve-current, Dr. Du Bois-Reymond supposes that the muscle-fibre and nerve-fibre (the same law applies absolutely to both) are made up of what he calls peripolar molecules—of molecules, that is to say, which are (with the exception of certain moments in which these electric relations may be reversed) negative at the two poles and positive in the equatorial belt between those poles. He supposes that the sides of the fibres are positive because the positive equatorial belts are turned in this direction, and that the two ends are negative because the negative poles of the molecules face towards the ends. He supposes also that the muscle-current and nerve-current are merely the outflowings of infinitely stronger currents ever circulating in closed circuits around the peripolar molecules of the muscle and nerve respectively. And this view no doubt has much to recommend it.

But another view may be taken of this matter—a view according to which this electrical condition of living muscle and nerve during rest is, not current, but static ; and this view is that which recommends itself to my mind as in every way more simple, more comprehensive, and more to the point practically.

In taking this view the great resistance of the animal tissues to electrical conduction serves as the starting point. I assume that parts of these tissues may be bad enough conductors to allow them to act as *dielectrics*. I assume that the parts which are thus capable of acting as dielectrics are the sheaths of the fibres in muscle and nerve, or the cell-membrane of the contractile cells of those fibres in muscle which have no proper sheath. I assume that a charge, usually the negative, may originate in the molecular reactions of the contents of the sheath or cell-membrane, and that this charge, acting upon the inner surface of the sheath or cell-membrane, may *induce* the opposite charge upon the outer surface of the sheath or cell-membrane, and that in this way the sheath or cell-membrane during rest is virtually a charged Leyden-jar. I assume that this charge is discharged when the state of rest changes for that of action. I assume that the surface made up of the sides of the fibres in muscle and nerve is positive because positive electricity has been *induced* upon this surface, and that the surface made up of either cut-end of the fibre is negative, because the negative electricity, developed upon the inner surface of the sheath or cell-membrane, is *conducted* to these ends by the contents of the sheath or cell.

All that I assume, indeed, may be readily illustrated upon a small cylinder of wood, left bare at its two ends, and having its sides covered with a coating which may be charged as a Leyden-jar is charged—a threefold coating, formed of an inner and outer layer of tinfoil, with an intermediate layer of gutta-percha sheeting, the latter layer projecting a little towards the two ends of the cylinder, so as to secure the necessary insulation of the inner and outer metallic surfaces; for by charging the inner layer of foil with negative electricity, this cylinder, which may be regarded as a model of a muscular fibre, is found to be, not only positive at the sides and negative at the two ends, but more positive at the sides and more negative at each end as the distance increases from the line of junction between the sides and ends. With this model thus charged, indeed, it is easy to imitate all the phenomena of the nerve-current and muscle-current, provided the electrodes of the galvanometer be applied in a suitable manner, and the charge kept up. With this model thus charged, it is also easy to imitate all the tensional phenomena of nerve and muscle which are made known by the electrometer. And thus the nerve-current and muscle-current, instead of being out-flowings of infinitely stronger currents ever circulating around peripolar molecules, may be secondary phenomena only, the accidental result of certain points of dissimilar electric tension upon the surface of the fibres of muscle and nerve being brought into relation by means of the galvanometer or the electrometer, as the case may be.

In this view, I have assumed that certain parts of nerve and muscle were sufficiently bad conductors to enable them to act as dielectrics, but I had not, it is easy to see, the firmest ground for this assumption. It was certain that these tissues were bad conductors; it was not certain that they were bad enough conductors for my purpose. Here, then, was occasion for new work—for work which must be done before I could hope to gain a secure footing for my theory; and this, therefore, was the task I set myself a few months ago, and about which I have now to say something.

In this work I have made use of a Wheatstone's Bridge having on each side resistance coils of the value respectively of 10, 100, and 1,000 B. A. units, of a set of resistance coils capable of measuring up to 1,000,000 of the same units, and of a battery consisting of six medium-sized Bunsen's cells. With this apparatus I have measured the resistance of muscle, tendon, yellow elastic ligament, brain, and spinal cord, the portion measured in each case being a parallelogram an inch in length by $\frac{1}{20}$ of an inch in breadth, formed by making a slice with a Valentin's knife, of which the blades were $\frac{1}{20}$ of an inch apart, and then cutting a strip from the slice by moving the knife, with its blades still separated to the same degree, at right angles to its surface. In order to eliminate the resistance due to secondary polarity, I measured each of these bodies at ·25, ·50, and ·75 of the inch, as well as at the full inch, the fact being, as was pointed out by Sir Charles Wheatstone in his first great paper on the means of measuring electrical resistance, that while the resistance of a conductor increases with its length, the resistance due to secondary polarity remains the same everywhere. Thus, *at* ·25 it is impossible to say how much of the resistance met with belongs to the body itself, and how much to secondary polarity; but not so *after* ·25, *at* ·50, or ·75, or 1·0; for the resistance belonging to secondary polarity being the same at ·50, ·75, and 1·, as at ·25, it follows that by deducting the resistance at ·25 from the resistance at ·50, ·75, and 1·0 the difference at each of these points will represent the resistance of the body itself between ·25 and that particular point.

Of these measurements those which I made last of all will serve as well as any others for the text of what I have now to say, and these are as follows:—

	Inch.		B. A. units.
Muscle (ox)	at ·25	=	17,000
	·50	=	27,000
	·75	=	36,000
	1·0	=	46,000
Tendon (ox)	at ·25	=	19,000
	·50	=	43,000
	·75	=	69,000
	1·0	=	99,000
Yellow elastic ligament (ox)	at ·25	=	160,000
	·50	=	300,000
	·75	=	820,000
	1·0	=	1,000,000 and more.
Brain (ox)	at ·25	=	11,500
	·50	=	16,100
	·75	=	23,000
	1·0	=	32,000
Spinal cord (ox)	at ·25	=	8,300
	·50	=	14,200
	·75	=	17,500
	1·0	=	22,500

I had made several measurements before these, corresponding more or less closely with them in results, and I was proceeding to make others, with a view to arrive at some common mean of numbers, when I found that the resistance went on continually altering, every moment becoming higher and higher, until in the end it was beyond the reach of my means of measurement.

Thus, in the strip of spinal cord, the resistance at ·25 inch, which at first was 8,300, was 180,000 in five hours, and more than 1,000,000 twelve hours later.

Thus, the resistance of the strip of brain, which at first was 11,500 at ·25 inch, was 25,000 five hours later, and upwards of 1,000,000 after the still further lapse of a dozen hours.

And so, likewise, with muscle, and tendon, and yellow elastic ligament, there was a corresponding increase of resistance when the measurement was repeated at these different times after the first trial.

Nor was this the only proof of a change of this sort; for on repeating these measurements on the same specimens some days later, after they had become thoroughly dried up, I found that the very shortest length which could be got for measurement—a length so short, that the two electrodes conveying the measuring current were all but touching—gave a higher resistance than that which could be gauged by the means at my disposal.

These, then, being the facts, it was evidently useless to go on searching for any numbers which could express anything like a common mean of resistance. It was evident, indeed, that the soft tissues, one and all, apart from moisture, were to be looked upon as insulators, rather than as conductors. Nay, it was possible that they might be insulators rather than conductors even in the fresh state; for it is quite supposable that in this fresh state the walls of the fibres and cells forming these tissues may be virtually dry, with moisture on each side, not with moisture percolating from side to side, and that the degree of resistance presented by these tissues, when fresh, is not that which would be encountered if the current passed *across* these walls, but that which is encountered by the current in passing along their outer moistened surface. It is quite supposable that the measuring current may not pass *across* the walls of the cells and fibres at all, but may glide over and between them only. All this is supposable; and therefore, the facts being as they are, I am, as I conceive, at liberty to assume that the walls of fibres and cells are sufficiently non-conducting to justify me in adopting the theory which I have ventured to propose—a theory, according to which, the electrical condition of muscle and nerve during rest is, not current, but static—the sheath of the fibre, or membrane, taking its place, being always charged as a Leyden-jar is charged, except during the time of action, when there is a discharge of this charge—a theory which, to say the least, has a less visionary foundation than that which rests on peripolar molecules seeing that it rests upon structural facts which cannot be called in question a theory also which, as will be seen in due time, has this in its favour,—that it will simplify not a little several important problems in physiology.

C. B. RADCLIFFE

ICE-MAKING IN THE TROPICS

THE most marked example of the influence of radiation of heat on temperature is its influence on the production of artificial ice by the natives of India.

The fields in which the ice is made are low, flat, and open; and the ice is produced in large quantities when the temperature of the air is 16° or 20° F. above the freezing point; and the plan followed is an interesting example of accurate observation applied to practical purposes by a people now ignorant of science. The same process has been employed from time immemorial in India with scientific accuracy; and while the theory was explained by Dr. Wells,* the practical application was not so well understood; and this first led me to investigate the subject in India.†

The following method is employed by the natives of Bengal for making ice at the town of Hooghly near Calcutta, in fields freely exposed to the sky, and formed of a black loam soil upon a substratum of sand.

The natives commence their preparations by marking out a rectangular piece of ground 120 feet long by 20 broad, in an easterly and westerly direction, from which the soil is removed to the depth of two feet. This excavation is smoothed, and is allowed to remain exposed to the sun to dry, when rice straw in small sheaves is laid in an oblique direction in the hollow, with loose straw upon the top, to the depth of a foot and a half, leaving its surface half a foot below that of the ground. Numerous beds of this kind are formed, with narrow pathways between them, in which large earthen water-jars are sunk in the ground for the convenience of having water near, to fill the shallow unglazed earthen vessels in which it is to be frozen. These dishes are 9 inches in diameter at the top, diminishing to $4\frac{3}{10}$ inches at the bottom, $1\frac{3}{10}$ deep, and $\frac{1}{10}$ of an inch in thickness; and are so porous as to become moist throughout when water is put into them.

During the day the loose straw in the beds above the sheaves is occasionally turned up, so that the whole may be kept dry, and the water-jars between the beds are filled with soft pure water from the neighbouring pools. Towards evening the shallow earthen dishes are arranged in rows upon the straw, and by means of small earthen pots, tied to the extremities of long bamboo rods, each is filled about a third with water. The quantity, however, varies according to the expectation of ice—which is known by the clearness of the sky, and the steadiness with which the wind blows from the N.N.W. When favourable, about eight ounces of water is put into each dish, and when less is expected, from two to four ounces is the usual quantity; but, in all cases, more water is put into the dishes nearest the western end of the beds, as the sun first falls on that part, and the ice is thus more easily removed, from its solution being quicker.

There are about 4,590 plates in each of the beds last made, and if we allow five ounces for each dish, which presents a surface of about 4 inches square, there will be an aggregate of 239 gallons, and a surface of 1,530 square feet of water in each bed.

In the cold season, when the temperature of the air at the ice-fields is under 50° F., and there are gentle airs from the northern and western direction, ice forms in the course of the night in each of the shallow dishes. Persons are stationed to observe when a small film appears upon the water in the dishes, when the contents of several are mixed together, and thrown over the other dishes. This operation increases the congealing process; as a state of calmness has been discovered by the natives to diminish the quantity of ice produced. When the sky is quite clear, with gentle steady airs from the N.N.W., which proceed from the hills of considerable elevation near Bheerboom, about 100 miles from Hooghly, the freezing commences before or about midnight, and continues to advance until morning, when the thickest ice is formed. I have seen it seven-tenths of an inch in thickness, and in a few very favourable nights the whole of the water is frozen, when it is called by the natives solid ice. When it commences to congeal between two and three o'clock in the morning, thinner ice is expected, called paper-ice; and when about four or five o'clock in the morning the thinnest is obtained, called flower-ice.

Upwards of two hundred and fifty persons, of all ages, are actively employed in securing the ice for some hours every morning that ice is procured, and this forms one of the most animated scenes to be witnessed in Bengal. In a favourable night upwards of 10 cwt. of ice will be obtained from one bed, and from twenty beds upwards of 10 tons.

When the wind attains a southerly or easterly direction, no ice is formed, from its not being sufficiently dry; not even though the temperature of the air be lower than when it is made with the wind more from a northern or western point. The N.N.W. is the most favourable direction of wind for making ice, and this diminishes in power as it approaches the due north, or west. In the latter case more latitude is allowed than from the N.N.W. to the north. So great is the influence of the direction of wind on the ice, that when it changes in the course of a night from the N.N.W. to a less favourable direction, the change not only prevents the formation of more ice, but dissolves what may have been formed. On such occasions a mist is seen hovering over the ice-beds, from the moisture over them, and the quantity condensed by the cold wind. A mist in like manner forms over deep tanks during favourable nights for making ice.

Another important circumstance in the production of ice is the amount of wind. When it approaches a breeze no ice is formed. This is explained by such rapid currents of air removing the cold air, before any accumulation of ice has taken place in the ice-beds. It is for these reasons that the thickest ice is expected when during the day a breeze has blown from the N.W., which thoroughly dries the ground.

The ice-dishes present a large moist external surface to the dry northerly evening air, which cools the water in them, so that, when at 61°, it will in a few minutes fall to 56°, or even lower. But the moisture which exudes through the dish is quickly frozen, when the evaporation from the external surface no longer continues radiative; a more powerful agent then produces the ice in the dishes.

The quantity of dry straw in the ice-beds forms a large mass of a bad conductor of heat, which penetrates but a short way into it during the day; and as soon as the sun descends below the horizon, this large and powerfully-radiating surface is brought into action, and affects the water in the thin porous vessels, themselves powerful radiators. The cold thus produced is further increased by the damp night air descending to the earth's surface, and by the removal of the heating cause, which deposits a portion of its moisture upon the now powerfully radiating, and therefore cold surface of the straw, the water, and the large moist surface of the dishes. When better radiators of heat were substituted, as glazed, white, or metallic dishes, the cold was greater, and the ice was thicker, and the dishes were heavier in the morning than the common dishes. Any accumulation of heat on their surface from the deposit of moisture is prevented by the cold dry north-west airs which slowly pass over the

* Essay on Dew, 1814.

† Experimental Essay: Jour. As Society, Calcutta, vol. ii, p. 80.

dishes. The wind quickly dries the ground, and declines towards night to moderate airs. The influence of these causes is so powerful that I have seen the mercury in the thermometer placed upon the straw between the dishes descend to 27°, when three feet above the ice-pits it was 48°.

So powerful is the cooling effect of radiation on clear nights in tropical climates, that in very favourable mornings, during the cold season, drops of dew may sometimes be found congealed in Bengal upon the thatched roofs of houses, and upon the exposed leaves of plants. In the evening the cooling process advances more rapidly than could be supposed by one who has not experienced it himself, and proves the justness of his feelings, by the aid of the thermometer. In the open plain on which the ice is made, I have seen the temperature of the air, four feet above the ground, fall from 70·5° to 57°, in the time the sun took to descend the two last degrees before his setting.

The tropical rains are succeeded by the cold season, when the night is cold, the sky quite clear, and the air becomes a bad conductor of electricity, from the dry northern winds which then prevail. This is proved by the rapidity with which evaporation proceeds, by the dispersion of clouds, and by the more evident proofs which the hygrometer exhibits. During the cold season vegetation proceeds, and electricity continues to be evolved by living bodies, and during their decomposition.

These remarks will enable us to explain the process by which the ice is prepared in Bengal.

1st. The large quantity of dry straw and moist dishes rapidly become cold, by their powerfully radiating surfaces, at the same time that the large body of dry straw strongly attracts positive electricity, and the descending currents of air deposit moisture in the dishes of water. Hence, during a cold and clear night, with airs from the N.N.W., the cooling process will advance more rapidly in proportion to the non-electric or attractive nature of the body, which, with the radiating power of the surface, regulates the cold and the quantity of dew deposited upon the body.

2nd. The high and dry situation and free exposure of the ice-fields to the sky, and the absence of all causes which could interrupt the influence of the large body of non-electrics, and the extensive surface of powerful radiating substances, sufficiently accounts for the degree of cold produced in the ice plates; and

3rd. The cool, dry north-west airs slowly pass over the ice-beds, absorbing the accumulation of moisture and of heat, which is given off by the liquefying of a large quantity of water that would otherwise accumulate over the beds; and, thus retaining the air clear and dry, allows the full operation of the other causes, particularly radiation.

T. A. WISE

NOTES

THE Academy of Sciences in Paris publishes the following telegrams received from M. Janssen. One dated Ootacamund, 18th December, 1h 6m P.M., says: "Great hydrogenous atmosphere very rare beyond chromosphere." The other, received on the 19th December by the Minister of Public Instruction, but not dated, simply says: "Eclipse observed; important results."—The Royal Academy of Sciences at Amsterdam has received the following telegram from one of its members, Dr. Oudemans, of Batavia:—"Preliminary results: Corona distinctly seen, pure white rays, dark rifts as far as the moon's limb; no outline of chromosphere; radial polarisation of Corona; no magnetic disturbances; moving shadows positively observed."

AT the meeting of the French Academy, held on Saturday last, to fill up the four vacant chairs, M. Thiers, M. de Remusat, Minister for Foreign Affairs, and M. Dufaure, Minister of Justice were present and voted. The first election was for a successor to Montalembert, and the Duc d'Aumale received 28 votes, one blank vote being recorded. For M. Villemaine's chair there were three candidates, M. Littré, who obtained 17 votes; M. Taillandier, 9; and M. de Viel Castel, 3. There were six candidates for M. Prévost-Paradol's chair. M. Camille Rousset had 17 votes; M. de Viel Castel, 7; M. de Mazade, 3; M. de Lomenie, 1; M. Taillandier, 1; and M. Mary-Lafon, 0. The choice of a successor to Prosper Mérimée was only made after two ballotings. At the first essay M. Edmond About obtained 13 votes; M. de Lomenie, 13; M. de Viel Castel, 2; M. de Mazade, 1; and M. Mary-Lafon, 0. At the second ballot M. de Lomenie received 15 votes, and M. Edmond About 14. Previous to the election a protest in the form of a lengthy pamphlet was distributed among the Academicians by the Bishop of Orleans, who, while professing the utmost respect for the personal character of M. Littré, declared that now, as in 1863, he opposed the admission into the Academy of one who in his writings was the defender of Materialism, Atheism, and Socialism. We learn that in consequence of M. Littré's election, Monseigneur Dupanloup has resigned his seat in the Academy.

WE greatly regret to hear of the death, announced by telegram, from choleraic diarrhœa, of the Venerable John Henry Pratt, M.A., Archdeacon of Calcutta. He was educated at Caius College, Cambridge, where he took his B.A. degree in 1833, when he was third wrangler, the Masters of Christ's and Sidney Sussex Colleges being also wranglers, with Dr. Boustead, afterwards Bishop of Lichfield. In 1838 he was appointed to a chaplaincy in connection with the East Indian Company, and in 1850 was nominated to the Archdeaconry of Calcutta, which he held up to the time of his death. He was well-known for his researches of the interior structure of the earth, and had been a frequent contributor to our columns.

DR. GUSTAV RADDE, Director of the Natural History Museum at Tiflis, has just returned to that town from an interesting journey to the head waters of the Euphrates. Mr. H. E. Dresser has received a letter from him, dated Tiflis, Dec. 14, from which we translate the following extract, viz.:—"Early in August I ascended, in company with Dr. Siewers, a young geologist, the Great Ararat, and we reached an altitude of 14,233 feet above the sea level. Our journey extended over three months, and we have brought back a splendid botanical collection, many good insects, and geological specimens. You will read full particulars ere long in Petermann's 'Mittheilungen.' As regards ornithology, I have not, I am sorry to say, time now to write further respecting the good materials we gathered together, and am just leaving home for another month."

THE Professors to the Newcastle-on-Tyne College of Physical Science have determined to institute evening classes, to commence immediately after the winter vacation, for the purpose of giving instruction in their respective subjects to persons who are unable to attend their day classes. The Professors wish it to be understood that the instruction given in these classes will be such as to require a certain amount of real study on the part of those who attend them.

THE Curator of the Clifton College Museum, Mr. Barrington Ward, has issued a circular asking for donations, to which we are glad to call attention. The following extract will show the very wise limitation placed on the acceptance of specimens:—"It has been decided, with the approval of the Head Master, that the museum shall be essentially a British one, and shall illustrate the natural history and antiquities of our land by good specimens, systematically arranged, under the departments of zoology, botany, geology, mineralogy, and archæology. In addition to this there will be a collection of rare and curious objects, derived from all sources, which may be considered useful for the purposes of scientific teaching, and a large typical series to be used at the lectures and demonstrations given in the College on Comparative Anatomy and other branches of Natural History. The committee of management will only accept of such specimens as can be classed under some one of these heads." In the Botanic Garden attached to the College nearly a thousand species of hardy herbaceous plants are now grown.

WE have received the Preliminary Report by Mr. Sidney I. Smith, on the dredging in Lake Superior; and a reprint from the *American Journal of Science and Art* for December, of Mr. S. I. Smith and Mr. A. S. Verrill's notice of the Invertebrata dredged in the same expedition. The main facts of these reports are already before our readers.

A SOCIETY of Arts, Sciences, and Letters, has just been started at Winona, Minnesota, in connection with the first State Normal School in that place, having for its object the collection of facts and materials looking toward the determination of the natural history, archæology, and general literature of the United States.

DR. HOY, in a paper read before the Wisconsin Academy of Sciences, Arts, and Letters, remarks, in reference to the mammals of Wisconsin, that the elk existed in that State as late as 1863, but is now probably extinct. The moose is still found in considerable numbers. The last buffalo was killed in 1832. Antelopes were also found in Wisconsin in the time of Father Hennepin, although now, of course, driven far to the west. Most of the wild animals are diminishing very rapidly in number, the panther and deer being almost exterminated. The otter and beaver, however, are very persistent. The last wild turkey was killed in 1846 near Racine.

A SCIENTIFIC commission in the interest of the Government of Peru has lately been investigating the guano deposits of the Lobos Islands; and it is reported that the result of their inquiries has been very satisfactory, and that immense quantities of very rich guano, equal, if not superior, to that of the Chincha Islands, have been observed. The analyses of samples are said to have yielded over 13 per cent. of ammonia. Should this be the fact, Payta, as being the nearest port, will probably become a place of considerable importance.

THE Report presented to, and read before, the Board of Visitors appointed by Government for the Royal Observatory, Edinburgh, after summarising the work done at the Observatory during the year, calls attention to the very inefficient manner in which the establishment is provided with funds for its necessary work, and to the scanty salary of its director and assistants. The Board of Visitors estimates the increased annual expenditure necessary to ensure the efficient working of the establishment at 1,050*l*., including 300*l*. increase in the salary of the Astronomer Royal. The report is accompanied by a coloured plan of the Observatory, showing the position of the various instruments, and diagrams of the quarterly means of the earth thermometers from 1837 to 1869; annual mean temperatures, for four several sub-annual epochs, of the rock at the Observatory in the same years; annual means of Schwabe's sun-spots, the earth thermometers, and others at Edinburgh; and eleven-year means for every successive year, from 1842 to 1864, of Schwabe's sun-spots and Edinburgh earth temperatures.

THE seventh Report of the Board of Visitors of the Observatory at Victoria, with the Annual Report of the Government Astronomer, is printed. The report of the buildings and instruments is in every respect satisfactory.

MR. W. H. ARCHER has brought down his records of patents and patentees for the colony of Victoria to the end of 1869; and the Reports of the Mining Surveyors and Registrars for the same colony are printed for the quarter ending June 30, 1871.

THE Report of the New Zealand Institute for its fourth session, 1871, contains the Annual Address, delivered by Sir G. F. Bowen, and a list of donations and deposits in the Museum, and the Laboratory Report for 1870–71. Captain Hutton has prepared a complete catalogue, with a diagnosis, of each species of bird in New Zealand; and arrangements have been made for the publication of similar catalogues of the insects, fishes, and other branches of zoology in the island.

WE have received the last two Annual Reports of the Plymouth Institution and Devon and Cornwall Natural History Society, forming together vol. iv. of its Transactions. Though many of the papers and lectures reported refer to subjects which do not come within our scope, the volumes bear evidence of the zeal and success with which the natural and physical sciences are pursued in the Western counties. Among the papers specially deserving of mention, we may notice, "Degeneration of our Deep-sea Fisheries," by Mr. J. N. Hearder; "The Fulgarator," a new electrical apparatus for producing electric sparks of very great length, by the same; "Rain," by W. Pengelly, F.R.S.; "Mistletoe on the Oak," by T. R. Archer Briggs; "The principles on which ships' sail-carrying power and steadiness in a sea-way depend," by W. Froude, F.R.S.

A PROSPECTUS is issued of a third enlarged and improved edition of Von Cotta's "Geology of the Present." Special reference will be made in this edition to the bearing on geological questions of the recent discoveries of Darwin, Mayer, and Helmholtz.

THE first number lies on our table of "The Mining Magazine and Review; a Monthly Record of Mining, Smelting, Quarrying, and Engineering," edited by Mr. Nelson Boyd. The principal articles in this number are—"The Coal Commission," by the editor; "Boiler Explosions," by E. B. Marten; "The Importance of Nitro-glycerine Explosives for Underground Quarrying Purposes," by S. J. Mackie; and "The Progress of Mineralogy," by F. W. Rudler. It contains also reviews, records of scientific progress, and miscellanea.

A LITTLE pamphlet by Mr. J. G. Fitch, entitled "Methods of Teaching Arithmetic," a lecture addressed to the London Association of Schoolmistresses, and published at the request of the Association, deserves a far wider circulation than among schoolmistresses only. We venture to say that if the admirable plan suggested in the lecture were generally adopted by teachers, of explaining in a rational manner the principles of the simple rules of arithmetic, which are generally learned by rote without the least exercise of intelligence on the part of either teacher or pupil, the teaching of arithmetic would soon cease to be the drudgery which it now is in both boys' and girls' schools, and the results, as exemplified by the reports of the Cambridge examiners and elsewhere, would be very different.

THE "Proceedings of the South Wales Institute of Engineers," Vol. vii., No. 4, contains an important paper by Mr. Thomas Joseph, "On Colliery Explosions in the South Wales Coal Field," which is also reprinted in a separate form. We find in it also many other papers and discussions of value to the engineering and coal interests.

MR. E. PARFITT reprints from the "Transactions of the Devonshire Association for the Advancement of Science, Literature, and Art" two interesting papers—"The Fauna of Devon, Part vii.: Cirripedia," and "On the Boring of Molluscs, Annelids, and Sponges into Rock, Wood, and Shells."

WE have received from Messrs. Nelson and Sons some specimens of Pictorial Natural History, consisting of packets of cards with coloured pictures of birds and some short account of each appended; they are as a series unusually good and elegant, though of unequal merit. Any of them would make a charming present for an intelligent child.

ON November 10 there was an earthquake in Salvador in Central America, and on the 12th a stronger one. At Simla there was an earthquake on November 25. Two sharp shocks were felt at Macedonia on November 26 at 11 P.M.

WHAT is called the Iquique earthquake took place on Oct. 8, at 1 A.M. Although alarming and lasting two minutes, with a terrible shaking of the earth, first vertical and afterwards oscillatory, it did no damage at Iquique. It was, however, simultaneously felt elsewhere, and has destroyed or damaged the towns of Tarapaca, Usmagama, Guasquina, Pica, Matilla, and the village of Pachica. Some persons were injured, but only two lost their lives.

AT a recent meeting of the Scientific Committee of the Horticultural Society, a letter was read from Mr. Anderson-Henry (printed in the *Gardeners' Chronicle* for Dec. 9), in which he gave some curious results of his observations on climbing plants. Mr. Henry stated that certain climbers evince a partiality for some other species, stretching out their tendrils or branches so as to come in contact with them, while to other species they have as strong an aversion, avoiding them and never touching them, though they may run up the same wall side by side. The subject is a curious one, and deserves further investigation.

"THE Fortunate Isles," translated from the French of Ogier, is an account of the Canaries. A chapter on the celebrated dragon tree contains the two passages here transcribed. Written apparently in sober earnest, they are, perhaps, not the least remarkable contribution to the scientific literature of the year now ended. "It is an undoubted fact that before the great Mediterranean deluge, and to a certain point even after it, strange creatures brought forth in transitional periods, inhabited the marshy grounds or those shallow seas which still remained warm. This epoch, called by modern geologists the Reptile Period, produced creatures belonging at once to the animal, vegetable, and mineral kingdoms, or to two only; monstrous products of creative forces; birds, quadrupeds, fish, plants, reptiles, all at once, either united or distinct; the greater number of these have been restored for us by geologists. . . . . The dragon has existed. The first men saw the last survivors of these prodigious creatures, and the memory of them has been preserved. The struggles of mankind with the mighty creatures which overran the earth must have been terrible. The excessive alarm of men possessing no weapons in the first ages, gave rise to the traditions of formidable beings attacking mankind and destroyed by the demi-gods, strong and brave men."

FROM the *Elizabeth Daily Journal* of New Jersey of Nov. 28 we have a marvellous story of a carrier pigeon, which we commend to the notice of Mr. Tegetmeier. It performed the journey from Sopus Farm, Warren Co., N.J., to Sandusky Ohio, a distance of 400 miles, in exactly an hour, and its condition on its arrival at the latter place is thus described:—"I found the greatest excitement had followed the arrival of the pigeon. Mr. Smythe told me that at precisely two o'clock the bird came like an arrow into his house. His movement was more like a blue streak than a well-defined bird. He seemed but little exhausted, although nearly all the feathers were off his body, except the small patch held on his back by the gutta-percha which fastened the note. A few miles more would have worn every feather from his wings, and then he would have to depend upon the momentum already acquired to carry him on his journey, and to steer by a tailless rump, and perhaps be killed in attempting to alight." No wonder the owner offers to match this pigeon "when he has grown a new suit of feathers" for 1,000 dollars against any carrier pigeon that has not done this distance in an equal time.

PERIODICITY OF SUN-SPOTS*

IN the short account of some recent investigations by Prof. Wolf and M. Fritz on Sun-spot phenomena, which has been published lately in the "Proceedings of the Royal Society" (No. 127, 1871), it was pointed out that some of Wolf's conclusions were not quite borne out by the results which we have given in our last paper on Solar Physics in the Philosophical Transactions for 1870, pp. 389-496. A closer inquiry into the cause of this discrepancy has led us to what appears a definite law, connecting numerically the two branches of the periodic sun spot curve, viz., the time during which there is a regular diminution of spot-production, and the time during which there is a constant increase.

It will be well, for the sake of clearness, to allude here again, as briefly as possible, to Prof. Wolf's results before stating those at which we have arrived.

Prof. Wolf has previously devoted the greater part of his laborious researches to a precise determination of the mean *length* of the whole sun-spot period, but latterly he has justly recognised the importance of obtaining some knowledge of the average character of the periodic increase and decrease. Hence he has, as far as he has been able to do so by existing series of observations, and his peculiar and ingenious method of rendering observations made at different times and by different observers comparable with each other, endeavoured to investigate more closely the nature of the periodic sun-spot curve, by tabulating and graphically representing the monthly means taken during two and a half years before and after the minimum, and applying this method to five distinct minimum epochs, which he has fixed by the following years:—

1823·2
1833·8
1844·0
1856·2
1867·2

In a table he gives their mean numbers, expressing the solar activity, arranged in various columns; and arrives at the following results:—

(1) It is shown now with greater precision than was previously possible, that the curve of sun-spots ascends with greater rapidity than it descends. The fact is shown in the subjoined diagram, which it may be of interest to compare with the curves given previously by ourselves in the above-mentioned place. The zero-point in this diagram corresponds to the minimum of each period; the abscissæ give the time before and after it, viz., two and a half years, or thirty months; the ordinates express the amount of spot-production in numbers of an arbitrary scale. The two finely dotted curves are intended to show the actual character of a portion of two periods only, viz., those which had their minima in 1823·2 and 1867·2; the strongly dotted curve, however, gives the mean of all periods (five) over which the investigation extends.

(2) Denoting by x the number of years during which the curve ascends, and presuming that the behaviour is approximately the same throughout the whole period of 11·1 years as during the five years investigated, we have the proportion

$$x : 11{\cdot}1 - x :: 1 : 2,$$

whence $x = 3{\cdot}7$,

or the average duration of an ascent is 3·7 years, that of a descent 7·4 years.

(3) The character of a single period may essentially differ from the mean, but on the whole it appears that a $\left\{\begin{matrix}\text{retarded}\\\text{accelerated}\end{matrix}\right\}$ descent corresponds to a $\left\{\begin{matrix}\text{retarded}\\\text{accelerated}\end{matrix}\right\}$ ascent. Thus the minimum of 1844·0 behaved very normally; but that of 1856·2, and still more that of 1823·2, shown in the following diagram, presents a retarded ascent and descent; on the other hand, the minimum of 1833·8, and still more in that of 1867·2, also shown in the diagram, both ascent and descent are accelerated.

Finally Prof. Wolf arranged in the manner shown in the following table the successive minima and maxima, in order to arrive at some generalisation which might enable him to foretell the general character and length of a future period. Taking the absolute differences in time of every two successive maxima, and

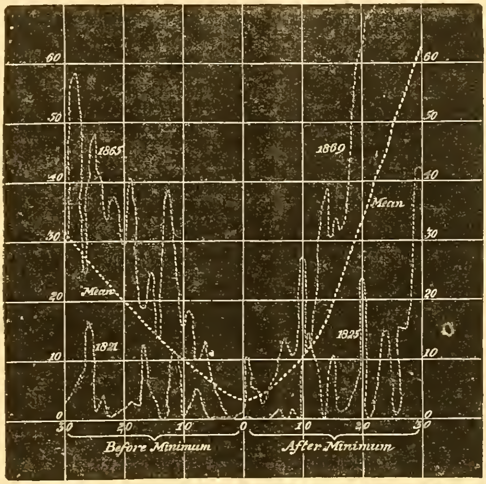
* Abstract of paper read before the Royal Society December 21, 1871. "On some recent Researches in Solar Physics, and a Law regulating the time of duration of the Sun-spot Period." By Warren De La Rue, F.R.S., Balfour Stewart, F.R.S., and Benjamin Loewy, F.R.A.S.

the mean differences of every two alternating minima, he shows that the greatest acceleration of both maximum and minimum happens together. This result strengthens our own conclusions, to be immediately stated, by new evidence, as it is derived from observations antecedent to the time over which our researches extend.

Minima.	Differences of alternating Minima.	Means.	Maxima	Differences of successive Maxima.
1810·5			1816·8	
1823·2	23·3	11·65	1829·5	12·7
1833·8	20·8	10·4	1837·2	7·7
1844·0	22·4	11·2	1846·6	11·4
1856·2	23·2	11·6	1860·2	11·6
1867·2				

From this Prof. Wolf predicts for the present period a very accelerated maximum—a prediction which seems likely to be fulfilled.

Comparing now M. Wolf's results with our own, it must not be overlooked, in judging of the agreement or discrepancy of these two independently obtained sets, that our facts have been derived from the actual measurement and subsequent calculation of the spotted area from day to day since 1833, recorded by Schwabe, Carrington, and the Kew solar photograms, which measurements are expressed as millionths of the sun's visible hemisphere, while the conclusions of M. Wolf are founded on certain "relative numbers," which give the amount of observed spots on an arbitrary scale, chiefly designed to make observations made at different times and by various observers comparable with each other. This will obviously, in addition to the sources of error to which our own method is liable, introduce an amount of uncertainty arising from errors of estimation, and the possibility of using for a whole series an erroneous factor of reduction. Nevertheless we shall find a very close agreement in various important results, and this seems a sufficient proof of the great value and reliability of M. Wolf's "relative numbers," especially for times previous to the commencement of regular sun observations.



The following is a comparison of the data of periodic epochs, as fixed by ourselves and M. Wolf:—

Minima epochs.	I.	II.	III.	IV.
De La Rue, Stewart, and Loewy	1833·92	1843·75	1856·31	1867·12
Rudolf Wolf	1833·8	1844·0	1856·2	1867·2

Maxima epochs.	I.	II.	III.
De La Rue, Stewart, and Loewy	1836·98	1847·87	1859·69
Rudolf Wolf	1837·2	1846·6	1860·2

It will be seen from this comparison that only one appreciable difference occurs, viz., in the maximum of 1847, which M. Wolf fixes nearly one and a quarter years before our date.

The mean length of a period is found by us to be 11·07 years, which agrees very well with M. Wolf's value, viz., 11·1 years.

We found the following times for the duration of increase of spots during the three periods, and for the corresponding decrease, or for ascent and descent of the graphic curve, beginning with the minimum of 1833:—

	Time of ascent.	Time of descent.
I.	3·06 years.	6·77 years.
II.	4·12 ,,	8·44 ,,
III.	3·37 ,,	7·43 ,,
Mean	3·52 ,,	7·55 ,,

Prof. Wolf gives 3·7 years and 7·4 years for the ascent and descent respectively; and considering that he derived these numbers only from an investigation of a portion of each period, the agreement is indeed surprising, and would by itself suggest that the times of ascent and descent are connected by a definite law.

M. Wolf has expressed in general terms the following law with reference to this relation of increase and decrease of spots:—

"The character of a single period may essentially differ from the mean behaviour, but on the whole it appears that a {retarded / accelerated} descent corresponds to a {retarded / accelerated} ascent."

We, on the other hand, have, by an inspection of our curves (*vide* Phil. Trans. 1870, p. 393), been induced to make the following remark on the same question:—

"We see that the second curve, which was no longer in period as a whole than either of the other two, manifests this excess in each of its branches, that is to say, its left or ascending branch is larger as a whole than the same branch of the two other curves, and the same takes place for the second or descending branch. On the other hand, the maximum of this curve is not so high as that of either of the other two—in fact, the curve has the appearance as if it were pressed down from above and pressed out laterally so as to lose in elevation what it gains in time."

Although both statements appear to lead up to the same conclusion—viz., that ascent and descent are connected by law—still they differ essentially in this respect, that if A, B, C represent the three following consecutive events, descent, ascent, descent, Prof. Wolf's law refers to the connection between A and B, while our remark refers to B and C. We consider two successive minima as the beginning and end of a single period, while M. Wolf, at least in this particular research, places the minimum within the period, and compares the descent from the preceding maximum with the ascent to the next one.

We have considered the connection thus indicated of sufficient importance to apply to it the following test. If, using the previous notation, a definite relation exists between A and B, the *ratio* of the times which the events occupy in every epoch ought to be approximately constant; similarly with respect to B and C; and this ratio should not be influenced by the *absolute* duration of the two successive events. It is clear that the greater uniformity of these ratios will be a test of their interdependence. The following is the result of the comparison :—

a. Prof. Wolf's law : comparison of A and B.

	Periods.	Duration of descent (A).	Periods.	Duration of ascent (B).
I.	1829·5 to 1833·8	4·3 years	1833·8 to 1837·2	3·4 years.
II.	1837·2 to 1844·0	6·8 ,,	1844·0 to 1846·6	2·6 ,,
III.	1846·6 to 1856·2	9·6 ,,	1856·2 to 1860·2	4·0 ,,

	Ratio $\frac{A}{B}$.		Difference from mean.
I.	1·265	Mean 2·093	−0·728
II.	2·615		+0·522
III.	2·400		+0·307

These differences from the mean are so considerable that in the present state of the inquiry a connection between any descent and the immediately *succeeding* ascent appears highly improbable. A very new and apparently important relation seems, however, to result from a similar comparison of any ascent and the immediately succeeding descent, or between B and C.

b. Comparison of B and C.

	Periods.	Duration of ascent (B).	Periods.	Duration of descent (C).
I.	1833·92 to 1836·98	3·06 years	1836·98 to 1843·75	6·77 years
II.	1843·75 to 1847·87	4·12 ,,	1847·87 to 1856·31	8·44 ,,
III.	1856·31 to 1859·69	3·38 ,,	1859·69 to 1867·12	7·43 ,,

	Ratio $\frac{C}{B}$.		Difference from mean.
I.	2·212	Mean 2·151	+0·061
II.	2·044		0·107
III.	2·198		+0·047

*PROF. AGASSIZ'S EXPLORING EXPEDITION**

WE have already announced the departure of the United States Coast Survey exploring steamer, *Hassler*, upon that scientific mission which, under the direction of Prof. Agassiz, will doubtless be productive of very important results. Just before starting on the expedition, Prof. Agassiz addressed a communication to the Superintendent of the Coast Survey, in which he ventured to assume the character of a prophet by stating in advance what it was probable would crown their efforts in the way of discovery.

The Professor makes this communication in the hope of showing within what limits natural history has advanced toward that point of maturity when science may anticipate the discovery of facts. Basing his expectations upon the ascertained principles of science, and taking into consideration the relationships between different forms of animal life, and the succession of geological epochs, and in view of the very interesting results of later deep-sea dredging expeditions in the North Atlantic, he anticipates the discovery, "from the greater depth of the ocean, of representatives resembling those types of animals which were prominent in earlier geological periods, or bear a closer resemblance to younger stages of the higher members of the same types, or to the lower forms which take their place nowadays."

Making no suggestion in regard to mammals, he remarks that if reptiles exist in the deep waters, they must be only such as are related to the extinct types of the Jurassic periods, such as the ichthyosauri, plesiosauri, and pterodactyles; but even of these he thinks there is very little probability that any representatives are still alive.

Among the fishes he expects to discover some marine representatives of the order of ganoids of the principal types known from the secondary zoological period. Among the sharks he thinks he shall find new forms allied to *Cestracion*, or *Hybodon*, or *Odontaspis*, as also new genera of chimæroids; and among ordinary fishes the allies of *Beryx*, *Elops*, &c. It is among the molluscs and radiates that objects of the greatest interest will probably be met with: and chief among these will be nautiloid cephalopods—perhaps even ammonites—and forms only known hitherto in the fossil state. Among *Acephala* he anticipates the discovery of a variety of forms resembling those from the Jurassic and Cretaceous deposits; while *Rudistes* will take the place of oysters, and brachiopods be found very abundant.

* Reprinted from advance sheets of *Harper's Weekly*, by permission of the Editor.

Among *Crustacea* it is not at all impossible that forms may be found resembling trilobites; while among echinoderms he confidently expects to meet with spatangoids approaching *Holaster*, and others akin to *Dysaster*, &c.

A careful comparison of the members of the deep-sea faunæ of the northern and southern hemispheres will probably prove of the greatest interest, and, judging from the peculiarities of the land and shore fauna of Australia, it is likely that the adjacent deep-sea animals will be equally divergent, and will represent remarkable forms, and especially of an extremely antique type.

The Professor also hopes that much light will be thrown upon the subject of the geology of the southern hemisphere, and upon the general features of the drift, since all the phenomena related to the glacial period must be found in the southern hemisphere with the same essential characteristics as in the northern, yet with this difference, that everything must be reversed; that is, the trend of the glacial abrasion must be from the south northward; the lee side of the abraded rocks must be on the north side of hills and mountain ranges, and the boulders must have been derived from rocky exposures lying to the south of their present position. This point, however, must be established by observation. The Professor thinks this will be found to be the case, with the exception, perhaps, of the present glaciers of Tierra del Fuego and Patagonia.

In reply to the possible inquiry as to what the question of drift has to do with deep-sea dredging, he remarks that the connection is closer than may at first appear. If drift is not of glacial origin, but the product of marine currents, its formation at once becomes a matter for the Coast Survey to investigate; but he expresses the belief that it will be found that, so far from being accumulated by the sea, the drift of the lowlands of Patagonia has been worn away to its present extent by the continued encroachment of the ocean, in the same manner as the northern shores of South America and of Brazil have been.

SCIENTIFIC SERIALS

Annalen der Chemie und Pharmacie, clix., August 1871. Fittig and Remsen communicate a second paper "On the Constitution of Piperine and its decomposition products, Piperic Acid and Piperidine;" in the former paper two oxidation products were described, piperonal and piperonylic acid, which stand to each other in the relation of aldehyde and acetic acid. In the present communication several new reactions of these substances are described. — The second note, "A Reaction of free Phenol-hydroxyls," shows that the benzene derivatives, containing hydroxyl associated with this nucleus, give colours with a neutral solution of ferric chloride; the intensity of the colour produced seems to bear some proportion to the number of free hydroxyl atoms, the more intense colours being produced by bodies containing more than one hydroxyl.—A paper "On the relations between the Glycerin and Allyl compounds," by Huebner and Mueller follows. They show that the dichlorhydrin prepared by Berthelot's method is a mixture of two isomeric bodies, one of which boils at 174° and can be obtained in a pure state by the action of hydrochloric acid on epichlorhydrin, the other boils at 182° and is identical with dichlorallyl alcohol. Both of these compounds yield allyl alcohol when acted on by sodium in the presence of ether. Kraut and Popp have found that if sodium amalgam containing 3 per cent. sodium is placed in potassic hydrate solution, hard cubes are formed, which, however, possess no definite composition; by the action of sodic hydrate solution long needles are obtained, having the composition $Na_2 Hg_{12}$.—A lengthy paper by Hoffmeister follows "On Phenyl Ether and Diphenyloxide." The former is prepared by the action of nitrous acid on aniline sulphate, the product from which is mixed with phenol when nitrogen is evolved and phenyl ether formed. It can also be produced by the dry distillation of cupric benzoate. Diphenyl oxide is produced by acting on phenol with phosphoric chloride, and again acting on the product with potassic hydrate. A number of substitution products of the two bodies have been prepared, and are here described. —The next paper is "On the Conversion of Acetone into Lactic Acid," by Linneman and Zotta. This is accomplished by heating dichloracetone with water to 200°, when a considerable proportion of lactic acid is obtained. Ladenburg has prepared stannic triethyl phenyl by the action of sodium on bromobenzol, and stannic triethyl iodide, mixed with ether. It is a colourless liquid, boiling at 254°, which is easily oxidised in the air; it reduces an alcoholic solution of silver nitrate, diphenyl being produced in the reaction. Hydrochloric acid forms with it, benzole and stannic triethyl chloride.—An interesting paper by Friedel and Ladenburg, "On Silico-propionic Acid," follows. By the action of absolute alcohol on silicic chloride, the chloride of triethylsilicic acid is obtained; sodium added to this compound, mixed with zinc ethyl, yields, on heating, ethyl orthosilico-propionate, $Si\,C_2H_5(OC_2H_5)_3$. Silico-propionic ether, on treatment with aqueous potassic hydrate, yields silico-propionic acid. It is a white powder resembling silica, from which it is easily distinguished by being combustible. It is soluble in hot potassic hydrate solution, but insoluble in boiling sodic hydrate. This acid is the first representative of a new series of acids, containing the group $Si\,O_2H$ in the place of CO_2H.—Translations of two papers by C. E. Monroe follow, the originals of which have already appeared in the American Journals.—The number concludes with a short note "On the Preparation of Creatinine hydrochloride from urine," by R. Maly. It is purified by combining it with mercuric chloride and decomposing the compound with sulphuretted hydrogen.

SOCIETIES AND ACADEMIES

London

Anthropological Institute, January 1.—Sir John Lubbock, Bart, F.R.S., President, in the chair.—Messrs. J. Thallon and J. Jeremiah, jun., were elected members.—Mr. C. Staniland Wake read a paper entitled "The Adamites." The object of this paper is to show, by reference to evidence extraneous to the Hebrew Scriptures, what peoples are entitled to be classed as Adamites. The name of the primitive race from which the Chaldeans sprung—the *Akkad*—proves that they must be thus classed. Akkad would seem to mean "sons of Ad;" the first syllable of the word being the same as the Gaelic *Mach* or *Ach*. The first Babylonian dynasty of Berosus was Median; and Sir Henry Rawlinson says that the name by which the Medes are first noticed on the Assyrian monuments is *Mad*. This people, the initial letter of whose name may be treated as a prefix, was doubtless the primitive stock from which the *Akk-Ad* were derived. The Medes had also the distinctive title *Már*; and many of the Aryan peoples appear to have retained a remembrance of the traditional *Ad*. The first part of the Parsee work known as *The Desatir* is called "the Book of the Great *Abad*," *i.e.*, Father *Ad*. The Puranas of the Hindus refer to the legendary king, *It* or *Ait*, who is supposed to be the same as the Greek *Aetus*. The primitive Celtic race of Western Europe was called *Gaidal*, *i.e.*, the progeny of Gaid or Aid, who may be identified with *Dis*, the mythical ancestor, according to Cæsar, of the Gauls. Dis (the Greek *Hades*) was also "Lord of the Dead" among the Chaldeans, and may well, therefore, have been the same as the legendary ancestor Ad. Among Hamitic peoples, the original Arab stock trace their first orign to Father Ad, who is probably referred to also in the name of the Egyptian deity, *At-um*. The paper also mentions certain facts showing that the name of the legendary ancestor of the Adamites may be traced in the names of the deities of Turanian and American peoples, and also among the Polynesian Islanders, whose word for "spirit" is *atua*, or *akua*, and whose Great Ancestor is called *Ta-ata*. Dividing all the races of mankind, according to the simple classification of Retzius, into brachycephali and dolichocephali, the conclusion arrived at by the paper is, that Ad was the legendary ancestor of the former, the Adamites, therefore, embracing all the actually brachycephalic peoples, and those whose brachycephalism has been lost by intermixture with the long-headed stock. The Adamites extend through the whole of the northern hemisphere, and are found in various parts of the southern hemispheae, on both the old and the new continents. The names "Adam" and "Eve" were, however, merely expressions of the philosophical notion of the ancients that the male and female principles pervade all nature, and originated all things and personifications of the ancestral idea in relation to the human race.

Chemical Society, Dec. 21, 1871.—Prof. Williamson, F.R.S., vice-president, in the chair.—After the usual business of the society had been transacted, the chairman announced that the celebrated Italian chemist, Prof. Canizzaro, had consented to deliver the Faraday lecture. A paper was then read by Mr. H. Bassett, "On Eulyte and Dyslyte," two beautifully crystalline compounds obtained by the action of nitric acid on citraconic acid, a product of the dry distillation of citric acid. Both these substances contain nitrogen, but owing to the comparatively small quantity obtained, namely, less than two ounces from thirty pounds of citric acid, the author has, as yet, been unable thoroughly to investigate their nature.—Prof. H. E. Armstrong also read a paper "On the Nitration of the Dichloro-Sulphonic Acids," being a continuation of his researches on the isomeric nitrochloro-phenols and their derivatives; after which the meeting adjourned until January 18, 1872.

Paris

Academy of Sciences, Dec. 18, 1871.—M. Chasles read a continuation of his theorems relating to the harmonic axes of geometrical curves, and presented a note by M. Halphen on right lines which fulfil given conditions.—M. H. Resal presented a memoir on the conditions of resistance of a fly-wheel, and M. Combes a note by M. Haton de la Goupillière on the transformation of the potential by reciprocal *radii vectores*.—Telegrams received from M. Janssen, with regard to his solar observations at Ootacamund, were communicated to the Academy. —Several members referred to the prevalence of cold during the first half of the month of December 1871.—M. Delaunay called attention to the remarkable concurrence of a change of barometric pressure with an alteration in the temperature of different parts of Europe between the 6th and 9th of December, the latter date showing the maximum of cold at Paris. The great cold of the 9th of December was also the subject of a note by M. E. Becquerel, who gives a minimum temperature of $-25°\cdot5$ C. ($=-13°\cdot9$ F.) at Montargis, and of $-27°\cdot5$ C. ($=-17°\cdot5$ F.) near Courtenay in the department of the Loiret. M. C. Sainte-Claire Deville remarked upon the concordance of this statement of M. E. Becquerel's with the minimum of $-26°$ C. ($=-14°\cdot8$ F.) recorded at Nemours. He also presented a table of minima obtained at various places in France from 7th to 15th December. —MM. Becquerel presented a memoir on the influence of snow on the temperature of the soil at various depths, according as it is covered with turf or denuded, founded chiefly on observations made from the 5th to the 15th December. The authors found that the temperature under the turfed soil, within two or three centimetres of the surface, was always above 0° C. (= 32° F.), and as constantly below that point in the naked soil.—M. Pasteur presented a note on a memoir by M. Liebig, relating to fermentation, in which he defended his views as to the nature of the phenomena of fermentation from certain criticisms upon them published by Prof. Liebig. Upon this subject M. Fremy also spoke at considerable length in opposition to M. Pasteur, who replied.—M. Bussy communicated a note by M. E. Bourgoin on the complex nature of cathartine, in which the author states that this substance, regarded as the active principle of senna, is in reality composed of three distinct substances, namely, chrysophanic acid, a dextrogyrous glucose, and a new principle to which he gives the name of chrysophanine.—M. Daubrée communicated a note by M. F. Gonnard, on the dolerites of the Chaux de Bergonne and the zeolites which they contain. In this paper the author ascribes very peculiar magnetic properties to the solid dolerite of this locality, and states that the cavities of its lower amygdaloidal parts contain three zeolites (christianite, phacolite, and mesole).—M. Trécul presented a note on the remarkable arrangement of the stomata in various plants, and especially in the petiole of ferns, in which he mentioned the occurrence of stomata upon the piliform appendages of the petiole in *Philodendron Lindenianum*, and noticed their existence in unusual positions in many ferns.—A note by M. P. Bert, on the influence of different colours on vegetation, was communicated by M. Milne-Edwards. His general results are as follows:—green is nearly as fatal to plants as total darkness, red is very injurious, and yellow less so than red, but more so than blue, but any colour taken isolatedly is injurious to plants.

December 26, 1871. A note by M. Brioschi, on the equation of the fifth degree, was read.—A note was read on the tension of the vapour of mercury at low temperatures, by M. Regnault, in which he claims to have proved long ago that mercury gives off vapours even below the freezing point of water. Upon this paper M. Boussingault made some remarks.—M. P. A. Favre presented a paper "On the Electrical Conductibility of Liquids without Electrolysis," in which he gives the details of certain experiments which seem to show that liquids have a conductibility of their own.—M. S. Meunier read a note on the co-existence of two lithological types in the same fall of meteorites. The author stated that the specimens in the Museum at Paris, from the falls of Sigena in Spain, on November 17, 1773, and of Trenzano in Italy on November 12, 1856, each includes two forms of rock, one, the Indian meteoric stone, described by Maskelyne under the name of *bustite*, the other identical with parnallite. He remarked upon the singularity of this phenomenon, which, he thinks, indicates that the stones which fell at Trenzano and Sigena were derived from the same deposits, and that bustite and parnallite have been stratigraphically related.—M. W. de Fonvielle presented an explanation by means of the theory of fringes of the appearance of luminous halos observed during balloon ascents.—M. Berthelot communicated a further series of thermo-chemical investigations upon the state of bodies in solutions, in which he discussed his researches upon the double decomposition of certain metallic salts.—A note was read on an apparatus for measuring the temperature of alterations and detonations of explosive compounds by MM. L. Leygue and Champion. This apparatus consists of a bar of metal to be heated at one end, upon various parts of which the explosive compounds may be placed.—M. F. Pisani communicated an analysis of the amblygonite (montebrasite) of Montebras, showing that the only difference between this mineral and the amblygonite of Arnsdorf consists in its containing a little less soda.—M. A. Trécul read an important memoir on the origin of the lactic and alcoholic yeasts, upon which M. Pasteur made some remarks.—M. H. Sainte-Claire Deville presented a note by M. F. Cailletet on the origin of the carbon fixed by plants containing chlorophyll, which he regards as wholly derived from the carbonic acid of the atmosphere; and M. Béclard referred to memoirs presented by him in 1858 on the influence of violet light upon vital phenomena.

BOOKS RECEIVED

English.—Researches of the Calculus of Variations: I. Todhunter (Macmillan and Co.).—Volcanoes, the Characters of their Phenomena: J. P. Scrope (Longmans).—A Vision of Creation, a Poem: C. Collingwood (Longmans).—Hymns for Modern Man: H. Noyes (Longmans).

Foreign.—Principes de Biologie appliqués à la Medecine; Dr. Ch. Girard (Baillière et fils).

PAMPHLETS RECEIVED

English.—Journal of the Iron and Steel Institute, Vol. II., No. 4.—Quarterly Journal of Amateur Mechanical Science, No. 4.—Science Directory of the Department of Science and Art.—Meteorological Notes for use in Science Classes; J. H. Collins.—Remarks on certain Oceanic Explorations: W. L. Jordan.—On Ocean Currents, Part 3. Jas. Croll.—The Quarterly German Magazine for November.—Inaugural Address before the Scottish Arboricultural Society: R. Hutchison.—Public School Reforms: M. A. B.—The Fauna of Devon, Part 7: E. Parfitt.—On the Boring of Molluscs, &c.: E. Parfitt.—Transactions of Engineers and Shipbuilders in Scotland.—Eight Days with the Spiritualists: Jas. Gillingham.—Report of the Board of Visitors to the Royal Observatory, Edinburgh.—Figures of Characteristic British Fossils, Part 3: W. H. Baily.—Method of Teachiog Arithmetic: J. G. Fitch.—On the Relation of Therapeutics to Modern Physiology: R. Madden.—On the Method of Measuring the Lateral Diffusion of a Current: J. G. H. Gordon.—The Power above Matter: D. de B. Hovell.—Annual Report of the Council of the Institution of Civil Engineers.—Mining Magazine and Review, No. 1.—Ordinary Meetings of the Newcastle-on-Tyne Chemical Society, 1871-72.—Annual Report and Transactions of the Plymouth Institute, Vol. II., Part 2; Vol. III., Parts 1, 2; Vol. IV., Parts 1, 2.—Denudation in relation to Sedementary Stratification: G. Race.—List of Members of the Royal Microscopical Society, 1871.

American and Colonial.—Notes of some Cretaceous Vertebrates: E. D. Cope.—Preliminary Catalogue of the Bright Lines in the Spectrum of the Chromosphere: C. A. Young.—Monthly Notices of Papers and Proceedings of the Royal Society of Tasmania. 1870.—A Catalogue of the Birds of New Zealand: F. W. Hutton.—Remarks on the Adaptive Colouration of Mollusca: E. S. Morse.—Transactions of the Entomological Society of New South Wales, Vol. II., Part 3.

Foreign.—Ofversigt af kongl. Vetenskaps Akad. Förhandlingar, Nos. 3, 4, 8, 9, 10.—Zeitschrift für Ethnologie, No. 5.—Zeitschrift für Meteorologie No. 23.—Giornale di Sicilia, No. 268.—Nova plantarum species: A. Kerner.—Konnen aus Bastarten Arten werden: A. Kerner.—Ueber Iris Cengialti Ambrosi: A. Kerner.—Ueber den Einfluss der Winde auf die Verbreitung der Samen: A. Kerner.—Association Scientifique di France, No. 213.—Gazzetta Chimica Italiana, No. 9.—Sul brumuro di etilidene: E. Paterno.—Sintesi due nuovi clorobromuri di carbonio: E. Paterno.—Azione del bromocloruro di fosforo al clorales: E. Paterno.

DIARY

THURSDAY, January 4.

London Institution, at 4.—The Philosophy of Magic. 3. The Magic of the Mediums: J. C. Brough, F.C.S.

FRIDAY, January 5.

Geologists' Association, at 8.—On the Overlapping of Several Geological Formations on the North Wales Border: D. C. Davies.

SATURDAY, January 6.

Royal Institution at 2.—On Ice, Water, Vapour, and Air: Dr. Tyndall. (Juvenile Course.)

SUNDAY, January 7.

Sunday Lecture Society, at 4.—On Atoms; being an explanation of what is definitely known about them: Prof. W. K. Clifford, M.A.

MONDAY, January 8.

Royal Geographical Society, at 8.30.—On Bunder Murayah, Somal Land: Capt. S. B. Miles.—On a Journey to the Murut Country in Northern Borneo: Lieut. De Crespigny.—On a Description of Fernando Noronha: Dr. A. Rattray.

Victoria Institute, at 8.—Chance Impossible: Dr. J. H. Wheatley.

TUESDAY, January 9.

Photographic Society, at 8.—On Photography in the Printing Press: J. R. Sawyer.

WEDNESDAY, January 10.

Geological Society, at 8.—On the Foraminifera of the family Rotalinæ (Carpenter) found in the Cretaceous formations, with Notes on their Tertiary and Recent Representatives: Prof. T. Rupert Jones, F.G.S., and W. K. Parker, F.R.S.—Notes on the Geology of the Plain of Morocco and the Great Atlas: Geo. Maw, F.G.S.—Further Notes on the Geology of the Neighbourhood of Malaga: M. D. M. d'Orneta.

THURSDAY, January 11.

Royal Society, at 8.30.

Society of Antiquaries, at 8.30.

Mathematical Society, at 8.—On the Surfaces the loci of the vertices of cones which satisfy six conditions: Prof. Cayley.—On the Constants that occur in certain summations by Bernouilli's series: J. W. L. Glaisher.—On the Construction of large tables of divisors and of the factors of the first differences of prime powers: W. B. Davis.—On the Parallel Surfaces of Conicoids and Conics: S. Roberts.

London Institution, at 4.—The Philosophy of Magic. 4. The Magic of the Laboratory: J. C. Brough, F.C.S.

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NOTICE

We beg leave to state that we decline to return rejected communications, and to this rule we can make no exception. Communications respecting Subscriptions or Advertisements must be addressed to the Publishers, NOT *to the Editor.*

THURSDAY, JANUARY 11, 1872

*THE UNITED STATES DEPARTMENT OF AGRICULTURE**

THE absence of a Department of Agriculture from the complicated scheme of British Government offices leads us to inquire whether it is possible for such a Department in the United States to publish annually eleven or twelve hundred pages of matter useful to the agricultural community, and whether those publications have any considerable circulation in the country.

The question of circulation is abundantly answered by a resolution of the House of Representatives passed on July 14, 1870 (the Senate concurring), which enacted, "That there be printed of the Annual Report of the Commissioner of Agriculture for 1869 *two hundred and twenty-five thousand extra copies*, one hundred and eighty thousand of which shall be for the use of the House, twenty thousand for the use of the Senate, and twenty-five thousand for distribution by the Commissioner of Agriculture." These figures are so startling in their magnitude that they seem to prove too much, until we recollect that the United States of America extend over an area proportionately enormous, including every gradation of climate, from the sub-tropical to the sub-arctic, and every variety of culture, from the cotton and rice of the south to the corn and roots of the north.

That these publications contain matter useful to the agricultural community will be readily admitted after even a cursory examination of either of the volumes; and a careful study of the official reports will lead many people to ask why we in England are not similarly favoured. The United States' Department of Agriculture fulfils two functions. It is primarily a Department of Administration, but it is also charged to acquire information concerning agriculture by means of books and correspondence, by practical and scientific experiments, by the collection of statistics, and by any other appropriate means. The papers in its annual volume include well-considered reports by all the chief officers of the Department, including, besides the Commissioner himself, the statistician, the chemist, the entomologist, the superintendent of the garden and grounds, the botanist, the editor, and others. The papers beyond these official documents consist, for instance, of Reports on Agricultural Education in Europe, on the Beet-Sugar Industry in Europe, on the Agricultural Resources of Alaska, on Agricultural Meteorology, &c. There are also papers on special subjects, many of them of the highest scientific value, such as are published in the journals of agricultural and other societies, and which may be regarded as supplementary to the strictly official work of the Department.

With such a sketch of the United States Department of Agriculture before us, it seems worth while, even in the pages of a scientific journal, to compare it with our English institutions. We have no representative of it as a department of administration; but we have a series of unconnected departments and commissions, which are as fancifully associated and divided as the stars of heaven in the time-honoured system of constellations. The Privy Council, for instance, takes cognizance of science and art, the education of children, and the diseases of animals. But why it should be the duty of the same high official to protect our flocks and herds from scab, cattle-plague, and other contagious diseases, and at the same time to educate our children, we cannot understand. Is the Vice-President of the Privy Council an *ex officio* Admirable Crichton, or is there some mysterious connection between the three R's and pleuro-pneumonia? Another of our agricultural anachronisms is the Copyhold, Tithe, and Enclosure Commission, which is the State authority on drainage and cottages, as well as the national land surveyor, valuer, and actuary. The Statistical Department of the Board of Trade is entitled to great praise for the manner in which it performs its varied work, including, besides a statistical report on the imports and exports of the United Kingdom, a fair statement of the agricultural condition of the country from year to year. Leaving out of the question the new Local Government Board, the Local Government Act Department, the Poor Law Board, and other departments which are more or less connected with the agricultural interest at home, we come to the Board of Customs, on which agriculturists are dependent for the enumeration of our agricultural imports and exports, while the nation looks to it for the collection of the revenue on our claret and cigars.

Neither as a means of disseminating information have we any representative of the United States Department of Agriculture, with its Annual Report, printed at the expense of the State in editions of nearly a quarter of a million. It is true that the Royal Agricultural Society of England, with less than 6,000 members, does more, probably, in its special walk than any other private society in the world; but it is still nothing more than a private society, and it cannot possibly, therefore, cover the whole ground required by the progressive agriculture of the present day. Indeed, it is, by its charter, expressly prohibited from interfering in matters which are questions of either law or politics. Its efforts are therefore confined to "practice" and "science," and it supports a large staff of scientific officers, including a chemist, botanist, veterinary inspector, engineer, and others, absolutely without State aid; it also expends at least 2,000*l.* per annum in testing machinery; gives away 3,000*l.* per annum in prizes for the best animals; promotes experimental investigations; and incurs very serious risk in exposing adulterations of manures and feeding stuffs.

It may, doubtless, be urged that if English farmers can do so much for themselves they require no help. But practically our Government has found out that there are things to be done which only a Government can do. Thus, after the nation had suffered fearful losses by the ravages of cattle-plague, it ordered an investigation of the subject, and—published a blue-book. After the condition of the agricultural labourer, and especially of women and children employed in agriculture, had been stigmatised as a blot on our civilisation, it issued a Royal Commission, and the result of this excessive effort for the advancement of agriculture was—a series of blue

* Report of the Commissioner of Agriculture for the year 1868, 8vo, pp. 671, Washington, 1869. Ditto for 1869, 8vo, pp. 702, Washington, 1870. Monthly Reports of the Department of Agriculture for the year 1868, 8vo, pp. 483, Washington, 1868. Ditto for 1869, 8vo, pp. 419, Washington, 1869. Ditto for 1870, 8vo, pp. 498, Washington, 1871.

books. But who reads blue-books? Farmers cannot perform successfully a feat which almost baffles the best-trained member of Parliament. What they want is a Department of Agriculture which shall improve the laws of the land, as well as investigate obscure subjects, and circulate the official reports in the manner of the United States department, in editions of a quarter of a million. The United States Commissioner not only expounds the laws of the federation on roads, fences, &c.; but he learns, for instance, that the beet sugar industry of Europe, and the system of agricultural education in Germany and other countries, present instructive features to the intelligent agriculturist, and he therefore sends a qualified commissioner to report on each of these subjects. American farmers are thus enlightened on European agriculture sooner and more authoritatively than we, who are separated from the Continent by nothing more than a "streak of silver sea." There are our Colonies also; and we would on their behalf inquire whether an intending emigrant to Canada, New Zealand, Australia, or the Cape, can obtain as much reliable information on their agriculture as the American farmer now possesses about his country's recent purchase, Alaska? It thus seems clear that the United States Department of Agriculture presents features which may be profitably copied by our Executive Government, and others which are equally instructive both to our agriculturists and to our men of science.

AGASSIZ'S SEASIDE STUDIES

Seaside Studies in Natural History. By Elizabeth and Alexander Agassiz. Marine Animals of Massachusetts Bay: Radiates. 2nd ed. (London: Trübner & Co., 1871.)

THIS is a reprint, with a few additions, of the charming work which became so popular in America and in England some five years since on account of its intrinsic merits and the beauty of the illustrations. The book includes descriptions and more or less truthful illustrations of the Actiniæ, Madreporaria, Alcyoninæ, Acalephæ, Hydroids, Holothurians, Echinoidea, and Asteroidea which may be found in the neighbourhood of Massachusetts Bay. The history of the development of many of the forms is carefully written, and is obviously the result of patient original observation.

In noticing the reproduction of the Actiniæ the authors remark that the eggs which hang on to the inner edge of the partitions of the visceral cavity drop off into it during different stages of development. Ordinarily they are passed out through the mouth as Planula-shaped ciliated creatures, which soon become attached to a foreign substance. The base enlarges, and the free extremity falls in to form a concavity, the future gastric and visceral cavity. But sometimes the embryo is provided with tentacles and with its stomachal cavity before it escapes. Lacaze-Duthiers has described a similar state of things in the reproduction of *Corallium rubrum*, and probably the embryonic condition of all the stony corals is that of a free swimming sac which undergoes metamorphosis. These usually sedentary Actiniæ are not without nomadic species, and *Arachnactis brachiola* A. Ag. is described as a small floating anemone, very nocturnal in its habits, which swims with its tentacles and mouth downwards, using the body as a float. This form is not quite symmetrical, and has an evident tendency towards establishing a longitudinal axis. The mouth is out of the centre. Bicidium is noticed as selecting the mouth-folds of the common large red Cyanea as its home. It undergoes retrograde development, and its tentacles are short and stout on account of its parasitic existence.

The only stony coral described is the littoral Astrangia, which is probably a descendant of the miocene forms which once flourished on the same area. The tentacles of this coral are covered with wart-shaped masses, crowded with nematocyst lasso cells. Such forms as Caryophyllia and Balanophyllia, which are so well represented on our coasts and in thirty fathom water, do not appear to have been found by the authors in Massachusetts Bay. Amongst the Acalephæ, Cyanea, of course, is well described, and it is observed that so large a portion of its bulk consists of water that one of no less than thirty-four pounds weight being left to dry in the sun for some days, was found to have lost 99 per cent. of its original weight. Writing of the not very attractive appearance of these huge jelly fish, Agassiz observes that "to form an idea of his true appearance, one must meet him as he swims along at midday, rather lazily withal, his huge semi-transparent disc with its flexible lobed margin glittering in the sun and his tentacles floating to a distance of many yards behind him. Encountering one of these huge jelly fishes when out in a rowing boat, we attempted to make a rough measurement of his dimensions upon the spot. He was lying quietly near the surface, and did not seem in the least disturbed by the proceeding, but allowed the oar, eight feet in length, to be laid across the disc, which proved to be seven feet in diameter. Backing the boat slowly along the line of the tentacles, which were floating at their utmost extension behind him, we measured these in the same manner, and found them to be rather more than fourteen times the length of the oar, thus covering a space of some hundred and twelve feet." This huge mass is produced by a hydroid measuring not more than half an inch in length when full grown.

The parasitic early life of *Campanella pachyderma* A. Ag. appears to throw a doubt whether this acaleph passes through the hydroid state or not. Should the eggs develop at once into the medusa in this instance, there is no small significance to be attached to the fact. An anomaly of an opposite character is noticed in the case of *Laomeda amphora* Ag. This campanularian develops medusæ which never separate from the parent hydroid, but wither on its stem after having laid their eggs. The development of these abortive medusæ is not far advanced. This species flourishes in the sewage of Boston. There is a very admirable drawing of *Tubularia Couthouyi* Ag., a tubularian whose medusæ buds are never freed from the stem, and do not develop into full-grown jelly fish, but always remain abortive. These buds cluster like a bunch of grapes under the expanded umbrella-shaped tentacles of the hydroid, which are gracefully supported by a curved stem.

The process of the budding of the medusæ of Hybocodon, where small jelly fish similar to the original grow by gemmation from a large tentacle, is well described, and the hydroid stage and general want of symmetry in the medusa also. Then the budding from the proboscis of *Dysmorphosa fulgurans* A. Ag. is noticed, and the nomadic or free-floating hydroid Nanomia also. Synapta, amongst the Holothurians, is noticed on account of its curious sand-ring clothing. "They live in very coarse mud, but they surround themselves with a thin envelope of fine sand, which they form by selecting the smaller particles with their tentacles, and making a ring around their anterior extremity. This ring they then push down along the length of the body, and continue the process, adding ring after ring, till they have entirely encircled themselves with a sand tube. They move the rings down partly by means of contractions of the body, but also by the aid of innumerable appendages over the whole surface. To the naked eye these appendages appear like little specks on the skin; but under the microscope they are seen to be little warts projecting from the surface, each one containing a little anchor with the arms turned upward. Around the mouth the warts are larger, but do not contain any anchors." "By means of these appendages, though aided also by the contractions of the body, the Synaptæ move through the mud, and collect around themselves the sand tube in which they are encased." They gorge themselves with mud and sand for the sake of the nutritious substances they may contain. The office of the pedicellariæ of the Sea Urchin is well described, as follows:—"If we watch the Sea Urchin after he has been feeding, we shall learn at least one of the offices which this singular organ performs in the general economy of the animal. That part of his food which he ejects passes out at an opening on the summit of the body, in the small area where all the zones converge. The rejected particle is received on one of these little forks, which closes upon it like a forceps, and it is passed on from one to the other down the side of the body till it is dropped off into the water. Nothing is more curious and entertaining than to watch the neatness and accuracy with which this process is performed. One may see the rejected bits of food passing rapidly along the lines upon which these pedicellariæ occur in greatest number, as if they were so many little roads for the carrying away of the refuse matters; nor do the forks cease from their labour till the surface of the animal is completely clean and free from any foreign substance." Some higher animals might take a profitable lesson from the Urchin. The Crinoids are passed by rather briefly. The existence of Comatulæ from Greenland to South Carolina is mentioned, but the authors do not appear to have devoted special attention to them. A very excellent notice of the embryology of the Echinodermata precedes the last chapter, which consists of a brief *résumé* of the distribution of life in the ocean. The book might be taken as a model by many European naturalists who write popular works, for there is a vast amount of philosophy in it. The authors have not contented themselves with serving up a number of "wonders" for the public bewilderment; nor have they simply given us a series of descriptions of forms, as is the practice especially amongst those who trade upon butterflies and beetles; but they have taken a vast amount of trouble in explaining the development and embryology of the Invertebrata which have come under their notice. In fact, they have given a reasonable amount of bread with their "sack."

P. M. D.

EARNSHAW'S DIFFERENTIAL EQUATIONS

Partial Differential Equations. An Essay towards an entirely new Method of Integrating them. By S. Earnshaw, M.A. (Macmillan and Co., 1871.)

THE present work, as its title indicates, contains a detailed explanation of a new method of integrating Partial Differential Equations; it is in no sense a text-book or introduction to the subject. The author's object is not to collect and describe the known methods, but to develop a new one. The principle of the method is easily explained and understood. The independent variables in the given differential equation being $t, x, y, z \ldots$, we can transform it so that the new independent variables are $t, \xi, \eta, \zeta, \ldots$, by equations of the form $D_t u = d_t u + d_x u \,.\, D_t x + \ldots$; but the practical application of the method consists in comparing the original equation with the equation last written, and thus determining relations from which, by the elimination of $\xi, \eta, \zeta \ldots$, the integral of the original differential equation is found. The quantities $t, \xi, \eta, \zeta, \ldots$, with the exception of the one with regard to which the differentiation is being performed, are treated as constants, and are here called *quasi-constants* (semi-constants we should have preferred). Mr. Earnshaw, as is apparent from the equation of transformation quoted above, adopts d when the differentiation is with regard to the old variables, and D when with regard to the new; the suffix notation for differential coefficients is also made use of. For this latter departure from custom the author in the preface offers an apology, and states that he has been warned that it "will form a serious hindrance to the acceptableness of the present work." This fear we think is groundless; the notation is not inconvenient in such investigations as the present, as it somewhat simplifies the appearance of the equations without rendering the analysis more difficult to follow.

In the first few chapters the method is applied to the integration of numerous equations of the first and second orders, and throughout the book the applications to particular cases are so numerous that whole chapters consist entirely of "examples worked out." This excessive number of examples is a drawback, as many of them (for instance, all in Chapter V., which treats of linear equations of the second order with constant coefficients) can be more simply and perfectly discussed by Boole's symbolic and other methods. The reader is also left in doubt as to how far the examples have been chosen so as to suit the method of solution here adopted. In the development of a new principle it is always a matter of great importance to point out the cases in which it enables us to obtain results previously beyond our reach, and also the cases in which the previous methods are preferable. This Mr. Earnshaw does not appear to have done; he has integrated a great number of equations, many of which, however, are capable of solution by well-known methods in as straightforward a way as ordinary quadratics in algebra. It is, for such reasons as these, generally desirable that original mathematical investigations should appear first in the memoirs of a Society or other recognised organ, where the new matter is distinctly stated, rather than in the form of a book where there is nothing to check the temptation to overburden the explanation with examples. Mr. Earnshaw claims to have for the first time integrated in finite terms several most important partial differential equations of the second order, including the equation of continuity in a homogeneous incompressible fluid; and the chapters in which these equations are discussed are by far the most important and interesting in the work. Mr. Earnshaw is already known for his able treatment of the equation for the motion of a sound wave in the Philosophical Transactions for 1860, and no one can doubt the importance of the subjects suggested for consideration by this and other equations. The question is discussed whether there must necessarily exist an integral of every partial differential equation that can be proposed, and on this part of the subject we wish the author had extended his remarks. The real question considered seems however rather to be the possibility of the existence of a continuous function expressible in finite terms as an integral. With regard to the considerations having reference to certain physical problems, we should not expect to learn very much from the discussion of such questions, as the differential equation might admit of a solution incapable of satisfying the physical conditions.

We must notice one singular error made by Mr. Earnshaw. He concludes that the well-known partial differential equation of the second order of surfaces having their principal radii of curvature equal and of opposite signs at all points, admits of no integral, because the form of a surface possessing this property would be such as could not exist; but it is well known that the surface formed by the revolution of a catenary round its directrix does possess the property in question, and it is easy to see that this arises simply from the fact that the normal and radius of curvature in the catenary are equal and of opposite signs; the form of the surface is quite easy to conceive. A particular integral of the equation obtained by Poisson's method is also given in Boole's Differential Equations, chapter xv. Even admitting Mr. Earnshaw's reasoning, it would only establish the non-existence of a real surface possessing the required property. The integrals of the equation of continuity in three dimensions, and of one or two other equally important equations, we do not remember to have seen before, and they are perhaps the most general finite solutions the equations admit of. Of the value and power of the method it is impossible to speak at present; but we heartily commend Mr. Earnshaw's book to the reader as one containing much matter of great interest systematically and clearly developed and treated by a novel method. It is remarkable that the subject of partial differential equations has not attracted more attention than it has in recent years, as an advance in this quarter is more immediately felt in physics than an advance in any other pure mathematical subject. The present work will help to bring the matter prominently forward; and as the analysis is nowhere of a very difficult nature, it will probably come under the notice of many readers not accustomed to study mathematical memoirs on their appearance.

If the work had been intended to be a Treatise on the subject, we should have had good reason to object to the total omission of all reference to the usual methods, but the title and preface explain that this was not contemplated; it is one of the few English books containing original mathematics.

J. W. L. G.

OUR BOOK SHELF

Three and Four Place Tables of Logarithmic and Trigonometric Functions. By James Mills Peirce. 16 pp. (Boston: Ginn Brothers, 1871.)

PERHAPS the best way of treating this work, which does not contain a single word of explanation, will be to give a summary of the tables contained in it. First we have proportional parts of all numbers up to 100; then on one page three-place logarithms of numbers and of the six trigonometric functions, natural and logarithmic. On pages 4 and 5 we find four-place logarithms of numbers, then logarithms of sums and differences (Gaussian logarithms) also to four places, then follow tables of logarithmic trigonometric functions, inverse trigonometric functions (a new table, to which attention is specially invited, for finding angles from the logarithms of their trigonometric functions), traverse table, the correction of the middle latitude (in an improved form), and meridional parts.

In a prospectus issued by the publishers, it is stated as a result of experiment that it has been found that the times occupied, in regular computation, in doing one piece of work by tables of 4, 5, 6, and 7 places, are proportional to the numbers 1, 2, 3, and 4; hence it is that the author has drawn up the majority of the tables under review to 4 places as sufficient for ensuring the degree of accuracy usually required in computations of common surveying, engineering, &c.

The type employed is very clear, the arrangement of the work is good, and the printer's part has been well done; the book requires only a few words of elucidatory matter. There is on the last page a useful Table of Constants with their logarithms, here we observe a few symbols which are new to us, and which are presented to our notice on the Title-page.

After all the value of such a work consists in its accuracy, and that can only be tested by practice, "the greatest pains have been taken both in preparing and printing to secure perfect accuracy." We commend the work to the notice of such as agree with old Burton (Anatomy of Melancholy, pt. II., sec. 2), "What so pleasing can there be . . . if a man be more mathematically given (as) to calculate or peruse Napier's logarithms, or those tables of artificial sines and tangents, not long since set out by . . . . Edmund Gunter, which will perform that by addition and subtraction only, which heretofore Regiomontanus' tables did by multiplication and division." But then the same quaint writer advises those who are melancholy to square a circle; does it follow that all circle-squarers are melancholy? R. T.

The Laws of the Winds prevailing in Western Europe. By W. Clement Ley. Part I. (Stanford, 1872.)

EVEN when we differ from an author's conclusions, the work of one who shows himself an honest and capable inquirer has a just claim to our attention. Mr. Ley evidently writes from practical knowledge of his subject, and his assiduity in collecting and charting observations must have entailed on him an amount of labour which only those who have been engaged in similar work can thoroughly understand. Unfortunately, as it appears to us, he has confined his investigations almost entirely to the limits set forth on his title-page; and the winds of Western Europe, though highly suggestive and subject to more exact observation than any others except those of the United States, are by no means to be taken as representative. Mr. Ley has taken them as such, and has thus laid down a series of general propositions, which may be briefly summed up in one—that revolving storms are caused by the barometric depression consequent on heavy rain over a large area. He brings forward some curious home instances in illustration of this; but looking farther afield, on the slopes of the Himalayas—to mention only one locality—a much heavier and longer continued precipitation than any he has instanced takes place every summer, and does probably cause a very great depression of the barometer, but certainly does *not* give rise to any winds such as he has described. On the hills of Khasia, again, where the unparalleled rainfall is as much as from 30 to 40 inches a day for days together, and puts the paltry $\frac{1}{2}$ or $\frac{3}{4}$ of an inch a day of Mr. Ley's examples almost beyond the pale of comparison, no such storms are generated. In the same way, the explanation of the eastward direction which these barometric depressions take in our latitudes, which differs only in its greater detail from that given by Prof. Mohn in the "Storm Atlas," is applicable only to temperate latitudes; the westward advance of tropical cyclones cannot be referred to it; and it seems to us improbable in the extreme that the course of a storm is regulated by one law in one part of the world, and by a totally distinct law in another. Besides this, in the detailed application of the law which he deduces for Western Europe, the author appears to fall into the mistake of attributing the rainfall of mountain districts to the mere contact of the moist air with the cold mountain slope; that this is not the case—that it is due rather to the hoist into the upper regions which the air receives on impinging against the slope—is curiously shown by the fact that, when the hills are not high, most rain falls on the lee side. One familiar instance of this will illustrate our meaning. The gauge which in all England shows the greatest rainfall is at Stockley Bridge, just above Seathwaite; it is distinctly under the lee of the ridge which joins Great Gable to Great End, and separates Wastdale from Borrowdale. The mist, blown in from seaward, fills Wastdale, and is lifted up the slope of this ridge (Stye Head Pass). Crossing over out of Wastdale, the mist curling up the hill is frequently so thick that the path cannot be seen 10 feet in advance; but immediately on reaching Stye Head Tarn the mist vanishes, to fall as rain over Seathwaite. But altogether, though we admit neither the author's premises nor his conclusions, his work is none the less highly interesting. It does not contain much that is new, but it discusses and illustrates the theories of Mohn and Buchan in greater detail than has yet been attempted. We would, however, decidedly object to the *ex cathedrâ* tone which is occasionally adopted. In empirical science very little is "obvious," and perhaps nothing is a "truism;" certainly the influence attributed to the earth's rotation is neither one nor the other, for it is denied, disputed, and doubted by very many capable meteorologists. J. K. L.

The Young Collector's Handy-book of Botany. By the Rev. H. P. Dunster. (London: L. Reeve and Co., 1871.)

We opened this little book with pleasure, hoping to find in it an addition to the too few popular manuals of botany, and the pleasure was increased by recognising at the end some familiar and excellent illustrations. Great therefore was our disappointment when we found that instead of "assisting the student in the beginning of his work by setting him forward on a right road," as is stated in the Preface to be its object, it would be far more likely to mislead him. Botany seems to be peculiarly unfortunate, in that every one who is fond of flowers thinks himself capable of writing a handbook, without himself possessing any accurate scientific knowledge of his subject. Some of the definitions given in this book are so bad that we should have been surprised to find them in the answers to the examination papers of the botanical classes in any of the great schools where natural science is now taught. Take four examples:—"Albumen: a gummy substance surrounding certain seeds;" "Embryo: the leaf in an immature state;" "Matrix: that upon which any other thing grows;" "Petals: leaves while in the corolla." After this we are somewhat prepared to hear that the corolla "is made up of petals which, when expanded, are the flower-leaves, *and* of the stamen and pistils;" and that "county collections (of ferns) are valuable as illustrations of the *fauna* of particular parts." We are utterly unable to see the object gained by the publication of this book, when beginners already have such admirable manuals as Oliver's "Lessons in Elementary Botany," Lindley's "School Botany," and Cooke's "Manual of Structural Botany," neither of which, by the way, is mentioned by Mr. Dunster in the list of books recommended to the learner. Especially are we unable to understand how the names of respectable publishers, who have issued many admirable works on natural history, come to be appended to a book of this character. As we see that it is intended to be the first of a series of Handy-books upon "the popular and recreative sciences," we would recommend the publishers to submit the manuscript of the remainder of the series to a competent judge before publication. 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

Leaving out of account a few small inland seas, the globe may be said to have but one sea, as well as but one atmosphere. We have, however, accustomed ourselves to speak of parts, or geographical divisions, of the one great ocean, such as the Atlantic and the Pacific, as if they were so many separate oceans. We have become accustomed, also, to regard the currents of the ocean as separate, and independent of one another; and this idea has, no doubt, to a considerable extent, militated against the acceptance of the theory, that the currents are caused by the winds, and not by difference of specific gravity, for it leads to the conclusion that currents in a sea must flow in the direction of the prevailing winds blowing over that sea.

The true way of viewing the matter, as I hope to be able to show in my next letter on the cause of Ocean Currents, is to regard the various currents merely as members of one grand system of circulation, produced, not by the trade winds alone, as some suppose, but by the combined action of all the prevailing winds of the globe, regarded also as one system of circulation.

If the winds be the impelling cause of currents, the *direction* of the currents will depend upon two circumstances, viz. (1) the direction of the prevailing winds of the globe; and (2) the conformation of sea and land. It follows, therefore, that as a current in any given sea is but a member of a general system of circulation, its direction is determined, not alone by the prevailing winds blowing over the sea in question, but by the general system of prevailing winds. It may, consequently, sometimes happen that the general system of winds may produce a current directly opposite to the prevailing wind blowing over the current.

Taking into account the effects resulting from the conformation of sea and land, the system of ocean currents is found to agree exactly with the system of the winds. I trust to be able to show that all the principal currents of the globe, the Gibraltar current not excepted, are moving in the exact direction in which they ought to move—assuming the winds to be the sole impelling cause. Given the system of winds and the conformation of sea and land, the direction of all the currents of the ocean, or more properly the system of oceanic circulation, can be determined *a priori*. Or given the system of the ocean currents, together with the conformation of sea and land, and the direction of the prevailing winds can also be determined *a priori*. Or, thirdly, given the system of winds and the system of currents, and the conformation of sea and land may be, at least, roughly determined. For example, it can be shown by this means that the Antarctic regions are probably occupied by a continent, and not by a number of separate islands, nor by a sea.

The influence of the rotation of the earth on ocean currents has certainly been greatly over-estimated. Rotation, as is well known, exercises no influence in generating motion in any body placed on the earth's surface; but if this body be already in motion, no matter in what direction the motion may be, rotation will deflect it to the right on the northern hemisphere, and to the left on the southern hemisphere, as has been shown by Mr. Ferrel. But it must be borne in mind that the deflecting power of rotation depends wholly on the rate at which the body is moving. If difference of specific gravity be regarded as the impelling cause of any current, the deflecting power of rotation will certainly be infinitesimal.

Difference of specific gravity, resulting from difference of temperature between the ocean in equatorial and polar regions, might, if sufficiently great, produce some such interchange of equatorial and polar water as Dr. Carpenter supposes; but surely the difference of temperature between the equator and the poles could not produce currents like the equatorial current and Gulf Stream in a wide expanse of water. Such a general difference of temperature might tend to produce a general motion of the ocean; but it is inconceivable that it should produce motion in particular parts of the ocean, as Maury, Colding, and others, conclude.

But I think it is by no means difficult to prove that the circulation of the waters of the ocean cannot be due to the difference of temperature between the equatorial and polar regions. And Dr. Carpenter must be mistaken in supposing that it requires great mathematical skill to determine the value of the forces to which he attributes the circulation of the ocean. The whole subject, when properly viewed, resolves itself into a mechanical problem of such extreme simplicity as not to require for its solution the aid of any mathematics whatever in the ordinary sense of the term. Taking Dr. Carpenter's own data as to the difference of temperature between the waters at the equator and the poles, and also his estimate of the rate at which the temperature of the equatorial waters decreases from the surface downwards, I have, in my paper in the *Philosophical Magazine* for October last, proved that the amount of *force* which gravity exerts on, say, a pound of water, tending to make it move from the equator to the poles supposing the pound of water to be placed under the most favourable circumstances possible, is only $\frac{1}{600}$ of a grain.

I have shown also that the greatest amount of *work* that gravity can perform in impelling the waters from the equator to the poles as a surface current, and back from the poles to the equator as an under current (assuming that the waters would actually move under an impulse so infinitesimal) is only nine foot-pounds per pound of water. And in regard to the Gibraltar current, the amount of work which gravity can perform does not exceed one foot-pound per pound.

If these results be anything like correct, and it be admitted that a force so small is insufficient to produce the necessary motion, then it is needless to expect that any future observations in reference to currents of the ocean will in the least degree aid Dr. Carpenter's theory; for, supposing it were found that the waters of the ocean do circulate in some such manner as he concludes—a supposition very improbable—still we should be obliged to refer the motion of the water to some other cause than to that of differences of temperature. JAMES CROLL

Edinburgh, Dec. 22, 1871

"Nature Worship"

IN a spirited article under this title in the last number of the *Medical Times and Gazette*, we are accused of "the most dismal want of appreciation of the true scope of the medical art and science." This is hard! The ground for it is to be found in the following sentences in the short notice of the Brown Institution in NATURE of Dec. 21:—

"The true physician fears to meddle with the processes of which he is the attentive and anxious spectator. Although the more ignorant members of the medical craft—the so-called 'practical' men—may sometimes, with the best intentions, experiment on their patients with harmful drugs, such experimentation is repudiated by the man of Science."

If objection had been taken by our guarded suggestion that it may happen that practitioners may sometimes use powerful agents by way of remedies without any adequate knowledge of their property, we should not have been surprised, and would have been very willing to apologise had we been assured that the insinuation was unfounded. What our critic finds fault with, however, is the second part of the sentence, viz., our assertion that such experimentation on human beings with harmful drugs is objectionable. If experiments had never been made on human beings, he argues, we should not have learnt to know some of our most useful and valuable drugs. This may be so; but even if it is, it perhaps scarcely affords a sufficient justification for a continuance of the practice.

In another part of the article we are accused of "unconsciously reproducing the superstitious and false philosophy of 2,000 years back," and we are distinguished by the epithet "Nature worshippers." Let us quote the superstitious sentence which has laid us open to so unexpected an imputation:

"The pathologist at the bedside is not in the position of an experimenter, but only in that of a student, who stands by at a greater or less distance; while another over which he has no control performs experiments in his presence without deigning to explain to him their nature or purpose."

By these words we are supposed to imply that while nature works we worship. Does the student who stands by while the professor performs an experiment in his presence, the nature of which he very imperfectly understands, ready to help if need be, but fearing to meddle or even ask a question least he spoil the wished-for result, worship his teacher? Or is it the mere speaking of Nature as a teacher at all that is superstitious and unphilosophical?

The truth is, that our contemporary has obviously found the sentences quoted from our article a convenient text for a telling homily on a subject with which our remarks had nothing whatever to do. Our object was to point out that for the purposes of pathological investigation, and for trying the action of unknown remedies, a fellow mortal stretched on a sick bed is not a fit subject; that it is better to use dogs, cats, and rabbits. His aim, on the other hand, is to impress upon his readers the important practical lesson, that the doctor when called to see a patient must not stand by inactive, but use every means at his disposal for the relief of suffering and the prolongation of life. If he had found that he could add force to the admonition by clothing it in figurative language, and had said that the physician should grapple with the disease as with a fiend, it would not have occurred to us to call him a "devil worshipper."

THE WRITER OF THE NOTICE

Prof. Helmholtz and Prof. Jevons

JEALOUS of any and every restriction to that full liberty of scientific thought which cannot be over-advocated, we have recently gone so far as to deny the necessary and universal validity of the old axioms or "self-evident principles," not only in geometry, but in logic. Now I would submit that, if without *some* elementary or initial certainties all scientific thought is impossible, we must either retract these denials altogether, or so far limit them as to leave the logical certainties intact. But can we do the latter while geometrical axioms are in dispute? Towards answering this question, I propose to consider the hypothesis advanced by Prof. Helmholtz, to be found in NATURE, No. 103, October 19, and ably commented on by Prof. Jevons.

In order to show how geometrical axioms, with conclusions based thereon, may not be necessarily or universally true, Prof. Helmholtz tells us "to imagine the existence of creatures whose bodies should have no thickness, and who should live in the mere superficies of an empty globe," and then, as a consequence, to admit that, "while, with us, 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." I propose to show that this position, so far as it affects the question, contains a logical uncertainty and unsoundness, which, if admitted, would vitiate all reasonings whatsoever.

We should premise that the "imagined creatures" are supposed to be "in possession of human powers of intellect," however their external conditions differ from ours. This assumed (and conceded), Prof. Helmholtz has to prove that the assumed difference of the external conditions will necessitate the intellectual difference assigned in his hypothesis; but he cannot assume this also without begging the whole question.

Let us first ask, what here is the import of the expression, "with them, the angles of a triangle would always, more or less, exceed two right angles"? To take the term "exceed," do the supposed beings detect the excess, or not? If they do, they find these three angles exceed two of our right angles, and they are acquainted with our right angles, and are consequently capable of conceiving four such rectilinear angles, and, thence, a rectilinear triangle with all its angles together equal to two right angles; and thus the entire supposition is unproductive. If we assert now that they do not detect the excess because they cannot, under their new conditions, conceive a rectilinear figure, we are simply begging the question we proposed to institute, viz., whether we derive our geometrical notions through our conditions, or whether these notions are intuitive? And, lastly, if we say that the beings in question take the spherical angles they have for rectilinear angles, and their four equal angles about a point for four right angles, *i.e.*, that they have our notion, but misapply it; then it follows that they have our conclusion, that the angles of a triangle together equal two right angles; and their misapplying does not avail anything, seeing that the geometrical conclusion (the universality of which is here disputed) does not propose to deal with facts, but with suppositions only. The supposed rectilinear figures of these beings are (though wanting all physical counterparts) the very figures of Euclid.

Now, first, the fallacy lies in what the late Professor John Grote called the "pseudo-psychology," the confusion of thought and thing, of the psychical and the physical. For the question is here of geometry, the science which regards (say) all the supposed or postulated rectilinear angles about a point as equal to four right angles: the question is not of the physical science which discovers "more or less" exactly what angular or other qualities may belong to any physical object; and so true is this, that geometry is not conversant with right and left hand, nor with above and below. And, secondly, the fallacy is concealed by an ambiguous use of terms in the statement, "with them, the angles of a triangle would always, more or less, exceed two right angles." The "with them" may mean with them in imagination, or with them in fact; and, but for this ambiguity, the fallacy must have exposed itself; for, first, it is obvious that two angles which they imagined right ones would, in their imagination, equal, and not be "exceeded by," the angles of a triangle they imagined rectilinear; we could not have said otherwise than this, with the case clearly stated And, secondly, we could never have said (distinctly) that the physical fact being one way or another, could affect the universality of a geometrical position which does not affirm anything of physical facts; but we should have perceived that we were only combating a statement that the angles of a physical triangle supposed to be, though not really, rectilinear, are together really equal to two right angles; a statement obviously not true, and as obviously not geometrical.

In mathematical argument, anything I should bring in aid of Prof. Jevons's able comments would be equally presumptuous and useless; and it is only because I feel that his reasonings are not quite so unassailable on the psychological side that I venture any additional evidence. Prof. Jevons asks (I think needlessly), "Could the dwellers in 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 angles the proposition would altogether fail to be verified; but they could hardly help perceiving that, as smaller and smaller angles 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." Now the terms "spherical excess" here mean the quantum by which all the angles of their triangle would, to the knowledge of these beings, exceed two *bonâ fide* right angles. They therefore know already (by Prof. Jevons's supposition) what a rectilinear angle is, and, thence, what a rectilinear triangle is with all its geometrical properties (as above shown), for it is admitted that we require no objective experience beyond that of a rectilinear angle in order to deduce said properties, and these beings, having our intellectual powers and our data, can deduce the same. I would only suggest here that, after this, to suppose any experimental evidence necessary to "verify" the proposition is very much like conceding the hypothesis that geometrical conclusions are not independent of experience.

Another point not directly met by Prof. Jevons is ingenious, but amounts to the assertion that, if we could not actually *draw* a straight line, we should not be able to define it as "the shortest distance between two points;" for these imagined beings, who cannot possess a physical straight line, will have "an infinite number of shortest lines between any two diametrically opposite points in their sphere." An argument, interesting only so far as it illustrates to what lengths of ingenuity a sophism may be carried; for have we not to prove that our geometrical conception or definition depends upon our physical experience, and are we not here advancing for proof, that beings without this experience cannot have the geometrical conception, and that they cannot have it because—we cannot have it? If anything could convince us of the inherent impotence of these *experimental hypotheses*, it should be this inevitable appearance of the "circle" just when proof is called for. And again, "shortest distance" here has two senses. First it means the shortest path available to the imagined beings, and then (in order to invalidate the definition of a straight line) it means the shortest path conceivable.

In this case it appears then (as I proposed to show) that, while the geometrical certainties have been questioned, the logical code has been violated, and all logical certainty confounded by an ambiguous use of terms. I have here attempted no demonstration of the opposite theory; but I think if the eminent supporters of the hypothesis just examined would be content to affirm roundly that all our notions, conclusions, and beliefs are mere resultants of intellectual action *plus* given experience, and to forbear any hypothetic deductions till this thesis is made good, they would find that the essence of the question is distinctly psychological, and that any experiments with hypothetical physics are so many attempts to get out of a complex thing that which is simply not in it. J. L. TUPPER

Meteorological Phenomena

ON the 10th of November, a little after 4 P.M., the sun was behind a bank of thick stratus clouds, on the upper edge of which, attached to it, about 10° above the sun's position, and 15° to 20° to the north of it, I, with two other persons, observed a small irregularly-shaped cloud, about 2° in apparent diameter, which exhibited the colours of the least refrangible portion of the spectrum, commencing with the red on the south end nearest the sun, succeeded by orange, yellow, and pale greenish yellow, fading into white on the north edge, the rays being perpendicular. This appearance continued for about five minutes or upwards while we viewed it, and then faded away. Though the phenomenon appears simple, the light cloud merely refracting the sun's rays, it is not evident why the complementary colours of the more refrangible portion of the spectrum should not have been visible; and, as far as I am aware, a similar appearance has not been recorded before. G. F. D.

IN NATURE of August 31 there is a note headed, "A Rare Phenomenon," from Magdeburg. Your correspondent, I think, evidently refers to what in India, or at any rate in Ceylon, is called "Buddhu's Rays," an appearance in the sky very commonly observed here, and for which I have never heard any scientific explanation attempted. I regret to say that hitherto I have never taken any exact notes of the position of these rays. They generally occur, I think, when the sun is low, sometimes in the west at sunset, but also occasionally in the east. The appearance presented is that of alternate broad streaks of rose colour and blue radiating from one point on the horizon, and extending, I should say, for about thirty or forty degrees. I will, whenever I see them in future, take exact notes of their position, &c. At present I can only say that I certainly think that dust in the atmosphere can take no part in their production.

Colombo, October 1871 BOYD MOSS

Crannogs in the South of Scotland

IT may interest some readers of NATURE to learn that a considerable number of crannogs, various articles of the New Stone Period, and some "kitchen-middens" have been discovered in connection with the small lochs which stud the surface of Wigtonshire and Dumfriesshire. Dowalton Loch, Machermore Loch, and the lochs which surround Castle Kennedy in Wigtonshire, have been examined within the last few years, and have disclosed ancient lake-dwellings. The Black Loch of Sanquhar and Lochmaben Loch in Dumfriesshire contain platforms of wood and stone. In some cases canoes and causeways connecting the artificial islands with the adjacent shores have been traced. Sir William Jardine, in his presidential address to the Dumfries Natural History Society, 1864–5, gives an interesting account of the crannog discovered at Sanquhar Black Loch; and recently the Rev. Geo. Wilson, Glenluce, read a detailed description of the crannogs in his vicinity to the Scottish Antiquarian Society.

J. SHAW

Freshwater Lakes without Outlet

IN your notice of Morelet's "Central America" (NATURE, December 28, 1871) you speak of the water of the lake of Peten as fresh, though without an outlet. This is uncommon, but not unexampled. The lake of Araqua in Venezuela, described by Humboldt, is of this kind, and so are the lakes near Damascus, into which the Abana and Pharpha respectively discharge. The best account of these latter is, I believe, in Mr. Macgregor's work, "The *Rob Roy* on the Jordan."

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, Jan. 1

Pupa of Papilio Machaon

WHILST working at the colour patterns of Insects in November 1867, I very carefully dissected off a portion, about one-eighth of an inch square, of the hard integument from the side of a pupa of *P. Machaon*, near the anterior extremity. The portion of the interior thus displayed was filled with a clear colourless fluid, in which was floating a delicate membrane, to which were attached several tubes, trachea, formed by a spiral fibre. In the fluid were floating many roundish grains. Another pupa of the same brood was examined January 15, 1868, and another on April 15. The floating grains were now evidently made up of ganglia of the spiral fibre of the trachea, and were connected with the tube by long pedicels of the same kind of fibre. On May 20 the tubes had enlarged to such an extent that they were almost contiguous, and were covered with minute granules, apparently incipient scales; in fact, a few small but well-formed scales appeared on one portion. The specimen examined in November was laid in cotton; a perfect cicatrice was formed, and the butterfly in excellent condition appeared at the usual time.

Rainhill, December 23, 1871 HENRY H. HIGGINS

Lunar Calendars

IN reply to "Myops" in NATURE, No. 111, p. 123, the English New Moon of the Jews is really the Month-Head (*Caput mensis*), formed from an artificial system. The true mean conjunction derived from the 19-year cycle is called the Molad or Moon-Birth, and generally differs from the festival-day.

Said artificial system consists in combining AZ, BY, CX, &c., as follows:—

1st Day of Passover has	Black Fast (9th Ab) on same week	day.
2nd „ „ „	1st of Pentecost.	do.
3rd „ „ „	1st of New Year (Tishri).	do.
4th „ „ „	Last of Tabernacles—Rejoicing of Law.	do.
5th „ „ „	White Fast (Atonement Day).	do.
6th „ „ „	Preceding Purim (Esther's Feast.	do.

This actual Jewish Calendar depends on the Moveable Feasts, 1st Passover never falling on Monday, Wednesday, or Friday.*

39, Howland Street, W., Dec. 15, 1871 S. M. DRACH

Hints to Dredgers

APPEALED to by name—spirits from the vasty deep—I have waited for my elders, also named, to answer Mr. Hennah's queries about dredging, and, failing to see anything more, I venture to trouble you with a few lines, the more so as I felt the want of advice when I was fitting out the *Norna* in 1870. Details would be out of place here; I will only at present give a few hints. And first—to repeat Punch's advice to those about to marry—if about to buy a yacht, DON'T! Begin by hiring one of the tonnage you require, the proper price being 1*l*. 5*s*. per ton per month, including the wages of skipper and crew, but rarely of cook or steward. After your first season buy by all means if you like.

If bound on a long cruise your craft should not be under 80 to 100 tons. But for dredging in the Channel or round our coasts 25 tons and upwards are sufficient; but not on any account under that. A little boat of 25 tons makes up two good berths and two more possible ones, exclusive of the crew's sleeping quarters, and being decked stands a good chance in a gale of wind.

Beware the discomfort of a half-deck and a small boat, remembering that you may unavoidably have to face some nasty breezes which an ordinary yachtsman would run away from. You may, for instance, be caught in a bay offering rich results, and have to thrash out of it.

Hire a man knowing the locality in which you desire to try your fortune.

Take a particular line, say the comparative life on the borders of fresh and salt water junctions, or at spots where the depth suddenly increases. No better locality, with a good pilot, could be picked out to begin with than the Channel Islands.

Especially note the submarine geology. Exactly fix the spots you dredge in by cross bearings. A small prismatic compass is invaluable, both afloat and ashore. Take carefully temperature, current, tidal observations, a multitude of soundings, and keep specimens of all. Fill a private log-book with the most trivial and infantile details. You will afterwards laugh at much you have noted; but it is a great gain, and, unlike partridges, impressions are best fresh.

* For Mahommedan Calendar inquire of a Moslem, or such an authority as Capt. R. J. Burton, the famous Hajji Et-Iraki, and Consul to El-*Sham*.

This is not the occasion to go into matters of outfit. One thing I must name, on no account let any man on board be without a life-belt for his own use.

Any intending dredger writing to me at this club will be cordially answered. A small squadron of yachts working together under a commodore of their own election would partition the labour, and produce a little emulation among the crews. Make a rendezvous every few days, and talk results over.

MARSHALL HALL

New University Club, St. James's Street, S.W., Jan. 6

Anacharis Canadensis (A. Alsinastrum)

I SHOULD esteem it as a favour if you would allow me to ask, through the medium of NATURE, if there be any published account of observations, confirmatory or otherwise, of Mr. Wenham's notes on the free-cell formation which he has described as being carried on at the terminal growing point of *Anacharis*, quoted by Dr. Carpenter in "The Microscope and its Revelations," p. 405, *et seq.* (3rd ed.) H. POCKLINGTON

FIGHT BETWEEN A COBRA AND A MONGOOSE *

THE snake was a large cobra 4 ft. $10\frac{1}{2}$ in. in length, the most formidable cobra I have seen. He was turned into an enclosed outer room, or verandah, about 20ft. by 12 ft., and at once coiled himself up, with head erect, about ten or twelve inches from the ground, and began to hiss loudly. The mongoose was a small one of its kind, very tame and quiet, but exceedingly active.

When the mongoose was put into the rectangle, it seemed scarcely to notice the cobra; but the latter, on the contrary, appeared at once to recognise its enemy. It became excited, and no longer seemed to pay any attention to the bystanders, but kept constantly looking at the mongoose. The mongoose began to go round and round the enclosure, occasionally venturing up to the cobra, apparently quite unconcerned.

Some eggs being laid on the ground, it rolled them near the cobra, and began to suck them. Occasionally it left the eggs, and went up to the cobra, within an inch of its neck, as the latter reared up; but when the cobra struck out, the mongoose was away with extraordinary activity.

At length the mongoose began to bite the cobra's tail, and it looked as if the fight would commence in earnest. Neither, however, seemed anxious for close quarters, so the enclosure was narrowed.

The mongoose then began to give the cobra some very severe bites; but the cobra after some fencing forced the mongoose into a corner, and struck it with full strength on the upper part of the hind leg. We were sorry for the mongoose, as but for the enclosure it would have escaped. It was clear that on open ground the cobra could not have bitten it at all; while it was the policy of the mongoose to exhaust the cobra before making a close attack. The bite of the cobra evidently caused the mongoose great pain, for it repeatedly stretched out its leg, and shook it, as if painful, for some minutes. The cobra seemed exhausted by its efforts, and putting down its head, tried hard to escape, and kept itself in a corner. The mongoose then went up to it and drew it out, by snapping at its tail, and when it was out, began to bite its body, while the cobra kept turning round and round, striking desperately at the mongoose, but in vain.

When this had continued for some time, the mongoose came at length right in front of the cobra, and after some dodging and fencing, when the cobra was in the act of striking, or rather, ready to strike out, the mongoose, to the surprise of all, made a sudden spring at the cobra, and bit it in the inside of the upper jaw, about the fang, and instantly jumped back again. Blood flowed in large drops from the mouth of the cobra, and it seemed much

* The following interesting narrative has been obligingly forwarded to us by Prof. Andrews, of Queen's College, Belfast.

weakened. It was easy now to see how the fight would end, as the mongoose became more eager for the struggle. It continued to bite the body of the cobra, going round it as before, and soon came again in front, and bit it a second time in the upper jaw, when more blood flowed. This continued for some time, until at last, the cobra being very weak, the mongoose caught its upper jaw firmly, and holding down its head, began to crunch it. The cobra, however, being a very strong one, often got up again, and tried feebly to strike the mongoose; but the latter now bit its head and body as it pleased; and when the cobra became motionless and dead, the mongoose left it, and ran to the jungle.

The natives said that the mongoose went to the jungle to eat some leaves to cure itself. We did not wish to prevent it, and we expected it would die, as it was severely bitten.

In the evening, some hours after the fight, it returned, apparently quite well, and is now as well as ever. It follows either that the bite of a cobra is not fatal to a mongoose, or that a mongoose manages somehow to cure itself. I am not disposed to put aside altogether what so many intelligent natives positively assert.

This fight shows at any rate how these active little animals manage to kill poisonous snakes. On open ground a snake cannot strike them, whereas they can bite the body and tail of a snake, and wear it out before coming to close quarters. This mongoose did not seem to fear the cobra at all; whereas the cobra was evidently in great fear from the moment it saw the mongoose.

Ratnapura, Ceylon, April 11, 1871 R. REID

AUSTRALIAN PREPARATIONS FOR OBSERVING THE SOLAR ECLIPSE

THE following letter has been received at the office of the English Government Eclipse Expedition, from the Government Astronomer at Melbourne:—

"Melbourne Observatory, Nov. 4, 1871.

"My dear Sir,—The Eclipse instruments, copies of instructions, and your letter, reached me safely. Some of the instruments slightly damaged however, though not serious. About half the collodion bottles broken.

"The organisation of the Expedition is not yet quite complete; but a start, I think, is now certain. About 1,000*l*. has been contributed by various Australian colonies:—Victoria, 450*l*.; New South Wales, 300*l*.; South Australia, 100*l*.; Queensland, 100*l*.; and we expect to get 50*l*. from Tasmania. The cost of steamer, &c., will be from 1,400*l*. to 1,500*l*. Twelve or fourteen amateurs have joined, paying 30*l*. each for passage. The voyage will occupy about four weeks, including a week or ten days at Cape Sidmouth. The country at Sidmouth is quite unknown, and inhabited only by Aboriginals, who, although not very warlike, are often exceedingly troublesome. Little is known of facilities for landing, &c., but as there are several coral islands in the vicinity, it is possible we may select some of them for observing stations, as they can easily be reached by laden boats. The whole of the coral sea inside the barrier reef is nearly always smooth water, so there cannot be much surf to contend with. The Expedition will have to start from here about the 20th instant.

"Now, about our equipment:—First, we have Grubb's integrating spectroscope, which, by-the-bye, was considerably damaged; it had got adrift from its packing, and had evidently made sundry excursions of its own inside its case. Our instrument maker has set this right, and it is now in good working order, and I tried it with the hydrogen spectrum yesterday, and it performs satisfactorily.

"Second, the large field analysing spectroscope came out all right, only one reflector of the kind indicated available, and that altazimuth mounting, Browning 8-inch. We can hear of no others. We are busy making equatorial mounting for this, but I am afraid we shall have no time to apply clock-work. One five-inch equatorial with its clock-work will be devoted to photographs, for this purpose the telescope will be dismounted and camera substituted, as no good can be done with both.

"Third, Photography. We shall have to confine ourselves to the operations with the camera as indicated in instructions, and we are doing all we can to ensure good results.

"Fourth. Polariscope work.—The two polarimeters came all right. Prof. Wilson, of our University, has offered to take charge of polariscope observations; his experience in experimenting on polarised light will ensure this part being thoroughly done if clouds permit.

"I think we thoroughly understand all the instruments and the instructions, and intend to take up such observations which appear from the latter to be most desirable, and for which we have instrumental means.

"We have sets of Kirchhoff's and Angström's maps here, we shall have several hand spectroscopes, opera-glasses, &c., provided for general observations.

"The little tube with the compound spectra of Mg, Ba, &c., appears to require Leyden jar and coil and a strong current, even then I am doubtful if it can be used.

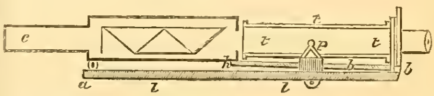
"We take up one or two field instruments to determine position, &c.

"The observing party of Melbourne will number about nine, that from Sydney about six. We can at best only form two observing stations, and those not many miles removed from one another. Sydney observers, under Mr. Russell, will be engaged principally in photographs with refractor and spectrum work (analysing), and possibly we shall be able to arrange some polariscopes for them.

"I shall send you the earliest possible information of our success or otherwise on our return, which will be about Christmas.

"Our chances of fine weather are somewhat doubtful, as the cloudy N.W. monsoon generally sets in about the middle of December; it appears, however, that this seldom fairly sets in till after Christmas, and as the eclipse takes place on the 12th, we have some reason to hope for success.

"We are trying to get a recording spectroscope ready, but I am afraid there is scarcely time to finish it. The small telescope has a loose tube around it, covered with paper. The eye-piece and pointer slip across the field, and are made to do so by a long lever, moved by a pricking frame.



ttt is a loose tube forming recording barrel, *bb* is attached to eye-piece by flat spring, *ll* long lever pointed at *a*, *h* slide bar parallel to telescope, *p* pricker frame which slides along *bb* crossing eye-piece and pointer to traverse field (the lever and slide bar are drawn too parallel, they should be more inclined to one another). By moving *p* up and down the slide bar the pointer is made to coincide with a line, and the pricker *p* is pressed—after pricking one set the loose tube is slightly revolved, and a second set obtained. It is nearly complete, but has not been tried yet. I hope you will have good success in India.

"At Cape Sidmouth we shall have 3m. 34s. totality, the sun at an altitude of about 45°, a more convenient position than I thought before the data were computed. Like you, we are working almost night and day to get ready, for it was only a fortnight ago I had authority from Government to organise a party and prepare instruments. Again wishing you the best success,

(Signed) "ROB. L. J. ELLERY

"J. NORMAN LOCKYER, ESQ."

ELECTROPHYSIOLOGICA:

SHOWING HOW ELECTRICITY MAY DO MUCH OF WHAT IS COMMONLY BELIEVED TO BE THE SPECIAL WORK OF A VITAL PRINCIPLE

II.

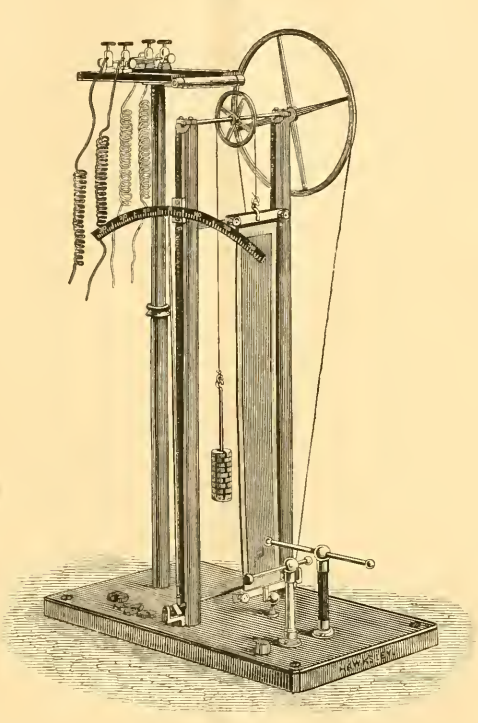
2. *How Electricity may do much of what is commonly believed to be the work of a vital principle in muscular action.*

I HAVE long held that a vital property of "irritability," or "tonicity," was unnecessary in muscular action. As it seemed to me, the state of relaxation in living muscle was to be accounted for by the mutual repulsion of molecules arising from the presence in the muscle at the time of a charge of electricity, sometimes positive, sometimes negative; as it seemed to me, muscular contraction, whether in ordinary muscular action or in rigor mortis, was nothing more than the result of the operation of the elasticity of the muscle upon the discharge, sudden or gradual, of the charge which had previously kept up the state of relaxation. And I still hold that the state of relaxation is caused by the presence in the muscle of a charge of electricity, and that muscular contraction is brought about by the elasticity of the muscle coming into play upon the discharge of this charge; but, since I began to work with the new Quadrant Electrometer of Sir Wm. Thomson, I have been obliged to take a different view of the way in which the charge operates in causing relaxation. The fact, discovered by means of this instrument, that there are two charges of electricity in muscle, positive *and* negative, was fatal to the idea that the state of relaxation was due to the mutual repulsion of molecules consequent upon the presence in muscle of a single charge, positive *or* negative. With either charge singly the idea might be entertained, though it was not easy to understand how, wanting effectual insulation, the electricity could be kept to its work; with two opposite charges, on the contrary, the attraction of each charge for the other *must* neutralise the repulsion arising from the presence of either singly. Nor did I find a way of escape from this difficulty until I began to seek it in a totally different direction, even in the theory according to which the sheath of muscular fibre during rest is charged as a leyden-jar is charged. Is it possible, I asked myself, that the two opposite charges, disposed leyden-jar-wise upon the two surfaces of the sheath, may cause elongation of the fibre by compressing between them the elastic sheath? Opposite charges of electricity *must* attract each other; that was plain enough. Opposite charges attracting each other across an elastic sheath *may* compress that sheath in such a way as to cause elongation of the fibre; that was not impossible. Upon this view, too, there was no difficulty in understanding how each charge was prevented from escaping, and made to work in this manner, by the mutual attraction of each for the other. In a word, the idea that the two charges might act in this way in causing muscular relaxation was far more easy to realise than that which regarded the state of relaxation as the result of the muscular molecules being kept in a state of mutual repulsion by the presence of one charge in the muscle. And so it was that it became necessary to look into this matter a little more closely—to put it to the test of experiment, as best I could.

In order to this, I began by inquiring whether the idea in question was possible or not. I wanted to be certain that the mutual attraction of two charges of electricity, dispersed leyden-jar-wise upon the two surfaces of the sheath of the fibre, would cause elongation, and that the discharge of this charge would be followed by contraction; and, after several abortive attempts, I found what I wanted, and more than I expected at first, by the means which are represented in the accompanying figure.

Vulcanised india-rubber sheeting being at once elastic and dielectric, it occurred to me that this material was the very thing for putting to the test of experiment what I believed might happen in the elastic and dielectric sheath of muscular fibre. I therefore took a band of this sheeting, provided it with the conducting surfaces necessary for charging and discharging it as a Leyden-jar is charged and discharged, and had constructed an apparatus for showing whether or not the anticipated changes in length were produced by this charging and discharging. The band (which is to be regarded as the counterpart of a *strip* of the actual sheath of the muscular fibre) is 14 in. in length by 2 in. in breadth, the commercial number of the india-rubber sheeting being 30. The necessary conducting surfaces to allow of the charging and discharging are made by painting the band on each side with fluid dutch-metal, care being taken to leave at the edges a sufficient unpainted margin to secure the necessary insulation of the two painted surfaces. The frame-work of the apparatus consists of two strong brass pillars, 18 in. in height, and 4 in. apart, rising from a flat brass stand. Across these pillars work two axles, horizontal in direction and parallel to each other—the one at the top, the other near the base, immediately above the stand. At the middle of the upper axle, midway between the pillars, is a wheel with a grooved edge, 2 in. in diameter, which may be called the driving-wheel; at one end, which projects beyond the pillar on that side, is another and larger wheel, 6 in. in diameter, also with a grooved edge, which may be called the multiplying-wheel. At one end of the lower axle, beyond the pillar on that side and immediately under the multiplying-wheel is a collar with a grooved edge; at the other end, also beyond the pillar on that side, is a socket for carrying a long index, of which the free end moves backwards or forwards before a graduated arc fixed immediately over the socket upon the same pillar near its top. The two axles move together, the upper telling upon the lower by means of an endless band which at one and the same time bites in the grooved edge of the multiplying-wheel at the end of the one, and in that of the collar at the end of the other; and thus the movements of the index before the graduated arc are made to represent a very considerable exaggeration of the movements of the upper axle. The india-rubber band is clipped at each end in a clamp, acting by screws, and having a hook on its free edge; and, being so clipped, it is fixed in a a vertical position by passing the hook on the clamp at its lower end into a socket provided for it on the stand, and by attaching the hook on the clamp at its upper end to a string which passes over the grooved edge of the driving-wheel to a short hanging rod with a button at its lower end, upon which rod are to be slipped coin-like weights, notched in the centre for this purpose, which weights have to be so adjusted as to put the band gently upon the stretch. In this way the band is so fixed that it cannot lengthen or shorten without these changes being made to tell upon the index, for as it lengthens or shortens, the driving-wheel which moves the index must be made to turn this way or that by the string which bites into its grooved rim in passing from the band to the weights. For charging and discharging, two short pillars are fixed to the stand in front of and at a short distance from the bottom of the band, that for the former purpose having an ebonite shaft, that for the latter being altogether metal; and through holes in the caps of these pillars the rods which are intended to serve as the actual channels for the charge and discharge are made to slide horizontally backwards or forwards in a suitable direction. In charging, the electricity is supplied to the metallic surface on the front of the band by pushing forwards the charging rod so as to touch this surface, and at the same time taking care that the discharging rod is drawn back so as to leave the necessary break in the circuit. In discharging, the discharging rod is pushed home so as to complete the circuit between the two opposite metal coatings of the band by touching the centre of the charging rod. And for the rest, all that need now be said of the apparatus (this is not all that has to be said, but what remains has to do with a totally different set of experiments, and had better be reserved until the time comes for dealing with these experiments) is, that in order to allow of this charging and discharging, the metal surface at the back, instead of being insulated all round like the metal surface at the front of the band, is put in communication with the earth by bringing it down a little so as to allow it to be clipped by the metal clamp which fixes the band to the stand.

In the actual experiment with the band, all that has to be done is first to charge and then to discharge, watching the index the while. It was anticipated that the band would elongate with the charge, and shorten with the discharge, and this is what happens in fact; for on charging, the index at once moves before the graduated arc in the way which shows that the band elongates in proportion to the charge, and on discharging it suddenly jumps back again to the position it occupied before the charging, these forward and backward movements being through 40° or 60°, or even over a still wider range, and not merely through one or two degrees. The band plainly elongates in proportion to the charge. The band as plainly shortens in proportion to the discharge, suddenly or gradually, as the case may be, suddenly if the charge be augmented until it overleaps the barriers of insulation, or if the discharge be brought about by pushing home the discharging-rod, gradually if the band be charged and then left to discharge itself slowly by keeping back the discharging-rod. And these results are constant, provided only before charging and discharging the weights attached to the band are so adjusted as to balance without overbalancing the elasticity of the band—a matter which is easily managed with but little patience and practice.



All, in fact, that was anticipated is fully borne out by the experiment. And thus it may be taken for granted that elongation of the muscular fibre *may* be caused by the attraction of two opposite charges of electricity disposed leyden-jar-wise upon the two surfaces of the sheath of this fibre, and that contraction of this fibre *may* follow the discharge of these charges; for what is assumed to happen in this case is nothing more than what does actually happen with the band of india-rubber sheeting under perfectly analogous circumstances.

But if this be the way in which muscular fibre may be affected by its natural charge and discharge, how will it be affected by an artificial charge of the same kind? Will this artificial charge—the sheath being still a dielectric—act like the natural charge, the charge imparted to one side of the sheath inducing an equivalent amount of the opposite charge on the other side? Will the artificial charge, presuming it to be larger in amount than the natural charge, overrule the natural charge? Will the artificial charge, thus larger in amount than the natural, produce a greater degree of elongation in the muscular fibre than that which is natural to the fibre? Will the contraction following the discharge of this artificial charge be greater in amount than that which is natural to the fibre, because the elasticity of the muscle has freer play under these circumstances? These questions, and others also of a like nature, are suggested by the experiment upon the elastic band; for not only does the band elongate with the charge and shorten with the discharge, but the elongation and shortening are manifestly in proportion to the amount of the charge and discharge. Nor are these questions unanswerable. On the contrary, answers may be found in more ways than one—in the examination of the phenomena of electrotonus more particularly; and these answers are in no way ambiguous in their meaning.

In electrotonus are strange modifications of muscular action. In electrotonus, too, as I have shown elsewhere,* are strange modifications of the electric condition of the parts, there being everywhere in the region of anelectrotonus a positive charge overflowing from the positive pole of the battery employed in the production of electrotonus, there being everywhere in the region of cathelectrotonus a negative charge overflowing from the negative pole of the same battery. In anelectrotonus there is a positive charge, not only present, but at work; in cathelectrotonus there is a negative charge, not only present, but at work. At work certainly, for as I have shown, the movements of the needle of the galvanometer characteristic of electrotonus are caused by the movement, not of a voltaic current, but of these charges through the coil of the instrument, the movement of cathelectrotonus by the flow of the negative charge, that of anelectrotonus by the flow of the positive charge. At work also, as I have also shown, in modifying muscular action. At all events, the presence of a positive charge in anelectrotonus and of a negative charge in cathelectrotonus are facts, and therefore I am justified in looking to the phenomena of muscular action in the two electrotonic states with a view to find answers to the questions now under consideration.

At the onset of the inquiry, however, a grave difficulty has to be coped with—a difficulty as to facts, for the actual facts are not what they are believed to be. In a word, it is not true that the action of anelectrotonus upon muscular action is essentially different from that of cathelectrotonus. Differences there are no doubt, but not any that will prove to be of moment in the present place. It is a fact that muscular action is suspended, not in anelectrotonus only, but in cathelectrotonus as well as in anelectrotonus. It is a fact that muscular *elongation* is a phenomenon common to both electrotonic states. Nor are these the only points in the history of electrotonus which require to be looked into carefully. So that, before proceeding further in this matter, it is necessary to ascertain what are the facts which have here to be dealt with.

The true history of muscular action during electrotonus may be well seen in the gastrocnemius of a frog by means of certain experiments for the exhibition of which the apparatus already used in the experiment with the elastic band is furnished with certain parts which have yet to be described. These parts consist of a pillar and a platform resting upon it horizontally, the pillar rising from the side of the stand opposite to that occupied by the charging and discharging rods. The pillar has a telescope arrangement, by which its length may be altered, and a screw-collar, by which it may be fixed at any length. The platform consists of a four-sided metal floor, five inches in length by three in breadth, with a narrow and rather thick border of ebonite in which are two binding screws for holding electrodes upon each of its sides, with a long roller at one of its ends, and with a moveable gutta-percha cover of such a shape and size as to allow it to be slipped on and off between the ebonite borders, and fixed when on by having its edges made to play under the hollowed-out inner margins of the borders. In the actual experiment what has to be done is—to remove the elastic band and the weights attached to it—to fix the platform, so that it is a little behind and above the level of the driving-wheel, with the end to which the roller is attached turned towards this wheel—to fix the wires from the battery and induction apparatus to the binding-screws on the platform, the wires from the battery being carried to the side on which the screws are farthest from the roller (the battery, I should have said, consists of four medium-sized Bunsen-cells, and the induction-apparatus is one in which the secondary coil may be slipped altogether away from the primary—a Du Bois-Reymond's inductorium, in fact),—to prepare a frog's limb by stripping off the skin and dissecting away all parts of the thigh except the sciatic nerve,—to remove the gutta-percha cover from the platform, and pin upon it the prepared limb with its heel close to one end, care being taken not to injure the nerve or muscle in doing this,—to tie to the tendo-achillis the string which belongs to the weights,—to put back the gutta-percha cover into its place with the limb thus pinned and arranged upon it, the string attached to the tendo-achillis being brought out over the end which comes next to the roller,—to carry this string over the driving-wheel to the rod carrying the weights,—and to adjust these weights so as to put the gastrocnemius gently on the stretch,—and lastly, to draw out the nerve, and carry it first across the electrodes belonging to the induction-apparatus and then across those belonging to the battery, these electrodes, to allow of this, being made to point inwardly to a sufficient distance across the platform, two from one side, two from the other. In this way, when the circuits are closed (they are open at first) the nerve may be acted upon by voltaic and faradaic electricity as in an ordinary experiment in electrotonus. In this way, any change in the length of the gastrocnemius must tell upon the index, just as the changes in the length of the elastic band were made to tell, only in the contrary direction.

These arrangements being made, two experiments have to be tried, the one for exhibiting the action of anelectrotonus upon the gastrocnemius, the other for exhibiting that of cathelectrotonus, and each differing from the other only in the relative position of the voltaic poles, the positive pole being next to the insertion of the nerve into the muscle in analectrotonus, the negative pole being in this position in cathelectrotonus.

In the experiment for exhibiting the action of anelectrotonus upon the muscle—that with the positive pole in the position next to the insertion of the nerve into the muscle—there are three distinct steps, the first taken before setting up the state of anelectrotonus, the two others after this time.

The first step, or that which is taken before the establishment of anelectrotonus, is to tetanise the muscle with faradaic currents only just strong enough to act upon the muscle at all in this way. In this case the circuit of the induction-apparatus is closed, but not that of the voltaic battery, and therefore the nerve is acted upon by faradaic currents before the establishment of anelectrotonus. At first, the faradaic currents used are strong enough to tetanise the muscle effectually; then these currents are weakened by drawing away the secondary coil from the primary until the tetanus comes to an end; last of all, the tetanus is brought back again to the very slightest degree by moving the secondary coil back again towards the primary coil, and leaving it at the point where the currents produced in it just begin to have a tetanising action. This is the first step in the experiment.

The second step consists in the establishment of anelectronus while the nerve is still being acted upon by these feeble faradaic currents. Hitherto the circuit of the induction apparatus was closed, while that of the voltaic

* "Dynamics of Nerve and Muscle." Macmillan, 1870.

battery was left open. Now the latter circuit is also closed, and with this result—that the index gives a sudden great jump in the direction showing contraction, and then, immediately moving in the opposite direction to that signifying contraction, takes up a position on the other side of zero—at 15° or 20°, it may be—a movement showing, not contraction, therefore, but elongation. Eliminating, as non-essential, the strong contraction which happens at the closing of the circuit—for this has to do, not with anelectrotonus, but with the *extra-current* which traverses the nerve between the poles at the closing of the voltaic circuit—what happens, therefore, on the establishment of anelectrotonus is, first, *suspension of the tetanus* caused by the feeble faradaic currents; and, secondly, *elongation of muscle*. This is the second step of the experiment, and these the results.

The third step follows upon the second. Its object is to ascertain whether the tetanus may be made to return during anelectrotonus by slightly increasing the strength of the faradaic currents acting upon the nerve; and the way of arriving at this is to leave the voltaic circuit still closed, to go on moving the secondary coil of the induction apparatus nearer to the primary, and to stop the moment the faradaic currents acquire strength enough to call back any tetanus. And this is what happens—that after moving the secondary coil but a short distance towards the primary, the index shows, not only that the tetanus has reappeared, but that it has reappeared in greater force. Before the establishment of anelectrotonus, the tetanus caused by faradaic currents only just strong enough to tetanise the muscle carried the index to 20° or thereabouts; after the establishment, the tetanus caused by faradaic currents only just strong enough to exert a tetanising action moved the index to 40° or 60°. In a word, contraction may happen in anelectrotonus, and when it happens it is considerably increased in amount. This is the third step of the experiment, and this the result.

In the experiment for exhibiting the phenomena of cathelectrotonus—that in which the negative voltaic pole is placed next to the insertion of the nerve into the muscle—all the steps are the same, and so are the results. The setting up of cathelectrotonus suspends the tetanus caused by feeble faradaic currents, and causes elongation in the muscle. The tetanus brought back during the cathelectrotonus by currents only just strong enough to have a tetanising action is in increased force. The degree of elongation is the same as in anelectrotonus. The increase of contraction is the same as in anelectrotonus. The only difference, indeed, between the two experiments is this, that somewhat feebler faradaic currents serve to recall the tetanus in cathelectrotonus than those which were required to do this in anelectrotonus.

Nor are these facts at variance with those which are brought to light when the state of electrotonus is produced by a smaller amount of battery power—by a single element, for example. In this case it often happens (not always) that the tetanus caused by salt or very feeble faradaic currents is suspended by anelectrotonus, and intensified by cathelectrotonus. It seems as if there was an essential difference between this action of the two electrotonic states upon nerve and muscle, but after what has just been seen this is by no means a necessary conclusion. It has been seen that anelectrotonus has a greater power of suspending tetanus than cathelectrotonus, therefore tetanus may be suspended by anelectrotonus when it is not suspended by cathelectrotonus. It has been seen that during both anelectrotonus and cathelectrotonus contraction when it happens is greater than that which happens in the non-electrotonised state; and therefore, during cathelectrotonus, if tetanus be not suspended, it is likely to be intensified. This is all. The facts are in keeping with those which have gone before when they are properly looked into, and there is no ground in them for supposing that there is an essential difference between the action of anelectrotonus and cathelectrotonus—no ground for supposing that the effects of using a small battery power in the production of electrotonus are in any way different from those which attend the use of a larger power of this kind.

C. B. RADCLIFFE

CONJOINT MEDICAL EXAMINATIONS*

WE are able to open the new year with the satisfactory announcement that the last difficulty has been removed which impeded the action of the great medical examining incorporations of England in uniting to frame a conjoint scheme for a minimum examination, which will constitute, in fact, a single and uniform portal to the profession. All the committees of the bodies concerned have signified their approval of the following scheme:—

In view of the legal difficulties which have been stated by the Society of Apothecaries to prevent that society taking part in the formation of an examining board in this division of the United Kingdom, it was resolved:

I. That a board of examiners be appointed in this division of the United Kingdom by the co-operation of the Royal College of Physicians of London, the Royal College of Surgeons of England, and of such other of the medical authorities in England, mentioned in Schedule (A) to the Medical Act, as may take part in its formation; it being understood that, liberty being left to such co-operating medical authorities to confer, as they think proper, their honorary distinctions and degrees, each of them will abstain from the exercise of its independent privilege of giving admission to the "Medical Register."

II. That the Board be constituted of examiners, or of examiners and assessors appointed by the several co-operating medical authorities.

III. That examiners be appointed on the following subjects: Anatomy and physiology; chemistry; materia medica, medical botany, and pharmacy; forensic medicine; surgery; medicine; midwifery; or on such subjects as may be hereafter required.

IV. That no examiner hold office more than five successive years, and that no examiner who has continued in office for that period be eligible for re-election until after the expiration of one year.

V. That the examiners be appointed annually by the several co-operating medical authorities on the nomination of a committee, called herein "The Committee of Reference;" but no member of the Committee of Reference shall be eligible for nomination as an examiner.

VI. That a Committee of Reference, to consist of an equal number of representatives of medicine and surgery, be appointed as follows: One representative of medicine and one representative of surgery to be appointed by each of the Universities in England; four representatives of medicine to be appointed by the Royal College of Physicians of London; four representatives of surgery to be appointed by the Royal College of Surgeons of England.

VII. That one-fourth of the Committee of Reference go out of office annually, and that, after the first four years, no retiring member be re-eligible until after the expiration of one year.

VIII. That the duties of the Committee of Reference be generally as follows: 1. To determine the number of examiners to be assigned to each subject of examination. 2. To nominate the examiners for appointment by the several co-operating medical authorities. 3. To arrange and superintend all matters relating to the examinations, in accordance with regulations approved by the co-operating medical authorities. 4. To consider such questions in relation to the examinations as they may think fit, or such as shall be referred to them by any of the co-ope-

* Reprinted from the *British Medical Journal*.

rating medical authorities, and to report their proceedings to all the said authorities.

IX. That there be two or more examinations on professional subjects, and that the fees of candidates be not less than thirty guineas to be paid in two or more payments.

X. That every matriculated student of an English university who shall have completed the curriculum of study required by his university, and shall have passed such an examination, or examinations, at his university as shall comprise the subjects of the primary examination, or examinations, conducted by the Board, be eligible for admission to the final examination; and that every candidate so admissible to examination be required to pay a fee of five guineas, but he shall not be thereby entitled to the license of the Royal College of Physicians of London, nor to the diploma of member of the Royal College of Surgeons of England, without the payment of an additional fee of not less than twenty-five guineas.

XI. That every candidate who shall have passed the final examination conducted by the Board shall, subject to the by-laws of each licensing body, be entitled to receive the license of the Royal College of Physician of London, and the diploma of member of the Royal College of Surgeons of England.

This is signed by George Burrows, President of the Royal College of Physicians, and George Busk, President of the Royal College of Surgeons.

Sir Roundell Palmer, Mr. Denman, and Mr. Bevis have given their opinion that this scheme can be legally carried into effect by means of by-laws to be adopted by the respective Colleges of Physicians and Surgeons. This opinion was presented at the meeting of the Joint Committee on the 3rd inst. The examiners in surgery will be chosen from among the examiners who have been appointed under the charters of the College of Surgeons, and the Court of Examiners will adopt the certificate of the new examining body.

Meetings are being held in Dublin with a view to the formation of a conjoint examining board for Ireland. So far, no insurmountable difficulty has arisen in the several matters which have come under the notice of the deputed representatives of the Universities and of the other licensing bodies, and it is hoped that the board, as proposed, will become an accomplished fact. A claim was put forward by the Universities that the first part of the professional examination conducted by the conjoint board should not be required of university students who had passed their examination on the same subjects; and that in their case the examination should be confined to the final one. To this, however, the other licensing bodies properly objected; but an offer has been made by the other corporations that the preliminary examination should be wholly conducted by examiners appointed by the Universities.

NOTES

The celebrated ethnological collection of the late Dr. Gustavus Klemm, of Dresden, which had obtained a world-wide celebrity from its richness in illustrations of dress and ornaments, household utensils, furniture, warlike, fishing, and hunting implements, &c., extending from the earliest times down to the immediate present, has been purchased by subscription, and transferred to Leipsic, where it forms the nucleus of the new German Central Museum of Ethnology, and around which is to be grouped whatever additional material can be procured in illustration of the general plan. An earnest appeal is made by the officers and others interested in this enterprise to their countrymen and others in the United States for contributions. It will occupy the place in Germany of the great Archæological Museum of Copenhagen: of that of Mr. Blackmore at Salisbury, in England; of the Museum of St. Germain, near Paris, under direction of M. Mortillet; and of the Smithsonian and Peabody Museums in the United States.

The Exhibition of Neolithic Instruments by the Society of Antiquaries at Somerset House will be re-opened to-morrow, and will finally close on Thursday, January 18. For tickets apply at the Society's apartments.

On Saturday last, at an early hour in the morning, the female hippopotamus in the Zoological Society's gardens gave birth to a young one—being the second occasion on which this interesting occurrence has taken place. As in the former case, it has been found necessary to close the building in which the female is placed entirely, not even the keepers entering into it except when absolutely necessary, in consequence of the extreme savageness and jealousy exhibited by the fond mother. Some days must therefore elapse before the "little stranger" can be prepared to undergo the ordeal of public exhibition.

Another interesting addition just made to the Zoological Society's collection is a young specimen of the King Penguin (*Apterodytes pennanti*) from the Falkland Islands. For this remarkable bird, which is still in the down-plumage, the Society are indebted to the kind exertions of F. E. Cobb, Manager of the Falkland Islands Company, who has been for some time endeavouring to obtain living examples of this species for the Society. The King Penguin is placed in the great eastern aviary, along with a specimen of the Cape Penguin (*Spheniscus demersus*) which has been for some time under the Society's care.

We have just received the fourth report of the Radcliffe Trustees from the Radcliffe Librarian, Dr. Henry W. Acland, including also a catalogue of the transactions of societies, periodicals, and memoirs, available for the use of professors and of students in the Library; a catalogue of books recommended to students in physical science by the museum professors; and the Regulations of the Library. The additions to the Library are made, as far as the annual grant of 500*l.* will allow, either on the judgment of the librarian as to the intrinsic value of a work, or on the advice of a professor, or upon the knowledge that students require it.

It is stated that the average yearly number of visitors at the South Kensington Museum during the last five years has been 905,084.

The University Court of the University of Edinburgh, at a meeting held on Tuesday, Jan. 2, declined to give effect to the recommendation of the Senatus, that the regulations in reference to the medical education of women should be rescinded. The Court guarded itself against being understood to indicate any opinion as to the claim of women to proceed to graduation, or as to the powers of the University to confer on women degrees in the faculty of medicine.

The Second Course of Cantor Lectures of the Society of Arts for the session will be delivered by the Rev. Arthur Rigg, M.A., on "Mechanism." The first lecture will be given on Monday evening, Feb. 5, at eight o'clock, and the remainder of the course will follow on the five succeeding Monday evenings.

At the annual meeting of the Birkbeck Literary and Scientific Institution, Sir John Pakington, M.P., in the chair, it was stated that, during the past year, the following new subjects have been introduced into the curriculum of the Institution:—Acoustics, Light and Heat, Practical Chemistry, Mineralogy, Metallurgy, and the Theory of Music.

The authorities of the American Museum of Natural History, at the Central Park in New York, have set apart Monday and Tuesday especially for the use of those persons who may desire to examine the specimens in the Museum for the purpose of special study. Notifications of this arrangement have been distributed to the principal learned societies throughout the country, inviting them to attend on these days.

De La Rue's indelible diaries for 1872 are as usual beautifully printed and bound, with ample room for memoranda. We miss the astronomical article, but still the letter-press being curtailed is an advantage, the book being less weighty for the pocket. The desk diary is a most useful appendage to the writing table, containing, besides the almanack, tables, &c., extra pages for memoranda and accounts.

The eighth Annual Report is issued of the Belfast Naturalists' Field Club for 1870-71. The papers of which abstracts are printed in the Report are of varied interest, the subjects comprised including—"The Geographical Distribution of Cyclones," "The Latest Fluctuations of the Sea Level on our own Coasts," "Ocean Currents and their Effect on Climate," "Report of a Committee appointed to examine some Ancient Remains in the neighbourhood of Armoy, county Antrim," and numerous others. A number of prizes are offered to be competed for during the session ending March 31, for the best herbarium, collections of fossils, recent Crustacea and Echinodermata, shells, insects, sponges, &c., and others.

Prof. Halford has received from Simla the thanks of the Government of India for his paper on "The Treatment of Snake-bite by the Injection of Liquor Ammoniæ into the Veins." The Governor-General in Council has determined to have Dr. Halford's pamphlet reprinted for general distribution to medical officers in different parts of India. It appears to be placed beyond doubt that this treatment is by far the most efficacious yet discovered in cases of poisonous snake-bite.

Condurango root, the reputed specific for cancer, is becoming a subject of speculation in Ecuador and the United States. In Ecuador it has reached 17*l.* a ton, but in New York it has been selling for fabulous prices, though its virtues are contested. The Government of Ecuador has imposed an export duty. The Condurango root is now reported to have been discovered by Mr. Simmons in the neighbourhood of Santa Marta in Colombia or New Granada, and a small shipment has been made to the United States. It is not stated whether it has been tried for cancer in that country.

The Chilian Government has sent the war steamer *Chacabuco* to survey the islands of Guaiatecas.

The U.S. Government has directed a survey of the Bay of Limou, the Atlantic terminus of the new Costa Rica Railway, where a city is being laid out with a pier.

Anthracite coal has been discovered in the district of San Miguel, five miles from the capital of Costa Rica in Central America. There are several seams of about 40 miles wide, and the coal has been proved to be of good quality. A railway is in progress in the neighbourhood. It may be remembered that coal is also found in the State of Panama.

It is noted as remarkable that a spring of fresh water has been discovered near Mollendo in Peru.

The pearl oysters are said to have disappeared this season from the Madras coast, as well as from that of Ceylon.

M. Bertillon lately read before the Academy of Medicine in Paris a paper on the relative influence of marriage and celibacy, based on statistical returns derived from France, Belgium, and Holland. In France, taking the ten years 1857-66, he found that, in 1,000 persons aged from 25 to 30, 4 deaths occurred in the married, 10·4 in the unmarried, and 22 in widowers; in females at the same age, the mortality among the married and unmarried was the same—9 per 1,000, while in widows it was 17. In persons aged from 30 to 35, the mortality among men was, for the married, 11 per 1,000, for the unmarried, 5, and for widowers, 19 per 1,000; among women, for the married, 5, for the unmarried, 10, and for widows, 15 per 1,000. There appears to be a general agreement of these results of marriage in Belgium and Holland, as well as in France.

We are so accustomed to associate tattooing almost entirely with the natives of New Zealand and the Indians of North America, that it comes to us almost as a new fact to learn from a correspondent of the *Field* what a high standard the art of tattooing has reached among the Japanese. There we find men who make it their business to tattoo others, and these "professors of tattooing" are artists of no mean power, "for no india-rubber or ink-eraser can possibly take out a false line once imprinted; and they most invariably in the 'printing in' improve upon the drawings previously made." The bettoes or Japanese grooms will frequently have depicted on their skins, not only perfectly-drawn pictures of birds, reptiles, beasts and fishes, but also representations of whole scenes, often from some old legend or history. A very common device is the red-headed crane, the sacred bird of Japan, depicted standing on the back of a tortoise, and this is emblematic of woman's beauty treading down man's strength. These designs are pricked in by needles, and two or three colours are used.

Prof. Kengott, of Zurich, states that a hail-storm lasting five minutes occurred at eleven o'clock in the morning of August 20, 1871, the stones from which were found to possess a salty taste. Some of them weighed twelve grains. They were found to consist essentially of true salt, such as occurs in Northern Africa on the surface of the plains, mainly in hexahedric crystals or their fragments, of a white colour, with partly sharp and partly rounded grains and edges. None of the crystals were entirely perfect, but appeared as if they had been roughly developed on some surface. They had probably been taken up and brought over the Mediterranean from some part of Africa, just as sand is occasionally transported thence to the European continent and the Canaries by means of hurricanes. A still more remarkable phenomenon has been recently recorded by Prof. Eversmann, of Kasan—namely, the occurrence of hailstones, each containing a small crystal of sulphuret of iron. These crystals were probably weathered from some rocks in large quantity, and were then taken up from the surface of the ground by a storm, and when carried into the hail-forming clouds served as a nucleus for the formation of hailstones.

A practical extension of the metalliferous region of Chile to the south is announced in the discovery of rich silver deposits in the southern province of Nuble. The place is called Cuesta del Caracol, and is between the Rivers Lota and Nuble, about fifty miles from San Carlos towards the east. The standard on assay is estimated at 100lb. of silver to the ton. Operations are already prepared on a large scale. The Lota district has hitherto only been known for its large trade in coal and fire-bricks.

The Indian Government has taken measures for a survey of the Tenasserim tin mines and their present state of production, for which purpose it has despatched Mr. Mark Fryar, mining engineer, to that province.

In the native State of Kolapore in the Bombay Presidency sheep suffered from a strange form of animal plague. This consists of a swarm of unusually voracious leeches. Besides this the wolves were out, carrying off children, invalids, and the aged in the exposed villages.

A white elephant having been discovered in our possessions in Tavoy, on the Malay Coast, the Buddhist sovereigns are extremely anxious to obtain such an important minister of religion.

The King of Burmah has made special application to be favoured with the holy beast.

AN earthquake took place at Valparaiso in the early part of November (date not given) at 10.5 P.M. The shock was smart, and apparently from E. to W. There was another slighter shock shortly after midnight.

ON Oct. 10 an earthquake was felt at Salvador, also in Central America, at 8.27 A.M. It was slight. Another was felt on the 12th, at 11.36 P.M., lasting nineteen seconds, with a strong shock. After the 12th were two slight shocks, it is to be supposed conforming with those of Nicaragua.

ON Sept. 25 an earthquake was felt at Carrizal Bajo, in North Chile, at 4.3 P.M., preceded by a loud noise.

THREE islands have lately been surveyed by the United States Government in the North Pacific Ocean. They are Ocean Island, in latitude 28° 25′ N., longitude 178° 25′ W. Midway Island, or Brooks Island, in latitude 28° 15′ N., longitude 178° 20′ W.; Pearl and Hermes Islands, latitude 27° 50′ N., and longitude 175° 50′ N. They are all three coral islands, and abound in turtle, and birds were found in great numbers. There is but little guano and not much vegetation.

IN connection with the bad weather in November in the Bay of Bengal, the telegraph lines were on the 10th affected by earth-lines on the east coast of India. At Madras these currents were first noticed at 6 A.M., they abated at 4 P.M., and were strongest in the lines forming a considerable angle with the magnetic meridian. They were also observed in the Madras cable. In Calcutta the currents were noticed at about 3 A.M. and ceased at 2 P.M.

*ANCIENT ROCK INSCRIPTIONS IN OHIO**

SEVERAL diagrams were presented to the section representing rock sculptures in Ohio, that are presumed to be ancient and to have some significance. The largest is a tracing made by Dr. J. H. Salisbury, of Cleveland, with the assistance of Mrs. Salisbury, from a mural face of conglomerate, near the famous "Black Hand" in Licking County, Ohio. Once there was a space of ten or twelve feet in height, by fifty or sixty feet in length, covered by these inscriptions. Most of them have been obliterated by the recent white settlers.

In 1861, Dr. Salisbury took copies from a space about eight by fifteen feet, by laying a piece of coarse muslin over them, and tracing such as remain uninjured, life size, on the cloth. In this space there are found to be twenty-three characters, most of which are the arrow-head or bird-track character. These are all cut on the edge of the strata, presenting a face nearly vertical, but a little shelving outward, so as to be sheltered from the weather.

Another copy of the remnants of similar inscriptions was taken by Colonel Whittlesey and Mr. J. B. Comstock, in 1869, from the "Turkey Foot Rock," at the rapids of the Maumee, near Perrysburg. These are on a block of limestone, and in the course of the twenty-five past years have been nearly destroyed by the hand of man. What is left was taken by a tracing of the size of nature.

On the surface of a quarry of grindstone grit at Independence, Cuyahoga County, Ohio, a large inscribed surface was uncovered in 1854. Mr. B. Wood, Deacon Bicknell, and other citizens of Independence, secured a block about six feet by four, and built it into the north wall of a stone church they were then building. Colonel Whittlesey presented a reduced sketch, one-fourth size of nature, taken by Dr. Salisbury and Dr. J. M. Lewis, in 1869, which was made perfect by the assistance of a photographer. Some of the figures sculptured on this slab are cut an inch to an inch-and-a-half in the rock, and they were covered by soil a foot to eighteen inches in thickness, on which large trees were growing. Like all of the others, they were made by a sharp-pointed tool like a pick, but as yet no such tool has been found among the relics of the mound-builders or of the Indians. The figures are very curious. Among them is something like a trident, or fish-spear, a serpent, a human hand, and a number of track-like figures, which the people call buffalo-tracks, but which Dr. Salisbury regards as a closer representation of a human foot covered by a shoe-pack or moccasin. Another figure somewhat resembles the section of a bell with its clapper.

Near the west line of Belmont County, Ohio, Mr. James W. Ward, then of Cincinnati, now of New York, in 1859 took a sketch of two large isolated sandstone rocks, on which are groups of figures similar to those already noticed. Here are the bird-track characters, the serpent, the moccasin or buffalo-tracks, and some anomalous figures. These are plainly cut, with a pick, into the surface of the rock, which, like the Independence stone, is substantially imperishable. Here we have also the representation of the human foot, and the foot of a bear. Another figure, which appears to be the foot of some animal with four clumpy toes, Prof. Cope thinks may be the foretrack of a Menopome. One peculiarity of these sculptured human feet is a monstrously enlarged great-toe joint, even greater than is produced by the modern process of shoe-pinching. This has been observed in other ancient carvings of the human foot upon the rocks near St. Louis, Missouri. These feet range in size from seven to fifteen inches in length. Of all these representations, the bear's foot is closest to nature. The bird-track, so called, presents six varieties, some of which are anatomically correct. The human hand is more perfect than the foot.

Dr. Salisbury finds, on comparison of these symbolical figures with the Oriental sign-writing, or hieroglyphical alphabets, that there are many characters in common. Some 800 years before Christ, the Chinese had a bird-track character in their syllable alphabet. The serpent is a symbol so common among the early nations, and has a significance so various, that very little use can be made of it in the comparison.

These inscriptions differ materially from those made by the modern red man. He is unable to read that class of them which appears to be ancient.

Lieut. Whipple has mentioned in the "Government Report of the Pacific Railroad Surveys," an instance of the bird-track character inscribed upon the rocks of Arizona. Prof. Kerr, of North Carolina, states that he has noticed similar characters cut in the rocks of one of the passes of the Black Mountains, at the head of the Tennessee river.

These facts indicate wide-spread universality in the use of this style of inscription, and they indicate something higher than the present symbolical or picture writing of the North American Indians.

* Paper read before the American Association for the Advancement of Science, Section of Archæology, by C. Whittlesey. Reprinted from the *American Naturalist.*

SCIENTIFIC SERIALS

Monthly Microscopical Journal, January.—"The markings on the Battledore scales of some of the Lepidoptera." By John Anthony, M.D., &c. In this paper the author contributes the result of his observations on the plumules of *Polyommatus Alexis*, from which he is led to the conclusion that the markings on the ribs of the scales are elevations, very much resembling in shape the vegetable glands on the petal of Anagallis, that is, the elevations have a base, a column, and a rounded head, or capital; the form being very much like that of an ordinary collar-stud. The methods employed during observation are detailed in the paper, which is illustrated by two plates.—"The Nerves of Capillary Vessels and their probable action in health and disease" By Dr. Lionel S. Beale, F.R.S., This paper is divided into two parts, the anatomical investigation, and probable mode of action. The first part, containing the results of anatomical investigation alone, is published in the current number of the journal. The sections of this paper are, "Structure of Capillaries," "Nuclei or Masses of Bioplasm of Capillaries," "Nerve Fibres," "Arrangement of the Nerve Fibres distributed to the Capillaries," "Central Origin and Peripheral Connections of Nerve Fibres distributed to the Capillaries," and the "Method of Demonstration." Such an important contribution to microscopic anatomy could not be abstracted within the limits of this notice with justice to the author and his subject. We therefore commend it to the notice of all interested therein, with the assurance that they will find much matter for reflection.—On a New Micrometric Goniometer Eye-piece for the Microscope. By J. P. Southworth. The eye-piece micrometer here described is obtained by photographic reduction from heavy India-ink lines drawn on a white Bristol board. In the micrometer the lines are $\frac{1}{200}$ of an inch apart, and jet black, whilst the spaces between them are translucent enough to admit of the accurate measurement of the details of minute algæ and fungi to the $\frac{1}{25000}$ of an inch. The goniometer is also described. Both are said to posses advantages not secured before by any instrument. The remaining papers are—Note on Dr. Barnard's Remarks on the Examination of Nobert's Nineteenth Band, by J. J. Woodward, Assist. Surg. U.S. Army; a New Erecting Arrangement, especially designed for use with binocular microscopes, by R. H. Ward, M.D.,; and On the Action of Hydrofluoric Acid on Glass, viewed Microscopically, by H. F. Smith.

OF the *Mémoires de la Société de Physique et d'Histoire Naturelle de Genève* the first part of the twenty-first volume has recently been published. It is chiefly occupied by an admirable memoir on the Orthopterous family Mantidæ by M. Henri de Saussure, forming the third fascicule of his "Mélanges Orthoptérologiques." In this paper the author not only describes a great number of new species, but also discusses the internal classification of the family, and gives tables of the subordinate groups and genera, and the synonymy of nearly all the species, so that his work (including its supplement) is very nearly a monograph of the curious and interesting group of insects which constitutes its subject. A great number of the species described by the author are figured on four beautifully executed plates which accompany the memoir, and these will astonish the non-entomological reader by the variety of curious forms produced by modifications of the same plan of structure.—The other papers in this part consist of descriptions of new or little-known exotic Cryptogamia (Mosses), by M. J. E. Duby, illustrated with four plates; a paper on gelatiniform matter by M. Morin, and a report on the labours of the society by its President, M. Henri de Saussure.

PART II. of the *Bulletin of the Royal Swedish Academy of Sciences* (Ofversigt af Kongl. Vetenskaps Akademiens Förhandlingar) for the present year commences with a paper (in Latin), by Dr. E. Fries, containing a description of *Queletia*, a new genus of Lycoperdaceous Fungi, and of a new species of the genus *Gyromitia*. The characters of the former are illustrated in a plate.—Another botanical paper is a notice of some Algæ from the inland ice of Greenland, by M. S. Berggren. The author describes and figures a peculiar form, which he regards as most approaching the Zygnemaceæ, but as having an unmistakable resemblance to some Desmidiaceæ.—Passing by a rather wide step from Greenland to South Africa, we have Latin descriptions of 226 Caffrarian Curculionidæ, collected by Wahlberg, from the pen of M. O. J. Fahreus. These all belong to Lacordaire's second division of the family.—M. B. Lundgren publishes a notice of the occurrence of amber at Fyllinge, in Halland.—The remaining papers are upon chemical subjects, and include a paper by M. P. T. Cleve on some remarkable isomerisms in organic chemistry; a paper by the same author on the nitrites of some platinum-bases; and one by M. L. F. Wilson on the sulphides of arsenic.

Journal of the Chemical Society, September 1871. Bolas and Groves have continued their researches on carbon tetrabromide, and have obtained some interesting results. In their former paper they mentioned that antimony terbromide could be substituted for iodine in the preparation of the tetrabromide. They now find that bromine will act on carbonic disulphide in the presence of the bromides of the following metals:—bismuth, arsenic, gold, platinum, cadmium, zinc, and nickel; the bromides of iron, tin, phosphorus, and sulphur, however gave very unsatisfactory results. The authors still think the mixture of bromine and iodine the most convenient reagent for the preparation of the tetrabromide. The authors recommend for the recovery of bromine from residues the action of dipotassic dichromate and sulphuric acid.—R. C. Woodcock has examined the action of ammonic chloride on normal and acid salts; he has experimented on the following bodies:—potassic chromate, microsmic salt, trisodic phosphate, dipotassic tartrate, succinate, &c. By the action of ammonic chloride on sodic metaborate the whole of the ammonia is evolved, sodic chloride and metaboric being formed. Borax also yields the whole of the ammonia, sodic chloride and tetrametaborate remaining behind. Both soluble and insoluble chromates yield ammonia when distilled with ammonia salts, an acid chromate being formed; the whole of the ammonia, however, is not evolved, the acid chromate formed at a certain point stopping the evolution of ammonia; if the acid salt be removed by crystallisation, a copious evolution of ammonia again takes place on boiling.—W. Mattieu Williams communicates a short abstract of a paper "On Burnt Iron and Steel." Iron which has been damaged by re-heating is designated "burnt iron;" it is brittle, its fracture being short, displaying the so-called crystalline structure. In all the samples which the author has examined, he has found particles of black oxide of iron diffused in the mass. The oxidation must of course take place after that of the carbon present in the iron. It is found that iron attains its maximum toughness when the carbon is reduced to the lowest possible proportion without the oxidation of the iron commencing. When steel is raised to a yellow or white heat, and is suddenly cooled, it turns brittle. Burnt steel has a coarse, granular fracture, and contains small cavities, technically called "toads' eyes." These are probably due to the sudden cooling of the iron imprisoning the carbonic oxide, which is evolved by the oxidation of the carbon; this oxidation not only takes place at the surface of the mass, but also in the interior, from the fact that certain gases can pass readily through heated iron. This explanation is strengthened by "burnt steel" being cured by welding up these cavities. The remainder of the number is occupied with the abstracts of chemical papers, which extend over seventy-five pages, and are quite up to the usual standard, both in scientific interest and as regards literary merit.

Journal of the Chemical Society, November 1871.—This number does not contain any papers originally communicated to the Society. It is not certainly to the credit of English chemists that this should be the case for two months in succession; the number of English chemists who devote their time to original research seems every year to become smaller; on the Continent, however, the reverse is the case, as is shown by the very large number of abstracts, which are published monthly by the Society. This month about 130 papers are abstracted, which fill 127 pages. Amongst them we notice a remarkable communication by Angström "On the Spectra of Simple Gases." Angström took a tube filled with atmospheric air and gradually exhausted it by a mercurial pump, the spectra being obtained by the use of an induction coil. He states that he observed successively the following spectra: 1st, that of atmospheric air; 2nd, the band spectrum of nitrogen; 3rd, that of carbonic oxide; and 4th, when the rarefaction had reached its limit, the lines of sodium and chlorine. He has also experimented on hydrogen, and concludes that it possesses only one spectrum, that of four lines, which is observed in the spectra of the sun and stars. He believes that the various spectra of hydrogen obtained by Plücker, Frankland and Lockyer, Wöllner, and others, are entirely due to impurities, such as acetylene and sulphur.—An abstract of a paper by Andrews contains a curious fact. A fine tube is half filled with bromine and hermetically sealed; on heating, the bromine becomes opaque, so that the tube appears to be filled with a dark red resin.—Lieben and Rossi continue their researches on the normal alcohols and acids of the methyl series; a review of their results has already appeared in these pages.—Ladenburg contributes another most interesting paper, "On the reduction products of silica, ether, and some of their derivatives;" these researches are very important, and have opened out quite a new branch of chemical inquiry. He has obtained such bodies as silicium, diethylketonic ether ($SiC_2H_{52}OC_2H_{52}$), silicoheptyl ether ($SiC_2H_{53}OC_2H_5$), and so on.—Another paper of some interest is by Heinrich, "On the Influence of Heat and Light on the Evolution of Oxygen by Water Plants." He experimented on the leaves of the *Hottonia palustris*, which were placed in common water. At a temperature of 2·7° C. in full sunlight no evolution of gas took place, but at 5·6° a regular evolution commenced. The most active formation was at 31°, and at 50° to 56° gas ceased to be formed, but the leaf resumed its activity in cooler water. If the leaves were exposed to a temperature of 60° for ten minutes, their power of decomposing carbonic acid was destroyed.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, December 20, 1871.—Mr. Joseph Prestwich, F.R.S., president, in the chair. Mr. Frederick H. Bowman, F.R.A.S., F.C.S., of Halifax, Yorkshire, and Mr. Thomas Charles Sorby, B.A., F.R.S., of 27, Brunswick Square, W.C., were elected Fellows of the Society. The following communications were read:—1. A Letter from Mr. G. Milner Stephen, F.G.S., to the late Sir Roderick Murchison, dated Sydney, 5th October, 1871, announcing the discovery of a rich auriferous deposit on the banks of the River Bondé, on the N.E. coast of

New Caledonia, and of a great deposit of tin-ore in the district of New England, New South Wales. The gold in New Caledonia is found in drift, and there are indications of the near proximity of a quartz-reef. The tin-ore in New South Wales is said to be in "pepitas, crystals, and beds of conglomerate, especially in micaceous granite, more or less decomposed." Mr. D. Forbes stated that in 1859 he had placed in his hands some specimens of granite from the district the discovery of tin in which was announced by Mr. Stephen, and that he found them to be perfectly identical with the stanniferous granites of Cornwall, Spain, Portugal, Bolivia, Peru, and Malacca, which he had also examined. These granites were all composed of white orthoclase, felspar, colourless or black Muscovite mica and quartz. He was not aware that tinstone (cassiterite or oxide of tin) occurred anywhere in rock of a different character. It was always accompanied by more or less native gold. Mr. Pattison remarked that in many places where tin occurred it was not present in sufficient quantity to be remuneratively worked. Mr. D. Forbes, in answer to a question from Prof. Ramsay, stated that, as far as could be ascertained, the age of the stanniferous granites mentioned by him must be between the end of the Silurian and the early part of the Carboniferous period. Prof. Ramsay would carry them down to the close of the Carboniferous period, and would be contented to term them pre-Permian.—"Remarks on the Greenland Meteorites." By Prof. A. E. Nordenskjöld, For. Corr. G.S. The author stated that the masses of meteoric iron brought from Greenland by the recent Swedish expedition seem to have formed the principal masses of an enormous meteoric fall of miocene date, extending over an area of some 200 miles. The iron appears to be free from silicates. Against its eruptive origin the author urges that when heated it evolves a great amount of gaseous matter, and that it contains imbedded particles of sulphide of iron, the mass itself being nearly free from sulphur. The masses are composed of meteoric nickeliferous cast and wrought iron, or of mixtures of the two; in the last case the Widmannstætten's figures are best developed. The author further noticed the various modes in which the iron occurs, viz., 1, as meteorites; 2, filling cracks; 3, as brecciæform stones cemented with oxide and silicate of iron; and 4, in grains disseminated in the basalt. Mr. Roberts protested against the evolution of gaseous matter being considered as a proof of meteoric origin. Prof. Ramsay reiterated his previously-expressed opinion, that the masses of iron might be of telluric origin.—"Further Remarks on the Relationship of the *Limulidæ* (*Xiphosura*) to the *Eurypteridæ* and to the *Trilobita*." By Mr. Henry Woodward, F.G.S. In this paper the author described the recent investigations made by Dr. A. S. Packard, Dr. Anton Dohrn, and the Rev. Samuel Lockwood upon the developmental history of the North American King-crab (*Limulus Polyphemus*), and discussed the conclusions as to the alliances of the *Xiphosura* and *Eurypteridæ*, and to the general classification of the *Arthropoda*, to which the results of these investigations have led Dr. Dohrn and some other Continental naturalists. According to this view, the *Xiphosura* and *Eurypteridæ* are more nearly related to certain Arachnida (the Scorpions, &c.) than to the Crustacea; and this opinion is further supported by the assertion of Dr. Dohrn, that in *Limulus* only one pair of organs (antennules) receives its nerves from the supraœsophageal ganglion, and that the nature of the underlip in *Limulus* differs from that prevailing among the Crustacea. Dr. Dohrn also recognises the relationship of the Merostomata to the Trilobites, as shown especially by the development of *Limulus*, and considers that the three forms (*Limulidæ*, *Eurypteridæ*, and *Trilobita*) should be combined in one group under the name of *Gigantostraca*, proposed by Haeckel, and placed besides the Crustacea. The author stated, on the authority of Prof. Owen, that *Limulus* really possesses two pairs of appendages which receive their nerves from the supraœsophageal ganglion; that, according to Dr. Packard, the young *Limulus* passes through a Nauplius-stage while in the egg; that no argument could be founded upon the lower lip, the condition of which varied extremely in the three groups proposed to be removed from the Crustacea; and he maintained that even from the ultra-Darwinian point of view taken by Dr. Dohrn, the adoption of his proposal would be fatal to the application of the hypothesis of evolution to the class Crustacea. Prof. T. Rupert Jones remarked upon the interest attaching to the study of the Crustacea, and called attention to the absence of any indications of convergence in our present knowledge of the class. He thought that, in the present day, we must nevertheless look back to some point of convergence from which the varied forms known to us may have proceeded by evolution. Prof. Macdonald remarked that difficulties must be expected to occur in classification. He believed that all Invertebrate animals were to be regarded as turned upon their backs, as compared with Vertebrata. The cephalic plate in *Limulus* he regarded as the equivalent of the palate-bone. The incisive palate was very distinct in the Crabs. The absence of one pair of antennæ did not appear to be any reason for removing *Limulus* from the Crustacea. Dr. Murie considered that the contemplation of the multitude of young forms referred to by Mr. Woodward should serve as a warning to describers of species, and also as a check to generalisations as to the number of species occurring in various formations. He remarked that if we were at a point when the presence or absence of a single pair of nerves could be taken as distinguishing class from class, these classes must be regarded as very nearly allied. He thought that the doctrine of evolution was being pushed further than the known facts would warrant. Mr. Woodward, in replying, drew attention to the diagrams of the embryo and larva of the recent *Limulus*, comparing them with *Limulus* of the Coal-measures, *Neolimulus* of the Silurian, and also with the larval stages of the Trilobites, discovered by Barrande. He pointed out the strong resemblance which the fossil forms offer to the early stages of the modern King-crab, and expressed his assent to the proposal of Dr. Dohrn to bring the Trilobita, if possible, nearer to the Merostomata. If, however, the Trilobites have true walking-legs instead of mouth-feet (gnathopodites) only, they would be more closely related to the Isopoda. He showed by a tabular view of the Arthropoda that the known range in time of the great classes is nearly the same, and therefore affords no argument for combining the Merostomata with the Arachnida; but on the contrary, he considered that the Trilobita were, with the Entomostraca, the earliest representatives of the class Crustacea, and could not therefore be removed from that class.—The following specimens were exhibited:—Specimens of Auriferous Quartz from New Caledonia, and of Tin Ore from New South Wales, exhibited by Mr. G. Milner Stephen; specimen of gold from the Thames Goldfield, New Zealand, exhibited by Prof. Tennant; specimens of *Eurypterus Scouleri* and of *Belinurus* and *Prestwichia*, exhibited by the President; specimens of recent and fossil Crustacea, exhibited by Mr. H. Woodward, in illustration of his paper.

Zoological Society, January 2.—Mr. John Gould, F.R.S., in the chair. An abstract was read from a letter received from Mr. T. G. F. Riedel, of Gorontalo, Celebes, in reference to the true locality of a rare Kingfisher, *Tanysiptera Riedeli*, which he stated to be from Kordo—an island in the Bay of Geelvink, and not from Celebes.—Prof. Newton exhibited and made remarks on a specimen of Ross' Gull (*Larus Rossi*), from the collection of the late Sir William Milner, which was said to have been obtained in Yorkshire.—Mr. Gould exhibited an adult specimen of the same bird, from the Derby Museum, Liverpool.—Mr. P. L. Sclater read a paper on the species of monkeys found in America north of Panama, being supplementary to a former paper on the northern limit of the Quadrumana in the New World. The species of monkeys now ascertained to occur in Central America from Panama to Mexico were stated to be eleven in number—namely, ten belonging to the family Cebidæ, and one to the Hapalidæ. Full particulars were given concerning the range of each of these species.—Mr. Henry Adams communicated some further description of new species of shells, collected by Mr. R. McAndrew, in the Red Sea. A second paper by Mr. H. Adams contained descriptions of fourteen new species of land and marine shells from Mauritius, Mexico, Formosa, Borneo, and the New Hebrides.—Mr. George Gulliver communicated a paper on the œsophagus of a hornbill (*Toccus melanoleucus*), being an appendix to a former paper by him on the taxonomic character of the muscular sheath of the œsophagus of the Sauropsida, read at a previous meeting of the Society.—Mr. J. Brazier communicated some observations on the distribution of certain species of volutes found in the Australian seas. In a second paper Mr. Brazier gave descriptions of six new species of land and marine shells from the Solomon Islands, Western Polynesia, and Australia.—Dr. J. C. Cox communicated descriptions of some new land shells from Australia and the South Sea Islands.

Entomological Society, January 1.—Mr. Alfred R. Wallace, president, in the chair.—The secretary read an extract from a letter received from Mr. Gould respecting the question of the liability of dragon-flies to the attacks of birds. Mr. Gould had no doubt that the hobby and kestrel attacked the larger kinds, and he had seen sparrows, &c., preying upon the smaller *Agrionidæ*.—Mr. Müller called attention to a statement by

M. Emile Joly to the effect that Latreille's supposed crustaceous genus, *Prosopostoma*, is probably founded upon the immature condition of certain *Ephemeridæ*.—Mr. Butler read a paper "On certain species of *Pericopides*"—Mr. F. Smith read a letter from Mr. J. T. Moggridge with reference to the habits of some species of ants belonging to the genus *Aphenogaster*, as observed at Mentone in the winter. Mr. Moggridge affirmed that those ants harvested the seeds of various plants in chambers, sometimes excavated in solid rock. He had seen them busily engaged in conveying the seeds into these chambers, and found that, in most cases, the radicle was bitten off, so as to prevent germination; but he had also observed sprouted seeds being brought out again as apparently unfitted for store purposes. Many of the seeds had their contents extracted through a hole in one side, and though he had not actually seen the ants feeding upon them, he was inclined to believe that the stores were made for the purpose of providing food in the winter months.

Society of Biblical Archæology, January 2.—Mr. S. Birch, president, in the chair.—A paper entitled "Hebræo-Ægyptian in Hebrew-Egyptian Analogues," contributed by M. François Chabas, Membre de l'Institut, and translated for the society by Mr. E. R. Hodges, was read by the translator. In this the learned Egyptologist, having enumerated the various sources and original texts from which his materials were taken, proceeded to consider the various moral and religious parallelisms of the Egyptians and Hebrews under three distinct sections: (1) Laws respecting charity and special duties; (2) Commands and proverbs enforcing the obligation of filial obedience; (3) Legal formulæ and reports, referring to the prohibition of blasphemous and irregular oaths. Under each of the divisions several translations of hieroglyphic texts were given, together with an exegesis justifying the renderings adopted by M. Chabas. The last section, in which the adjuration "by the life of God, and by the life of Pharaoh" was explained, possessed, in the opinion of the learned author, special interest from its exact attestation of the minute accuracy of certain portions of the Pentateuch, and as throwing much light upon a passage hitherto obscure or unknown to the bulk of English students.—The president read a paper "On the Cypriote Inscription on the Bronze Tablet of Idalium" (Dali). Having referred to the felicitous discovery, by Messrs. Lang and Smith, of the Cypriote alphabet, as announced to the society at its last meeting, he entered into the consideration of the Cypriote parts of the bi-lingual inscription of Dali, and the Hellenic element of the Cypriote language. He then proceeded to give some account of the Cypriote inscription on the bronze tablet of Dali, which records donations to the Temple of Idalium by the monarch, Pythagoras, and Indostes. It also referred to various writings in connection with a temple of Isis. Its date of inscription appears to be about B.C. 256. Examples were given of the Hellenic structure of the language, and the identification of many Cypriote with Greek words. An interesting discussion took place, in which Sir C. Nicholson, Emanuel Deutsch, Rev. J. M. Rodwell, S. M. Drach, W. R. A. Boyle, the president, and the secretary, took part.

EDINBURGH

Royal Physical Society, Dec. 20, 1871.—Mr. C. W. Peach, president, in the chair.—"Zoological Notes," by Prof. Duns. (1.) On a dog-fish (*Scyllium marmoratum*) from Java (2.) On the Porbeagle, or Beaumaris shark (*Lamna cornubica*). The specimen exhibited was a beautiful young one captured last year near Elie, Fifeshire. The difference between the dentition of the adult and the young was well illustrated in this case. The lanceolate teeth of the former have a small basal cusp on each side. The cusps are absent in the latter. (3.) On Rondelet's little Sepia (*Sepiola Rondeletti*). A specimen taken in the Firth of Forth was exhibited. (4.) On the Redwing (*Turdus iliacus*).—On the Extirpation of Venomous Serpents from Islands, by Robert Brown. This consisted of correspondence addressed to the author and Mr. W. B. Tegetmeier relating to the subject. It was shown that the common domestic pig had exterminated rattlesnakes in the vicinity of the Dalles and other settlements in Oregon, and that in India the same antipathy is shown by the same animal to the deadly cobra di capello. The subject was important economically to the inhabitants of some of the West Indian Islands infested by these reptiles, and physiologically in so far as facts went to show that the pig enjoyed an immunity from the poison of both the rattlesnake and the cobra. In Ireland it was well known few or no snakes of any kind are found, and nowhere is "the pig" more abundant, showing a probable relation between these two facts, without calling in the supposed aid of St. Patrick.—Exhibition of Glacial Shells of the Clyde Beds, from a recent Excavation near Greenock, by David Grieve. Also of Specimens of various Polyzoa and Foraminifera from the same locality, with remarks by C. W. Peach.—"On Shells, Foraminifera, &c., from the recent post-tertiary beds between the Bridge of Allan and Stirling" (specimens exhibited), by C. W. Peach.

GLASGOW

Geological Society, December 14, 1871.—Mr. James Thomson, F.G.S., read a paper on "The Stratified Rocks of Islay." He described in detail the sedimentary deposits on the south side of the island, and then gave a transverse section of them from Port-na-Haven on the west to Port Askaig on the east. Although the rocks in the central valley of the island had not yet yielded identifiable organic remains, he did not despair, if properly investigated, of forms being found that would place them beyond doubt in the Lower Silurian series. In mineral character they quite coincided with those described by the late Sir Roderick Murchison as occurring in Ross and Sutherlandshires. On the east side of the island, at Port Askaig, these deposits repose upon a series of stratified rocks of much higher antiquity, which correspond to the Cambrian rocks of the North-West Highlands, described by the same distinguished author. At the base of these latter sedimentary rocks there is a mass of conglomerate, made up of fragments and boulders of granite, imbedded in an arenaceous talcose schist; and as no granite occurs *in situ* in the island, he was disposed to account for its presence in this conglomerate by the agency of ice. Specimens of the granite and a striated block of quartzite were laid upon the table. He then described the rocks of the western extremity of the island, which consist of highly metamorphosed stratified rocks, as gneiss, serpentine, dolomite, quartzite, and schists, extending from Port-na-Haven, on the west, to Brouch-Ladach, a distance of nine miles. At the latter point the superior deposits are seen resting on the metamorphosed sedimentary rocks, nearly at right angles to the planes of stratification. In lithological aspect and mineral character these rocks agreed so entirely with the "fundamental or Laurentian gneiss" of Sir R. Murchison, as occurring in the North-Western Highlands and other parts of the world, that he had not the slightest hesitation in placing them as belonging to this, the oldest division of known sedimentary rocks. It thus appeared that both Cambrian and Laurentian rocks occurred farther south in Scotland than had hitherto been recorded. Taking a general view of the group of deposits to which he had called attention, there were—1. The calcareous deposits in the central valley of the island, of Lower Silurian age; 2. The deposits from Ardnahuamh on the north to Balleochreoch on the south, of Cambrian age; 3. The metamorphic rocks in the west of the island, of Laurentian age. He was not prepared to speak with any degree of certainty regarding the source of the materials constituting the basic conglomerate mass. These differ so widely from the granites found *in situ* in other parts of the Highlands, that he felt the necessity for tracing them to another source, and hoped he would not be thought to overstep the bounds of prudent speculation in suggesting that these erratics are the reassorted materials of some great northern continent that has yielded to the gnawing tooth of time, leaving only these scattered fragments to attest its former existence. The portion of striated rock which he had laid before the meeting pointed to an agency adequate to the transport of such materials, and indicated that we should have to contemplate a glacial period deeper in time than had hitherto been suspected, when glaciers and icebergs planed down the hardest rocks and dispersed their fragments, obedient to the same great laws which still regulate the economy of Nature.

NEW ZEALAND

Wellington Philosophical Society, August 26, 1871.—Capt. Hutton described the two species of bats found in New Zealand, and proposed that the name *Mystacina tuberculata* be changed to *M. velutina*, to avoid confusion with *Scotophilus tuberculatus*. Dr. Hector mentioned that large numbers of the former species lodged in the topsails of H.M.S. *Clio* when in Milford Sound last summer.—Mr. Skey proposed as a convenient method of generating H_2S for laboratory use, to employ galena, zinc, and dilute hydrochloric acid.—Captain Hutton described the microscopic structure of the egg-shell of the moa, and showed that it was altogether different from the kiwi egg.

September 16.—Mr. W. T. L. Travers described the traditions of the Maories, showing reasons why they were not reliable as history, and that the usual date assigned for the first landing of the Maories is much too recent.—Captain Hutton read a paper on the lizards of New Zealand, and described a new species from

White Island, belonging to the genus *Norbea*, hitherto only found in Borneo, and also a new species, *Mocou laxa*.

September 30.—Mr. Travers described the habits of the birds that frequent the lake in the interior of Nelson, mentioning that the blue duck (*Hymenolaimus*) does not exhibit solicitude for the safety of its young like other ducks. Captain Hutton showed that this supported the Darwinian theory, as the blue duck belongs to a genus peculiar to New Zealand when there were no destructive animals previous to the arrival of man, and in which genus, therefore, instinctive fear has not been developed. Dr. Hector showed that absence of fear is characteristic of most of the birds peculiar to New Zealand, but that the weka of the North Island is much more shy than the species in the South.—Dr. Hector described a portion of a wreck discovered on the west coast of the Middle Island, and pointed out that the coast line had advanced 300 yards since it was cast up.

October 14.—A communication by Dr. Wojeikof, of St. Petersburg, on the change of climate effected by clearing forests, led to much discussion, from which it appeared that this colony is now suffering in many districts from the sudden and severe floods that are due to this cause.—Captain Hutton read critical notes of the birds of New Zealand that accompany a descriptive catalogue he has published.

October 28.—Dr. Hector reported the result of Dr. Thomson's exploration of the cave in Otago in which the Moa's nest was found (see NATURE, vol. iv. pp. 184, 228). It is an irregular fissure in mica schist rock, about fifty feet deep, and with thin flat ledges or floors on which the bones rest. There are entrances, one from rocks on the mountain side, and the other by a funnel-shaped hollow in an alluvial flat. On the first floor Dr. Thomson found traces of a fire and charred bones. On the second floor, by scraping away the loose dust to the depth of two feet, leg bones, ribs, vertebræ, a pelvis, toe bones, tracheal rings, and pieces of skin and muscle were found. On the third floor were found fragments of egg-shell, and the bones of a bird with a keeled sternum. In Dr. Thomson's collection there are sixteen tibiæ, so that he obtained remains of at least eight birds. A perfect skull with lower jaw and trachea attached, and a femur with well preserved muscular tissues attached, were found on the spot where the nest was obtained. From another locality in the same district Dr. Thomson sends twenty feathers. These were found by a gold digger eighteen feet below the surface. A report on these feathers by Capt. Hutton showed that they were of the form peculiar to struthious birds, but quite different from any known species. They are eight inches long, with soft yellow down on the lower half, and black above except the tip, which is white. The form of the feather is very peculiar, as it expands in width to the tip. He considers that the structure of these feathers shows that the bird to which they belonged was allied more to the American robin than to any of the struthious birds of the old world.

VIENNA

I. R. Geological Institution, Dec. 5, 1871.—M. Ernest Favre exhibited a geological map of the central part of the Caucasus Mountain chain, which he had surveyed last summer. The region which formed the object of his inquiries is limited to the east by the military road which leads to Georgia, to the west it ends with the Elbrus Mountain, to the north it is limited by the Steppe, and to the south by the Koura Valley, the mountains of Souram and the plain of Mingrelia. In this region the Caucasus rises to its greatest height; summits of 12,000 to 18,000 feet above the the sea level being not rare. Granite and crystalline slates form large masses in the central part, further to the east and west they disappear beneath the younger sedimentary rocks. The lowest fossiliferous strata belong to the Liassic formation. The gigantic peaks of the Elbrus and the Kayhek on the north flank of the chain are formed by trachite.—Mr. F. Schrökenstein "On the Cyipka Balkan." The author has crossed the Balkan mountains in two lines, unvisited before by any geologist, once from Drawna by Selce to Kysanlik, and than back over the Cyipka to Grabowa. The series of rocks found there he enumerates as follows:—1. Crystalline schists; 2. Coal formation, the base of which is formed by quartzite, higher up follows calcareous slate, and finally sandstone and slate with coal measures; 3. Dyas; 4. Magnesian limestone; and 5. The Neocomian series covering the older rocks unconformably. The discovery of large coal seams in the coal formation near Radience is very important. German capitalists have got permission to work them, and have already traced a railway from the mine to the Danube.

BOOKS RECEIVED

ENGLISH.—Schellen's Spectrum Analysis: Translated by Jane and Caroline Lassell; Edited, with Notes, by W. Huggins (Longmans).—Deschanel's Natural Philosophy; Part III., Electricity and Magnetism: Translated by Prof. Everett (Blackie and Sons).—Zoological Record, Vol. vii.—Rudimentary Magnetism: Sir W. S. Harris and H. M. Noad (Lockwood).—Spiritualism Answered by Science: Serjt. Cox (Longmans).

AMERICAN.—Reports on Observations of the Total Solar Eclipse of Dec. 23, 1870, conducted under the direction of Rear-Admiral Sands, U.S.N.

DIARY

THURSDAY, JANUARY 11.

ROYAL SOCIETY, at 8.30.—Experiments made to determine Surface Conductivity in Absolute Measure: D. McFarlane.—On the Myology of the Cheiroptera: Prof. Macalister.

SOCIETY OF ANTIQUARIES, at 8.30.—Ballot for the Election of Fellows.

MATHEMATICAL SOCIETY, at 8.—On Surfaces: the loci of the vertices of cones which satisfy six conditions: Prof. Cayley.—On the Constants that occur in certain summations by Bernouilli's series: J. W. L. Glaisher.—On the Construction of large tables of divisors and of the factors of the first differences of prime powers: W. B. Davis.—On Parallel Surfaces of Conicoids and Conics: S. Roberts.

FRIDAY, JANUARY 12.

ASTRONOMICAL SOCIETY, at 8.

QUEKETT MICROSCOPICAL CLUB, at 8.

MONDAY, JANUARY 15.

ANTHROPOLOGICAL INSTITUTE, at 8.

LONDON INSTITUTION, at 4.—Elementary Chemistry: Prof. Odliog.

TUESDAY, JANUARY 16.

ZOOLOGICAL SOCIETY, at 9.—On a fourth collection of Birds from the Pelew and Mackenzie group of Islands: Dr. G. Hartlaub and Dr. O. Finsch.—Notes on the Myology of *Leiolepis bellii*: Alfred Sanders.

STATISTICAL SOCIETY, at 7.45.—On Licensing and Capital Invested in Alcoholic Drinks; Prof. Levi.

ROYAL INSTITUTION, at 3.—On the Circulatory and Nervous Systems: Dr. W. Rutherford.

WEDNESDAY, JANUARY 17.

SOCIETY OF ATS, at 8.—On the Oral Education of the Deaf and Dumb: G. W. Dasent.

METEOROLOGICAL SOCIETY, at 7.

THURSDAY, JANUARY 18.

ROYAL SOCIETY, at 8.30.

SOCIETY OF ANTIQUARIES, 8.30.

ROYAL INSTITUTION, at 3.—On the Chemistry of Alkalies and Alkali Manufacture; Prof. Odling, F.R.S.

LINNEAN SOCIETY, at 8.—On the Anatomy of the American King-Crab (*Limulus polyphemus*, Lat.): Prof. Owen, F.R.S. (*Continued.*)

CHEMICAL SOCIETY, at 8.

CONTENTS

NOTICE

We beg leave to state that we decline to return rejected communications, and to this rule we can make no exception. Communications respecting Subscriptions or Advertisements must be addressed to the Publishers, NOT *to the Editor.*

THURSDAY, JANUARY 18, 1872

THE SOLAR ECLIPSE

SURELY if eclipse expeditions had their mottoes, that of the expedition of this year should be *per mare per terram;* for it has been *per mare per terram* in our case with a vengeance! Probably when we return, the curious individuals who total up in the *Times* the aggregate number of years those people have lived whose deaths are there recorded, will, in asking us for our autographs, beg also a detailed statement of the number of miles each of us has travelled in the performance of our duty. I fear it will be very difficult to give the information; and if the temperature in the shade be wanted too, the thing will be perfectly hopeless; for, thank goodness, we took the precaution to bring no thermometers; had we done so and looked at them, it might have been all over with us. Let me point my remarks. A week ago I was at Bekul, having travelled I know not how many thousand miles by sea, and having scarcely set foot on land for a month. We were in the jungle, the heat was burning, some of us had fever, and it was opium which enabled me at all events to get through the eclipse, for it was that memorable day just a week ago. Since then, by night and by day, Dr. Thomson, Captain Maclear, and myself, have been—I seek a word, wafted is too weak, jolted is too strong, for some parts of our journey, though ridiculously lacking in expression for others—well, conveyed from Bekul, now in men-carried conveyances, the cunning bearers with their plaintive moaning, by no means unmelodious, keeping step, giving us an idea of the tremendous labour they were undergoing, and reminding us of a certain journey which we must all make once; now on men's shoulders, now in bullock bandy, speed about two miles an hour, thanks to a brutal breach of contract, which has upset my plans terribly, now in Indian railway carriages, average speed ten miles an hour, temperature of carriage at noon unknown, and lastly in the horse transit of the Madras Carrying Company. Oh! that their carriages were as good as their arrangements and the speed of their horses; and, now, here I am shivering, surrounded by hoar frost, with a soupçon of a difficulty of breathing in this higher air after the dense atmosphere of the jungles, but all the same in an earthly paradise with hedges of roses although it is mid-winter, the whole place a perfect garden. I am at Ootacamund, at an elevation of some 7,000 feet with an Australian fauna; and within a few hours I hope to see Janssen, who is still here; Tennant, Herschel, and Hennessy I have unfortunately missed, owing to the breach of contract already referred to.

We can all of us, or nearly all of us, afford to laugh now at any inconveniences we have suffered; for of the eleven who landed at Galle nine have seen the eclipse, some of us perhaps as an eclipse has never been seen before. Unfortunately, to the regret of all, Mr. Abbay and Mr. Friswell, who were among the best prepared for doing good work, and were at a station at which everybody said cloudless weather was certain, found themselves on the 12th in a storm of cloud and mist, which obscured the sun for, I believe, the whole day. With this exception the telegrams from all the English parties have been sent regularly, while we have all been thankful to learn from the telegrams which Dr. Janssen and Colonel Tennant have had the great courtesy to send me, that they too saw the eclipse well, as also did Mr. Pogson, as I gather from the newspapers, but of course the details of their observations are still unknown to me. Hence, I can only give the facts observed by the party at Bekul and Poodocottah; Prof. Respighi, who observed at that station, having joined me at Pothanore, the station on the Madras Railway, at the foot of the hills which we ascended yesterday from 4 30 A.M. till 1 P.M.

But before I say a word about the observations themselves, it is incumbent upon me to express our deep obligations to the supreme Madras and Ceylon Governments for the magnificent manner in which they have aided us. Nothing could be more complete than the arrangements at Bekul made by the collector, Mr. Webster, and his assistant, Mr. McIvor, both for the work to be done and the comfort of those who had to do it. The same must be said for the Poodocottah party, where not only the collector, Mr. Whiteside, but the Rajah did everything in their power, the latter loading the observers with presents when they left. We have at present heard only of the discomforts of the Manantoddy party, and it is clear that here the local arrangements were in strong contrast to those elsewhere. The Ceylon parties, who parted from the main body at Galle, have doubtless been well looked after; as Captain Fyers, the Surveyor-General of the island, accompanied and aided them in their observations.

This brings us to another part of the arrangements. The Ceylon party had the unreserved use of the Government steamer the *Serendib*, to take them from Galle to their places of observation, Jaffna and Trincomalee, both on the coast, and the accommodation on board was perfect. The Indian parties proceeded to their various destinations, or the ports on the coast nearest to them, in the Admiral's flag-ship the *Glasgow*, which, however, could not remain to bring them back, a circumstance which has given rise to very considerable inconvenience and great risk for the instruments, which are now scattered all along the line, to be sent to the coast and from the coast to Bombay or Galle, as circumstances may determine. This of course was not to be helped, and we must hope for the best, especially as all the parties have done their utmost in superintending their repacking, and handing them over in perfect condition to the different Government officers who accompanied each party. Still, although it was not to be avoided, the withdrawal of the ship has been the unfortunate circumstance in the arrangements. Nothing could exceed the kindness of the Admiral, who vacated his own quarters to give us room, of Captain Jones, who took the warmest interest in our proceedings, and helped the arrangements greatly, and by the officers of the ship generally. Without the equal kindness of Mr. Webster at Bekul, the step from the Admiral's cabin into the jungle hut would have been a seven-league one.

As the mail, the first available one after the eclipse, leaves this place to-day, I must lose no more time in recording preliminaries. I will therefore at once state the general arrangements of the parties, and what I at present know of the observations. The stations and observers as finally arranged were as follows :—

Bekul—Analysing Spectroscope, Capt. Maclear and Mr. Pringle.
Polariscope, Dr. Thomson.
Photography, Mr. Davis.
Manantoddy—Analysing Spectroscope, Mr. Friswell.
Integrating Spectroscope, Mr. Abbay.
Poodocottah—Spectroscope, Professor Respighi.
Sketches of Corona, Mr. Holiday.
Jaffna—Integrating Spectroscope, Capt. Fyers and Mr. Ferguson.
Polariscope, Capt. Tupman and Mr. Lewis.
Photography, Captain Hogg.
Trincomalee—Spectroscope, Mr. Moseley.

Besides these observers, we had at Bekul the valuable assistance of General Selby, commanding the troops in Canara and Malabar (for whose help in supplying guards' tents, &c., the friends of Science cannot be too thankful), Colonel Farewell, Judge Walhouse, and others, in sketching the Corona. At all stations, of course, most precious help in various ways was given by all present who volunteered for the various duties, though some of them lost a sight of the eclipse in consequence. Among those who helped in this way at Bekul were Mr. McIvor, Mr. Pringle, Captain Bailey who timed the eclipse, Mr. Cherry, and Captain Christie, the Inspector of Police, whose presence there turned out to be of the most serious value, for the natives seeing in the eclipse the great Monster Rahoo devouring one of their most sacred divinities, not only howled and moaned in the most tremendous manner, but set fire to the grass between our telescopes and the sun to propitiate the representative of the infernal gods. Captain Christie with his posse of police stopped this sacrifice at the right moment, and no harm was done.

Now for the observations. Perhaps I may be permitted to begin with my own, as at the present moment I know most about them. I determined to limit my spectroscopic observations to the spectrum of a streamer, and to Young's stratum, thereby liberating a number of seconds which would enable me to determine the structure of the undoubted corona with a large refractor, to observe the whole phenomena with the naked eye, and through a train of prisms with neither telescope nor collimator, and finally with a Savart and biquartz. I found the 120 seconds gave me ample time for all this, but owing to a defect in the counterpoising of my large reflector, which disturbed the rate of my clock, I missed the observation of the bright line stratum (assuming its existence) at the first contact. At the last contact Mr. Pringle watched for it and saw no lines.

Having missed this, I next took my look at the corona. It was as beautiful as it is possible to imagine anything to be. Strangely weird and unearthly did it look—that strange sign in the heavens! What impressed me most about it, in my momentary glance, was its serenity. I don't know why I should have got such an idea, but get it I did. There was nothing awful about it, or the landscape generally, for the air was dry and there was not a cloud. Hence there were no ghastly effects, due generally to the monochromatic lights which chase each other over the gloomy earth, no yellow clouds, no seas of blood—the great Indian Ocean almost bathed our feet—no death-shadow cast on the faces of men. The whole eclipse was centred in the corona, and there it was, of the purest silvery whiteness. I did not want to see the prominences then, and I did not see them. I saw nothing but the star-like decoration, with its rays arranged almost symmetrically, three above and three below two dark spaces or rifts at the extremities of a horizontal diameter. The rays were built up of innumerable bright lines of different lengths, with more or less dark spaces between. Near the sun this structure was lost in the brightness of the central ring.

But from this exquisite sight I was compelled to tear myself after a second's gazing. I next tried the spectrum of a streamer above the point at which the sun had disappeared. I got a vivid hydrogen spectrum, with 1474 (I assume the point of this line from observation) slightly extended beyond it, but very faint throughout its length compared with what I had anticipated, and thickening downwards, like F. I was, however, astonished at the vividness of the C line, and of the continuous spectrum, for there was no prominence on the slit. I was above their habitat. The spectrum was undoubtedly the spectrum of glowing gas.

I next went to the polariscope, for which instrument I had got Mr. Becker to make me a very time-saving contrivance—a double eye-piece to a small telescope, one containing a Savart and the other a biquartz. In the Savart I saw lines vertical over everything—corona prominences, dark moon, and unoccupied sky. There was no mistake whatever about this observation, for I swept three times across and was astonished at their unbrokenness. I next tried the biquartz. In this I saw wedges, faintly coloured here and there; a yellowish one here, a brownish one there, with one of green on each side the junction, are all the colours I recollect. Then to the new attack—the simple train of prisms which, the readers of NATURE know, Professor Young had thought of as well as myself; its principle being that, in the case of particular rays given out by such a thing as the chromosphere, or the sodium vapour of a candle, we shall get images of the thing itself painted in that part of the spectrum which the ray inhabits, so to speak, we shall see an image for each ray, as if the prisms were not there. What I saw was four exquisite rings, with projections where the prominences were. In brightness, C came first, then F, then G, and last of all 1474! Further, the rings were nearly all the same thickness, certainly not more than 2′ high, and they were all enveloped in a line of impure continuous spectrum.

I then returned to the finder of my telescope, a $3\frac{3}{4}$ inch, and studied the structure of the corona and prominences. One of the five prominences was admirably placed in the middle of the field, and I inspected it well. I was not only charmed with what I saw, but delighted to find that the open-slit method is quite competent to show us prominences well without any eclipse. I felt as if I knew the thing before me well, had hundreds of times seen its exact equivalent as well in London, and went on to the structure of the corona. Scarcely had I done so, however, when the signal was given at which it had been arranged that I was to do this in the 6-inch Greenwich refractor. In this instrument, to which I rushed, for Captain Bailey had just told us that we had "*still* 30 seconds more"—which I heard mentally, though not with my ears, as "*only* 30 seconds more"—the structure of the corona was simply exquisite and strongly developed. I at once exclaimed, "like Orion!" Thousands of interlacing filaments varying in intensity were visible, in fact I saw an extension of the prominence-structure in cooler material. This died out somewhat suddenly some 5′ or 6′ from the sun, I could not determine the height precisely, and then there was nothing; the rays so definite to the eye had, I supposed, been drawn into nothingness by the power of the telescope; but the great fact was this, that close to the sun, and even for 5′ or 6′ away from the sun, there was nothing like a ray, or any trace of any radial structure whatever to be seen. While these observations were going on, the eclipse terminated for the others, but not for me. For nearly three minutes did the coronal structure impress itself on my retina, until at last it faded away in the rapidly increasing sunlight. I then returned to the Savart, and saw exactly what I had seen during the eclipse, the vertical lines were still visible!

Captain Maclear has promised to forward to you himself an account of his observations. I need only here therefore refer to their extreme value, adding what I should have stated before, that I saw the bright lines at the cusps, as he was so good as to draw my attention to them. I am however not prepared to say that they were visible through a large arc of retreating cusp.

Dr. Thomson confined his observations to the polariscope, using the Savart. He states that his observations were identical with my own.

Mr. Davis's photographic tent was below the cavalier in which our telescopes had been erected; and immediately after the observations I have recorded were over, I went down to see what success had attended his efforts. I was hailed when half-way there with the cheering intelligence "five fine photographs," and so they are, those taken at the beginning and end of the eclipse being wonderfully similar, with, I fancy, slight changes here and there; but on this point I speak with all reserve until they have been examined more carefully than the time at our disposal has permitted, and until they have been compared with those taken at Ootacamund, Avenashi, and, I hope, at Jaffna and Cape Sidmouth.

This exhausts the principal work done by the Bekul party, with the exception of the sketchers with General Selby at their head, who have recorded most marked changes in the form of the outer corona, and Mr. Webster, who was so good as to photograph the eclipse from a fort some eight miles away, with an ordinary camera, and obtained capital results.

Next a word about the Poodocottah, the other fortunate Indian party. Prof. Respighi has promised to send his results to you with this. About Mr. Holiday's labours I know nothing, except that he has obtained three sketches.

Concerning the Ceylon parties I give you a verbatim extract from the telegrams. From Jaffna: "Exceedingly strong radial polarisation, 35′ above the prominences; corona undoubtedly solar to that height, and very probably to height of 50′." From Trincomalee Mr. Moseley informs me that he carefully watched for Young's bright line stratum, and did not see it, and that 1474 was observed higher than the other line.

This is the sum total of the information which has at present reached me. It is clear there are discordances as well as agreements, the former being undoubtedly as valuable as the latter. It remains now to obtain particulars of all the observations of all the parties, before a final account can be rendered of the eclipsed sun of 1871. This, of course, will be a work of months; but if all goes well, I trust to obtain information shortly of the outlines of the work done by the Indian observers and M. Janssen, as I am now remaining in India for that purpose, and this I will communicate to NATURE by the earliest opportunity. In the meantime I hope the good people at home will think we have done our duty, and that all the members of the Government Eclipse Expedition of 1871 will soon be safely with them to give an account of their work.

J. NORMAN LOCKYER

Ootacamund, Dec. 19, 1871

CAPTAIN MACLEAR'S OBSERVATIONS

LONG before this, no doubt, you have heard of the success of the expedition, but you must be anxious to hear more of the details, and what the observations really were. When I last wrote to you from Point de Galle,* the expedition had arrived there on November 27th in the *Mirzapore*, and was about to proceed to the different stations selected. The Ceylon sections left on the 28th in the Colonial steamer *Serendib*, placed at our disposal by the Government. She was to leave Messrs. Moseley and Ferguson at Trincomalee, and then proceed to Jaffna, with Captain Fyers, R.E., Captain Tupman, R.M.A. and Mr. Moseley. We have since heard of the safe arrival of these gentlemen at their stations, and, by telegraph, of their successful observations on December 12th.

The Indian parties left Galle on the 28th in H.M.S. *Glasgow*, flag-ship of Admiral Cockburn, who kindly gave us his cabin accommodation. With a fair wind we made sail, and arrived at Beypore on the night of the 1st December. The next morning we landed Signor Respighi and Mr. Holiday to go by train to Poodocottah, and then we left for Cannanore where Messrs. Abbay and Friswell were disembarked to make their way across country to their station at Manantoddy. They had a troublesome and fatiguing journey to perform, with heavy instruments, which however they safely accomplished in three days, and we can only heartily regret that their labours were not recompensed by fine weather on the morning of the eclipse. At Cannanore we were fortunate enough to enlist the services of General Selby, commanding the troops; he came across to Bekul, and rendered good aid in making some valuable sketches of the corona during the eclipse.

We left Cannanore on the 3rd, and with the strong tide that sometimes runs up that coast, were only six hours in reaching Bekul. We found that Mr. McIvor, assistant collector, and Mr. Pringle, engineer, had arrived that morning from Mangalore, on the part of the Indian Government, had prepared the travellers' bungalow for our reception, and had cleared the keep of an old fort erected by Tippoo which would make a capital observatory. The bay is open and shelving, but there

* See NATURE, vol. v. p. 163.

was little surf, and on the morning of the 4th, instruments and all were safely landed and carried up to the fort.

Our voyage in the *Glasgow* had been uneventful ; but I cannot take leave of her without speaking of the kindness and assistance we received from Captain Jones and all on board, and we were truly sorry that the duties of the station did not allow them to remain and give us that aid, which, with the interest that all took in the work, would have been so invaluable.

Bekul is an out-of-the-way place, twenty-five miles from Mangalore, from which place all our supplies had to be carried on the backs of coolies; this did not, however, prevent several gentlemen, interested in our proceedings, coming out to join us.

Our party consisted of four who came out from England, viz., Mr. Lockyer, Dr. Thomson, Mr. Davis, and Commander Maclear, besides Messrs. McIvor and Pringle, to whose foresight and care we are very much indebted for our success. It was further strengthened by Mr. Webster, collector at Mangalore, who took some valuable photographs during the eclipse, by General Selby from Cannanore, and several others, making our numbers up altogether to eighteen. Our bungalow was about a mile from the fort, of which the highest bastion in the inner rampart had been selected to mount the equatorials ; it was in a most commanding position about eighty feet above the sea, and overlooking a vast extent of country. Just below us, in a well-sheltered spot, Mr. Davis fixed his camera and dark chamber.

The day of our landing the heat of the sun was terrible, and we had to wait till the cool of the afternoon before we could proceed to work. That night, however, a great advance was made, the bases of the equatorials were up, and all ready for the tubes, and a " chuppa," or awning of palm leaves erected to protect them from the night dews and midday sun. The next seven days were employed in getting our instruments perfectly adjusted and in practising with them. The weather left nothing to be desired, except that the sun would take his revenge out beforehand and strike down with such force as to render it impossible to work in the middle of the day. Only one morning was cloudy, and then not to an extent that would have interfered with observations. At night the stars shone with great brilliancy, and we had great delight in observing the clusters and nebulæ, pity we could not have remained longer to make spectroscopic observations of the latter in such a clear atmosphere.

The morning of the 12th dawned bright and clear, only a few small clouds to be seen near the western horizon, a light breeze from the N.E. All were early at their stations watching anxiously the appearance of the sun, which rose over the distant hills about half-an-hour before the commencement of the eclipse. But now I shall speak only of my own observations ; Mr. Lockyer has already given the account of those made by himself.

The instrument I used was a double equatorial of two 6-inch refractors mounted on the same base, one at either end of the declination axis. To one was attached a 6-prism spectroscope from Kew, lent by Mr. Spottiswoode, of great dispersive power. To the other was fixed a spindle bar, carrying an erecting eye-piece, and a 7-prism direct vision spectroscope, either of which could be swung at pleasure into the focus of the object glass ; the two tubes had been carefully made parallel, so that the same object was viewed in both telescopes. The 6-prism was worked nearly the whole of the time by myself, and the direct vision by Mr. Pringle, who had practised with it constantly during the last few days. I add the observations made by him. At the commencement of the eclipse the slit of the 6-prism was placed tangential to the point of contact, that of the direct vision radial, width such that the absorption lines were very distinct, but not too fine. No change was observed from the ordinary solar spectrum. Keeping the slit for the next quarter of an hour tangential to the northern cusp, C was very bright the whole length ; F bright, but thin. The slit was then placed radial to the cusp, and four bright lines near C (besides C itself) became visible, one on the direct side within 10 units Kirchhoff, and three on the red side within 20 units, the length of all five varying, but not together the average being about $\frac{1}{8}$ the height the visible spectrum.

At 6h. 51m. M.T., twenty-five minutes after contact, on a large prominence, C lengthened to half height of spectrum ; nine minutes afterwards cusp was at another prominence, the positions of these must have been about N. 13°, and nearly north.

At 7h. 8m. M.T. I watched with the direct vision radial and, besides the Hyd. and "near D" lines, observed another bright line a little more refrangible than the air band between *b* and F. At 1830 Kirchhoff it was very faint, and soon disappeared ; soon after this I saw F line double about the same height as usual, $\frac{1}{8}$ spectrum.

At 7h. 23m. M.T., having returned to the 6-prism radial to the cusp, I observed the Hyd. D, E and *b* very plain ; several lines then began to come into view, as near as I could judge all the iron lines from halfway between D and E to beyond *b*. These kept on brightening and more lines coming in. I called Mr. Lockyer to look at the phenomenon, and we watched it together for two or three minutes until it became time to take position to observe totality. During these two or three minutes the cusp must have passed from about N. 38° E. to N. 70° E. or further, and the lines were not lost sight of till I moved the telescope and placed the slit tangential to the point where the light would disappear, keeping it there with R.A. movement. On looking through the spectroscope the field was full of bright lines, the light just enough to let me distinguish the positions from the well-known solar lines.

As totality came on the light decreased, and the lines increased exceedingly, rapidly in number and brightness, until it seemed as if every line in the solar spectrum was reversed ; then they vanished, not instantly, but so quickly that I could not make out the order of their going, except that the Hyd. D, *b*, and some others between D and *b*, remained last. Then they vanished, and all was darkness. I then unclamped, and swept out right and left, but saw nothing ; then went to the direct vision, but saw nothing ; placed the telescope on the moon's limb by the eye-piece, then put in the spectroscope, but the light was not sufficient to show any spectrum ; pointed the telescope carefully, first on the dark moon, and then on a bright part of the corona, but no spectrum. I then looked at the corona with the naked eye, saw a bright glory around the moon, stellar form, six-pointed, something like the nimbus painted round a saint's head, extending to a diameter and a half. Looked through the finder, and saw the same form, but very much reduced in size and brilliancy; then examined with the 6in. and eye-piece, and saw nothing but a bright glow round the moon, not much more than the height of the big prominence plainly visible in the S.E. quarter. The last thirty seconds had now arrived, and, as previously arranged, Mr. Lockyer took my place at the 6in., while I again looked through the 6-prism spectroscope to record anything that might be visible, but I saw nothing. As the spectroscope was not on the sun's limb at the re-appearance of the light, I cannot state what took place.

During the remainder of the partial eclipse I watched the northern cusp as the moon uncovered the sun, and several times I saw distinctly the four bright lines near C; but saw nothing else worth recording.

The colour of the corona appeared to me a light pinkish white, very brilliant. I saw no streamers. The rest of the sky and everything around had a bluish tinge.

I will now give an extract from Mr. Pringle's report. He was observing with the direct-vision spectroscope attached to the other 6-inch telescope, and with myself watching the northern cusp, slit radial:

"Until 6h. 47m. (mean time) bright lines C, near D, and F, of uniform brightness, and varying but slightly from normal height. At that time F brightened, C remained bright, line near D very faint. At 6h. 54m. all the lines lengthened to some four or five times their normal height, showing a prominence at the cusp. For the next ten minutes lines varying but little. At 7h. 4m. a large prominence at cusp; bright lines lengthening some eight or nine times their normal height. At 7h. 4m. 30s. a bright line appeared on the more refrangible side of F, and close to it, F lengthening considerably, and bending towards the red. All the before-mentioned lines were now bright, F longer than the rest, and remaining bent, the line near it being one-third its length. At 7h. 13m. observed three bright lines at *b*, visible only at the extreme point of the cusp. Half a minute before totality, turned the slit tangential; but the slit not being exactly at the same place as that of Commander Maclear's, both refractors working by the same slow-motion screw [this was owing to the sway of the bars carrying the spectroscope when it was being turned.—J. P. M.] I failed to obtain any results at the moment of totality. I then observed at the 6-prism just quitted by Commander Maclear, whilst that gentleman, observing at the direct-vision spectroscope, swept out from the sun on one side, then brought the finder on the dark moon, and thence swept out from the sun on the opposite side. During this time nothing whatever was visible in the spectroscope. I next observed with the naked eye: corona appeared radial, of a purplish white colour, brightest near the body of the moon; no very long rays perceptible. On holding the head sideways, rays of corona remained permanent, showing none to be due to defect of vision. Next observed corona through $2\frac{1}{2}''$ finder of refractor. Structure well-defined, wavy, nebulous, permanent. Remarked a curiously-curved portion of corona, divided by a partial rift from an oblique ray. I should imagine the corona to extend about 7′ beyond the sun, but did not accurately estimate the distance whilst observing. When thirty seconds of totality remained, I went to finder of equatorial reflector; structure of corona not so apparent with higher power. Several prominences visible; one of large size, structure similar to that of corona. At about twelve seconds before end of totality, a perceptible brightening along the edge of the moon on the side of appearance; a few seconds before end of totality, I went to one prism corona spectroscope attached to $7\frac{1}{4}''$ reflector. At the end of totality a considerable number of bright lines flashed in (what proportion of the whole I cannot say, perhaps a third). The line near D noticeably bright; continuous spectrum faintly visible a moment before the sun's limb showed. After totality observed at finder, the summit of a large prominence opposite the point of sun's re-appearance visible for several seconds after totality."

During the afternoon I tried to make an accurate sketch of the prominences on the sun's disc, but clouds came on, and I was prevented. It was not worth while keeping the instruments up another day for the purpose, so we commenced, and in two days they were safely packed for Bombay.

The rumours that our presence gave rise to among the natives were very amusing. First we heard that part of the sun was about to fall, and the wise men had come to the East to prevent it. Then when the formidable-looking instruments were seen mounted on the fort, they thought there was a war, and we were engineers going to put the fort in order to prevent a landing. This was strengthened by the fact that the *Glasgow* practised at a target before returning to Ceylon. This gave place to a flood about to descend, and all the Europeans were coming to the high ground to escape it.

When the eclipse commenced the usual shouting and beating of tom-toms went on, but a cordon of police prevented an invasion of the Observatory, and only a confused noise from below reached us.

J. P. MACLEAR

S.S. *Indus*, January 6, 1872

MORSE ON TEREBRATULINA

The Early Stages of Terebratulina septentrionalis. By Edward S. Morse, Ph. D. (Boston Society of Natural History, vol. ii.)

MR. MORSE is one of the band of New England naturalists who have lately been making themselves known to us through that excellent periodical the *American Naturalist*, and who have shown themselves determined to take advantage of the opportunities offered to them by the presence on their sea-board of such zoological treasures as Limulus and Lingula. Mr. Morse obtained Terebratulina in abundance in the harbour of Eastport, Maine, and gives in this paper an account of the change in the form of the shell and the "arms" during development of this Brachiopod from a scarcely visible speck onwards. The changes are illustrated in two plates containing outline figures, and as far as Mr. Morse has observed consist firstly in the passage of the shell from a flat and shorter form to the elongated and convex shape with which we are familiar. Further, the arms were found to commence as a series of ciliated tentacles placed around the mouth, and as nearly as possible identical with the lophophor of such a Polyzoon as *Pedicellina.* At first but six of these tentacles are seen; these increase in number, whilst the lophophor takes on its horse-shoe shape; and finally by the development of the free ends of the two sides of the horse-shoe the great Brachiopodian arms are produced. This is very interesting, and confirms *a priori* notions. At the same time we must dissent from the stress which Mr. Morse lays on the affinities of structure of Brachiopoda and Polyzoa, in so far as he wishes to separate these two widely from the Mollusca, and join them to a group which he calls Vermes. The Vermes have never been accurately defined, and are in fact at present, as Carl Gegenbaur (whom Mr. Morse cites) fully admits, one of those classficatory lumber-rooms, which are so convenient from time to time in the progress of zoological science. Whilst we fully admit the close affinities of the Polyzoa and the Brachiopoda—now long recognised by all zoologists—we cannot overlook the very strong affinities of these to the true Mollusca. Even a hasty study of the embryology of the Mollusca is sufficient to bring under one's eyes larval forms of various classes bearing many of the characteristics of the Polyzoa on the one hand, and of certain Vermes on the other. The early condition of the gill-plates in some Lamellibranchs is only to be compared to the tentacula of the Molluscoidan lophophor, though presenting so large a shifting in some relations. Rather than detach the Molluscoida (with regard to the Tunicata there are a variety of new facts and considerations which require long discussion) from the Mollusca to place them in the lumber-room Vermes—we should prefer to put the whole of the Mollusca along with them there—a proceeding at present useless, but which would express a truth which Mr. Morse does not seem to admit, though it is indicated by Gegenbaur, and accepted also by Huxley, namely, that there are close genetic ties between the group Mollusca (including Molluscoida), and certain so-called Vermes, such as the Turbellaria, Archi-annelida, &c.

In a paper published prior to this, Mr. Morse has spoken of the Brachiopoda as a division of Annelida, on the ground of certain resemblances between Lingula and Annelids. We are not sure whether Mr. Morse adheres to this startling proposition, or whether it was due to the intensity of the impressions produced by his study of living Lingulæ, which must have been exceedingly interesting. By the way, we may mention that Semper has also studied living Lingula. That there is a fundamental community of organisation between Lingula and Annelids we are, as stated above, not indisposed to believe, but that this can be expressed advantageously by making the Brachiopoda a division of Annelida, or that such a classification would be anything more than reactionary exaggeration, we cannot for a moment suppose. Mr. Morse attaches importance in this regard to the setæ of Lingula, and equal or perhaps more importance to the red colour of the blood. The discovery of red-coloured blood in Lingula is interesting, because in all probability it is due, as in vertebrates and all other causes where it is really red, to the presence of hæmoglobin, and is another instance of the exceptional appearance of this chemical principle in the blood of an animal whose nearest congeners do not possess it. We should be very glad of confirmation with the spectroscope of the supposed existence of hæmoglobin in the blood of Lingula. But how can Mr. Morse suppose that this red blood, or hæmoglobin-bearing blood, is a character of the slightest classificatory importance? A great number of Annelids do not possess the vascular system at all, which in others carries this red blood; in some the fluid in that vascular system is coloured green by chlorocruorin, in others the hæmoglobin is present in the perivisceral fluid, which is in most Annelida colourless. Certain Mollusca have blood coloured red by hæmoglobin (Planorbis) as deeply and brightly as that of any lob-worm, so again have some Crustacea and Insect larvæ. The presence or absence therefore of hæmoglobin in the blood of Lingula is a matter of complete indifference as far as the relations of that animal to the Annelida are concerned.

We are much interested by a reference in Mr. Morse's paper on Terebratulina to some observations which he has made on the development of Lingula, observations which we hope before long to see published. From these he states that he is led to believe that the supposed Discina larva figured by Fritz Müller might equally as well be that of a Lingula. Some further information about this remarkable larval form will be very welcome.

Mr. Morse apologises for the undetailed character of his drawings, and for the absence of information in his paper upon the development of Terebratulina *ab ovo*—a great desideratum—by the fact that when he went to Eastport to study the development of Terebratulina he had a microscope with him which he found to be utterly inadequate to the purpose. Since this is an error which is easily remedied, we trust that Mr. Morse will soon return to the attack, if he has not yet already done so, duly armed.

E. RAY LANKESTER

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 Solar Eclipse

IT does not happen more than once in a lifetime to see such a glorious and magnificent sight as that from which I have just returned; that is, the total eclipse of the sun. I have seen many eclipses before, but never anything to equal this. I was engaged to go with the Morgans to the top of the hill to see it. Got up at six, and found it a lovely morning; rode up to Morgan's, about half a mile, carrying with me glasses, smoked glass, and sun hat. Got there before seven, and found eclipse already begun. Got our two mirrors and watched the hole in the sun grow bigger and bigger. It began from the top, and we all went off to the highest point on the hill, from whence we could see all Ooly and the mountains round. When the eclipse got so far, the cold on the mountain grew much greater, the grass was so wet that no one's boots kept it out, the feet and hands grew cold, and with your back to the sun the light over the country was like twilight, or the earliest dawn. Gradually the lower streak got thinner and thinner, until at last there shone a light like the famous lime-light, and in a moment or two that went out and the sun was totally concealed; many stars were visible, the whole country looked dark—that is, half dark, like moonlight—the crows stopped cawing, and for two minutes and a half the total eclipse lasted, a sight I shall never forget, and then the lime-light again appeared at the bottom rim of the sun, and gradually more and more of him appeared, the crows began again at once, and the cocks began to crow, the shadow now was inverted, and by degrees got smaller, until at nine o'clock the eclipse was over. I cannot but suppose that the scientific men must have had grand opportunities of observation, and that to-day's pencil will carry home many a description. Anything more beautiful, more sublime, or more perfect, it would be impossible to conceive.

Upway House, Mercara, Coorg, Ooly, Dec. 12, 1871 R. N. TAYLOR

The Rigidity of the Earth

I HAVE been urged from several quarters to defend my argument for the rigidity of the earth against attacks which are supposed to have been made upon it. It has, in fact, never been attacked to my knowledge, and I feel under no obligation to defend it. There is, I believe, a general impression that grave objections to it have been raised by M Delaunay, and it seems that even in this country some geological writers and teachers, in their reluctance to abandon the hypothesis of a thin solid crust, enclosing a wholly liquid mass, hastily concluded that all dynamical arguments against it had been utterly overthrown by Delaunay.

In point of fact Delaunay made no reference at all to the tidal argument, and clearly was unaware that I had brought it forward when he made his communication on the "Hypothesis of the interior fluidity of the terrestrial globe,"* to the French Academy, three years and a half ago, objecting to Hopkins's argument founded on precession and nutation, and merely quoting me as having expressed acquiescence. On this subject I say nothing at present, except that ten years ago, before I expressed (in my first communication of the tidal argument to the Royal Society) my assent to Hopkins's argument from precession and nutation, I had thought of the objection to this argument since brought forward by Delaunay, and had convinced myself of its invalidity. But I hope to be able on some future occasion to return to the subject, and to prove that any degree of viscosity, acting in the manner and to the effect described by Delaunay, must in an extremely short time abolish the distinction between summer and winter. My reason for writing to you at present is that I see in Mr. Scrope's beautiful book on Volcanoes (just published as a second edition) a sentence ("Prefatory Remarks," page 24), written on the supposition that the tidal argument had been brought forward for the first time at the recent meeting of the British Association in Edinburgh. I therefore take the liberty of suggesting to you that a reprint of the short abstract of my tidal argument, which appeared in the Proceedings of the Royal Society, for May 16, 1862, might not be inappropriate to your columns. I ought, however, to inform you that the tidal argument was carefully re-stated in the first volume of the treatise on Natural Philosophy, by Prof. Tait and myself, published in 1867, but as the volume is at present out of print, you may not consider this objection fatal to my proposal.

Glasgow University, Jan. 9 WILLIAM THOMSON

Abstract of Paper on the Rigidity of the Earth, by Prof. Sir William Thomson, F.R.S., received April 14, 1862

The author proves that unless the solid substance of the earth be on the whole of extremely rigid material, more rigid for instance than steel, it must yield under the tide-generating influence of sun and moon to such an extent as to very sensibly diminish the actual phenomena of the tides, and of precession and nutation. Results of a mathematical theory of the deformation of elastic spheroids, to be communicated to the Royal Society † on an early occasion, are used to illustrate this subject. For instance, it is shown that a homogeneous incompressible elastic spheroid of the same mass and volume as the earth, would, if of the same rigidity as glass, yield about $\frac{2}{5}$, or if of the same rigidity as steel about $\frac{1}{3}$ of the extent that a perfectly fluid globe of the same density would yield to the lunar and solar tide-generating influence. The actual phenomena of tides (that is, the relative motions of a comparatively light liquid flowing over the outer surface of the solid substance of the earth), and the amounts of precession and nutation, would in one case be only $\frac{3}{5}$ and in the other $\frac{2}{3}$ of the amounts which a perfectly rigid spheroid of the same dimensions, of the same figure, the same homogeneous density, would exhibit in the same circumstances. The close agreement with the results of observation presented by the theory of precession and nutation, always hitherto worked out on the supposition that the solid parts of the earth are perfectly rigid, renders it scarcely possible to admit that there can be any such discrepancy between them as 3 to 5, and therefore almost necessary to conclude that the earth is on the whole much more rigid than steel. But to make an accurate comparison between theory and observation, as to precession, it is necessary to know the absolute amount of the moment of inertia about some diameter; and from this we are prevented by the ignorance in which we must always be as to the actual law of density in the interior. Hence the author anticipates that the actual deformation of the solid earth by the lunar and solar influence may be more decisively tested by observing the lunar fortnightly and the solar half-yearly tides.* These tides, it may be supposed, will follow very closely the "equilibrium theory" of Daniel Bernouilli for all oceanic stations, and the author suggests Iceland and Teneriffe as two stations well adapted for the differential observations that would be required.

The earth's upper crust is possibly on the whole as rigid as glass, more probably less than more. But even the imperfect data for judging referred to above render it certain that the *earth as a whole must be far more rigid than glass*, and probably even more rigid than steel. Hence the interior must be on the whole more rigid, probably many times more rigid, than the upper crust. This is just what, if the whole interior of the earth is solid, might be expected when the enormous pressure in the interior is considered, but it is utterly inconsistent with the hypothesis held by so many geologists that the earth is a mass of melted matter enclosed in a solid shell of only from 30 to 100 miles' thickness. Hence the investigations now brought forward confirm the conclusions arrived at by Mr. Hopkins, that the solid crust of the earth cannot be less than 800 miles thick. The author indeed believes it to be extremely improbable that any crust thinner than 2,000 or 2,500 could maintain its figure with sufficient rigidity against the tide-generating forces of the sun and moon, to allow the phenomena of the ocean tides and of precession and nutation to be as they are.

Extract from Thomson and Tait's "Natural Philosophy."

"§ 832. . . . . All dynamical investigations (whether "static or kinetic) of tidal phenomena, and of precession and "nutation, hitherto published, with the exception referred to "below, have assumed that the outer surface of the solid earth "is absolutely unyielding. A few years ago, for the first time, "the question was raised: Does the earth retain its figure with "practically perfect rigidity, or does it yield sensibly to the de-"forming tendency of the moon's and sun's attractions on its "upper strata and interior mass? It must yield to *some* extent, "as no substance is infinitely rigid. But whether these solid "tides are sufficient to be discoverable by any kind of observa-"tion, direct or indirect, has not yet been ascertained. The "negative result of attempts to trace their influence on ocean "and lake tides, as hitherto observed, and on precession and "nutation, suffices, as we shall see, to disprove the hypothesis "hitherto so prevalent, that we live on a mere thin shell of solid "substance enclosing a fluid mass of melted rocks or metals, "and proves, on the contrary, that the earth is much more rigid "than any of the rocks that constitute its upper crust."

"§ 833 The character of the deforming influence will be "understood readily by considering that if the whole earth were "perfectly fluid, its bounding surface would coincide with an "equipotential surface relatively to the attraction of its own "mass, the centrifugal force of its rotation and the tide generat-"ing resultant of the moon's and sun's forces, and their kinetic "reactions. Thus there would be the full equilibrium lunar and "solar tides; of $2\frac{1}{2}$ times the amount of the disturbing deviation "of level if the fluid were homogeneous, or of nearly twice "this amount if it were heterogeneous with Laplace's hypotheti-"cal law of increasing density. If now a very thin layer of "lighter liquid were added, this layer would rest covering the "previous bounding surface to very nearly equal depth all round, "and would simply rise and fall with that surface, showing only "infinitesimal variations in its own depth, under tidal influences. "Hence had the solid part of the earth so little rigidity as to "allow it to yield in its own figure very nearly as much as if it were "fluid, there would be very nearly nothing of what we call tides "—that is to say, rise and fall of the sea relatively to the land; "but sea and land together would rise and fall a few feet every

* *Comptes Rendus* for July 13, 1868.

† Communicated August 22, 1862, and read November 27, of same year "Dynamical Problems regarding Elastic Spheroidal Shells and Spheroids of incompressible Liquids."

* High tide, as far as the influence of either body is concerned, is produced at the poles, and low average water at the equator, when its declination, whether north or south, is greatest, and low water at the poles and high water at the equator, when the disturbing body crosses the plane of the equator.

"twelve lunar hours. This would, as we shall see, be the case "if the geological hypothesis of a thin crust were true. The "actual phenomena of tides, therefore, give a secure contradiction "to that hypothesis. We shall see, indeed, presently, that even "a continuous solid globe of the same mass and diameter as the "earth, would, if homogeneous and of the same rigidity as glass "or as steel, yield in its shape to the tidal influences three-fifths "as much or one-third as much as a perfectly fluid globe; and "further, it will be proved that the effect of such yielding in the "solid, according as its supposed rigidity is that of glass or that "of steel, would be to reduce the tides to about $\frac{2}{5}$ or $\frac{2}{3}$ of what "they would be if the rigidity were infinite."

"§ 834. To prove this, and to illustrate this question of elastic "tides in the solid earth, we shall work out explicitly the solu- "tion of the general problem of § 696 for the case of a homo- "geneous elastic solid sphere exposed to no surface traction; "but deformed infinitesimally by an equilibrating system of forces "acting *bodily* through the interior, which we shall ultimately "make to agree with the tide generating influence of the moon "and sun. . . . ."

"§ 847. We intend in our second volume to give a dynamical "investigation of precession and nutation, in which it will be "proved that the earth's elastic yielding influences these pheno- "mena in the same proportionate degree as it influences the "tides. We have seen already that the only datum wanted for a "comparison between their observed amounts and their theoreti- "cal amounts on the hypothesis of perfect rigidity, to an accuracy "of within one per cent., is a knowledge of the earth's moment "of inertia about any diameter within one per cent. We have "seen that the best theoretical estimates of precession hitherto "made, are in remarkable acordance with the observed amount. "But it is not at all improbable that better founded estimates of "the earth's moment of inertia, and more accurate knowledge than "we yet have from observation, of the harmonic of the second "degree in the expression of external gravity, may show that "the true amount of precession (which is known at present with "extreme accuracy) is somewhat smaller than it would be if "the rigidity were infinite. Such a discrepancy, if genuine, "could only be explained by some small amount of deformation "experienced by the solid parts of the earth under lunar and "solar influence. The agreement between theory on the hypo- "thesis of perfect rigidity, and observation as to precession and "nutation, are, however, on the whole so close as to allow us to "infer that the earth's elastic yielding to the disturbing influence "of the sun and moon is very small—much smaller, for in- "stance, than it would be if its effective rigidity were no more "than the rigidity of steel."

"§ 848. It is interesting to remark that the popular geological "hypothesis, that the earth is a thin shell of solid material, "having a hollow space within it filled with liquid, involves "two effects of deviation from perfect rigidity, which could in- "fluence in opposite ways the amount of precession. The com- "paratively easy yielding of the shell must, as we shall see in "our second volume, render the effective moving couple, due "to sun and moon, much smaller than it would be if the whole "interior were solid, and on this account must tend to diminish "the amount of precession and nutation. But the effective "moment of inertia of a thin solid shell containing fluid, "whether homogeneous or heterogeneous, in its interior, would "be much less than that of the whole mass if solid throughout; "and the tendency would be to much greater amounts of pre- "cession and nutation on this account. It seems excessively "improbable that the defect of moment of inertia due to fluid in "the earth's interior, should bear at all approximately the same "ratio to the whole moment of inertia, that the actual elastic "yielding bears to the perfectly easy yielding which would take "place if the earth were quite fluid. But we must either admit "this supposition, improbable as it seems, or conclude (from "the close agreement of precession and nutation with what "they would be if the earth were perfectly rigid) that the defect "of moment of inertia, owing to fluid in the interior, is small in "comparison with the whole amount of inertia of the earth "about any diameter; and that the deformation experienced by "the earth from lunar and solar influence is small in compari- "son with what it would be if the earth were perfectly fluid. "It is, however, certain that there is some fluid matter in the "interior of the earth; witness eruptions of lava from vol- "canoes. But this is probably quite local, as has been urged "by Mr. Hopkins, who first adduced the phenomena of pre- "cession and nutation to disprove the hypothesis that the solid "part of the earth's mass is merely a thin shell."

The Kiltorcan Fossils

I HAVE just seen Mr. Carruthers' letter in your number of January 4th, to which I beg leave to reply.

In this communication it now appears that Mr. Carruthers' former remarks in the discussion upon Prof. Heer's paper were intended as a personal attack upon me; as he now states that on me alone rests the credit of misleading Prof. Heer by my erroneous determination of the Kiltorcan plant.

I have no hesitation in acknowledging to having referred the Kiltorcan plant in question to *Sagenaria veltheimiana*, and I think it very possible I may even now be correct. I will however now state the reason for my afterwards adopting Professor Schimper's name in preference. When that gentleman was in Ireland he spent some time in the examination of the Kiltorcan fossils, and did not then object to my determination of the species; it was afterwards, on my sending him a collection, that his further study of these fossils and comparison with the original species (of which I had only seen figures) enabled him to announce to me what he believed to be the distinctive characters in relation to the fruit which accompanied it, of those I had named *Sagenaria veltheimiana;* these fossils in his letter to me he referred to *Sagenaria*, and afterwards in his work "Traité Paléontologie Végétale," to *Knorria* under the name of *K. bailyana*. In the meantime I had read my report on these fossils at the British Association, and naturally adopted the generic name first applied to it by Prof. Schimper, which I afterwards corrected to *Knorria*, on his authority, in my "Figures of British Fossils," as Mr. Carruthers states.

In my letter to Professor Heer (June 1870) accompanying the specimens which I was requested to send him for his comparison with the Bear Island flora, I named those from Kiltorcan *Sagenaria bailyana* in accordance with Prof. Schimper's determination, whilst others from Tallow Bridge, co. Waterford, which he specially wished to see, I still referred to *S. veltheimiana*. I made him aware of Prof. Schimper's views on these plants, stating distinctly that they were originally referred by me to *S. veltheimiana*, but that Prof. Schimper, in consequence of his being enabled to compare the fruit accompanying it with that of the true *S. veltheimiana*, had arrived at the conclusion that it could not be that species, and therefore he had named it as a distinct one. Under these circumstances I cannot see how Mr. Carruthers can charge me with misleading Prof. Heer, who had the whole facts, with examples of the specimens from both localities, to draw his own conclusions from; with his acknowledged powers of discrimination, surely he was fully competent to judge for himself as to their correct identity.

The amount of Mr. Carruthers' knowledge on the subject about which he writes, is evidenced from his intimation that the fossil figured by me in the explanation to Sheet 187, &c., of the Irish Survey maps, is from Kiltorcan (co. Kilkenny), whereas it was sketched by me, on the spot, at Tallow Bridge (co. Waterford), where the section exposed exhibited a profusion of these plants in various conditions and stages of growth. The character of the rock in which they occur is totally different from that at Kiltorcan, the former being a grey shale, corresponding with the Lower Carboniferous shales, the latter a fine-grained greenish sandstone; neither has any of the associated Kiltorcan fossils, including the fish which are of typical Devonian or Old Red sandstone genera, ever been found at Tallow Bridge. I did however state in this memoir my belief that the *S. veltheimiana*, as identified by me at Tallow Bridge, was similar to the Kiltorcan plant in question, and also that it corresponded with the so-called *Knorria* of the Marwood beds, N. Devon.

With reference to Mr. Carruthers' announcement that *Sagenaria veltheimiana* is a "coal measure plant," I may remark that it is a particularly abundant fossil, occurring in various conditions, but seldom, if ever, met with in the typical coal series of Great Britain; I have identified it from the sandstones of the lower coal measures in the North of Ireland, as well as at various localities in the Lower Carboniferous shales of the Counties of Cork and Kerry. On the Continent, especially in Germany, it appears to be still more universal, and has been recorded under various names by fossil botanists, as Dr. H. R. Goeppert, in his "Fossile Flora der Silurischen der Devonischen," &c., mentions more than twenty synonyms for this species; moreover the same author states its occurrence to be "In der Kulmgrauwacke, dem Kohlenkalke und in der *jungsten Grauwacke*." Dr. F. Unger and Dr. H. B. Geinitz, the latter of whom personally inspected the collections from Kiltorcan and Tallow Bridge, also mentions similar lower geological horizons at which it occurs; and Dr.

W. P. Schimper in the work before cited places it in Lepidodendron as a characteristic plant "des formations houllières inférieures (grauwacke culm) correspondant au calcaire carbonifère." It is therefore evidently more characteristic of the Lowest Carboniferous than of the coal measures; the older of these formations being considered by Sir Charles Lyell "as equivalents of the Lower Carboniferous, and were even formerly referred to the Devonian group."

I believe enough has now been said to show the part I took in misleading this eminent Professor, and I will leave those interested to judge between the merits of Mr. Carruthers' or Prof. Heers' classification, but in conclusion I must request to be allowed to state that prior to this gentleman's accusation against me, he made me a proposal to help him out of his controversy with Prof. Heer, and to "join him in a memoir to describe and figure the valuable materials I had collected;" this I had to decline, because it would not only have interfered with my official duties, but might also have drawn me into a discussion in which I had no interest, besides the probability of its committing me to what may prove to be erroneous opinions.

Dublin, Jan. 10 WM. HELLIER BAILY

Circumpolar Lands

IN NATURE of December 28 there is an interesting letter endeavouring to show that the land everywhere about the North Pole down to lat. 57° is rising. We know less about the South Polar regions, but there are active volcanoes in the Antarctic Continent, and Darwin has shown in his work on volcanic islands that the land and sea-bottom are rising. This appears to be at least a remarkable coincidence.

The earth must be cooling by the escape of the central heat in volcanic eruptions and hot springs, and by slow upward conduction through the strata. As it cools it must contract. Can any mathematical reason be assigned why the contraction should be least in the direction of the polar diameter? This would account for the rising of the land at the poles. J. J. MURPHY

English Rainfall

IN NATURE of the 11th inst. your reviewer, "J. K. L." (p. 201), makes a mistake in stating that the greatest English rainfall takes place at Cockley Bridge, Seathwaite. The greatest fall takes place at the Stye and on the north side of Stye Head, Seathwaite, Borrowdale; whereas the Cockley Bridge named by your reviewer is Seathwaite, Valley of the Duddon, and many miles from the place of greatest fall. He has evidently confounded the two Seathwaites. A reference to Mr. J. G. Symons' annual rainfall returns willl show that the Seathwaite named is the one in Borrowdale. G. V. VERNON

Wanted, a Government Analyst

I AM a grocer in a small way in a country place, so that I retail almost all that comes under the name of food; and I am very desirous that all should be unadulterated and worth its price, as far as a fair profit will allow. But how am I to ensure this, even supposing I possessed the requisite knowledge and appliances? Time would be wanting to carry out a systematic analysis, and the ordinary "rule of thumb" tests are not a match for the increasing cleverness of "manufacturing chemists." It only remains to send samples to some known food analyst; but here the expense becomes a barrier, when the dealings dependent on it are on a small scale. Is there (or, if not, ought there not to be?) some Government functionary to whom samples could be sent for testing, at a charge to just cover necessary expenses? After reading a very sad article on "Artificial Milk," in your paper of Dec. 15, I feel emboldened to ask whether, either of yourself or through any of your readers, you could assist me to render practical a feeling I am sure you must sympathise with. For obvious reasons, I ask you to receive in strict confidence the name and address I have given to show the genuine nature of my application. GROCER

Earthquakes in Celebes

I WISH to contribute to the list of earthquakes and eruptions in your journal the following, all of which I have witnessed:—

1871

May 1 . Eruption of a volcano on the Island Camiguin, south of the Philippine Islands.

June 13 . Earthquake in Kakas, at the Lake of Tondano in Minahassa, North Celebes, $7\frac{1}{2}$ P.M. This shock was at the same time felt throughout Minahassa.

July 15 . Earthquake at Gorontalo, North Celebes, Bay of Tomini, $12\frac{1}{2}$ P.M. and $10\frac{1}{2}$ P.M.

,, 19 . Earthquake at Gorontalo, $12\frac{1}{4}$ A.M., heavy.

August 7. Eruption of the volcano of Ternate. This eruption had not ended August 23. Most of the inhabitants of Ternate ran away. Stones and ashes were thrown as far as Halmaheira.

,, 19. Earthquake at Gorontalo, 5 A.M.

,, 25. Seaquake at Gorontalo, 3 P.M.

,, 31. Earthquake at Gorontalo, 1 P.M., very strong, vertically.

In the month of August there were at Gorontalo a series of earthquakes, all of which I did not notice in my diary, some of them very severe, shocks so severe and numerous have not been experienced for years at that place. I do not doubt that they were in connection with the long-continued eruption of the volcano of Ternate in the same month.

Some years ago there was communicated to the Paris Academy, from South America, the fact that permanent magnets lose their magnetism during earthquakes. I will not discuss here the theoretical point of view of the question. During my whole stay in the northern part of Celebes I have always hung up a magnet, with a maximum weight attached to it, but never, not even during the severe earthquakes of Gorontalo, has the weight fallen down. I therefore doubt the fact.

Earthquakes are felt throughout the northern part of Celebes, on the coasts of the Bay of Tomini, at the Togian Islands in the Bay of Tomini; whereas in the southern part of Celebes, for instance at Macassar, earthquakes are scarcely ever felt or only very slight ones. The geological structure of the southern part of Celebes differs entirely from that of the northern.

I enclose a list of earthquakes observed at Gorontalo from 1866-70 by Mr. Riedel.

List of earthquakes at Gorontalo (N. lat. 0° 29′ 42″, W. long. 23° 2′ 50″) between the year 1866 and 1870:—

Year.	Month.	Day.	Hour.	Direction.	Direction of the Wind.
1866	February	18	1 p.m.	E.—W.	N.W.
	April	5	$7\frac{1}{2}$ p.m.	—	E.S.E.
	April	6	$10\frac{1}{4}$ a.m.	E.—W.	—
	June	20	$6\frac{1}{2}$ a.m.	E.—W.	E.S.E.
	September	5	$8\frac{1}{2}$ a.m.	—	S.E.
	December	2	$3\frac{1}{2}$ p.m.	E.—W.	W.
1867	February	26	$11\frac{1}{2}$ p.m.	E.—W.	W.
	March	22	$4\frac{1}{2}$ p.m.	—	N.W.
	March	30	9 p.m.	—	—
	April	22	10 a.m.	E.—W.	N.W.
	May	17	3 p.m.	E.—W.	S.E.
	June	26	$8\frac{1}{2}$ p.m.	E.—W.	S.E.
	July	26	8 a.m.	—	S.E.
	August	27	2 a.m.	E.—W.	S.E.
	September	14	$10\frac{1}{2}$ p.m.	E.—W.	S.E.
	December	23	10 p.m.	E.—W.	W.
1868	April	7	$9\frac{1}{2}$ p.m.	E.—W.	W.N.W.
	May	27	$6\frac{1}{2}$ p.m.	E.—W.	S.S.E.
	June	13	$9\frac{3}{4}$ p.m.	E.—W.	S.S.E.
	July	27	$11\frac{1}{2}$ a.m.	E.—W.	S.E.
	September	4	$9\frac{1}{2}$ p.m	—	S.E.
	November	18	$6\frac{1}{4}$ a.m.	E.—W.	S.E.
1869	March	3	$10\frac{1}{2}$ a.m.	E.—W.	S.E.
	May	3	7 p.m.	E.—W.	N.W.
	August	22	$9\frac{1}{2}$ p.m.	E.—W.	S.E.
	November	17	$4\frac{1}{2}$ p.m.	E.—W.	W.N.W.
1870	April	7	$12\frac{1}{2}$ a.m.	E.—W.	W.S.W.
	July	12	$5\frac{1}{2}$ a.m.	E.—W.	W.S.W.
	August	28	$3\frac{1}{2}$ a.m.	E.—W.	W.S.W.

I am now going to the southern parts of the Philippine Islands, and in the following year to New Guinea. A short communication about my travels in Celebes will be found in *Petermann's Geographische Mittheilungen.*

Macassar, Celebes, Nov. 10, 1871 A. B. MEYER

ELECTROPHYSIOLOGICA:

SHOWING HOW ELECTRICITY MAY DO MUCH OF WHAT IS COMMONLY BELIEVED TO BE THE SPECIAL WORK OF A VITAL PRINCIPLE

III.

2. *In continuation of the question—How in muscular action electricity may do much of what is commonly believed to be the work of a vital principle.*

CONNECTED with the history of electrotonus as exhibited in these experiments* are also other facts which must not be overlooked in this attempt to trace out the workings of electricity in muscular action—facts which show that the departure of contractility and the arrival of rigor mortis are considerably retarded by both forms of electrotonus. Left to itself, the gastrocnemius of the frog loses its contractility and passes into the state of rigor mortis in a time varying with the season and from other causes from 6 to 12 hours; but not so when left to the action of electrotonus. In this latter case, indeed, the contractility may remain for 18, 24, or 36 hours—for a longer time in anelectrotonus than in cathelectrotonus—and even then there may still be no signs of rigor mortis. Once, where anelectrotonus was kept up steadily all the time, and where contractility lingered for 36 hours, the muscles were still limber at the end of 48 hours. No doubt, before exact conclusions can be drawn in these matters more experiments are wanted, many more; but it is not necessary to wait for these in order to be certain that the departure of contractility, and the arrival of rigor mortis, are considerably retarded by the action of both forms of electrotonus. And it is simply to the bare fact that attention is now directed.

What then? Do these facts bear upon what has gone before, and, if so, how?

The facts are obvious. In anelectrotonus and cathelectrotonus alike there are—suspension of the tetanus caused by feeble faradaic currents, elongation of muscle, exalted contractility, together with considerable retardation in the time at which contractility passes off and rigor mortis comes on. In anelectrotonus and cathelectrotonus the parts, muscle and nerve alike, are charged with a charge larger in amount than that which is natural to them—a positive charge in anelectrotonus, a negative in cathelectrotonus. The facts, indeed, are strangely in keeping with the premises. Only let it be supposed that the artificial charge acts upon the dielectric sheaths of the fibres as the natural charge has been supposed to act, but in the contrary direction, that is from without to within instead of from within to without, the charge imparted to the outside inducing the opposite charge on the inside, and all the rest follows. The artificial charge is larger in amount than the mutual charge, and hence the increased elongation of the muscular fibres, the compression arising from the natural attraction of the two opposite elements of the charge keeping up a state of elongation proportionate to the amount of the charge. Hence, also, the suspension of the tetanus by electrotonus, for if the charge elongates the fibres it is easy to see that another of its actions may be that of suspending or antagonising muscular action. And hence again the increased contractility, for, according to the premises, contraction, happening under these circumstances, will be greater because the elasticity of the muscle has freer play at the discharge. In these matters the artificial charge plays the same part as the natural charge, only more energetically, nothing more. And not less so, as it would seem, in the action exercised upon the passing off of contractility and coming on of rigor mortis. Contractility passes off and rigor mortis comes on in the ordinary course of things, because the muscle loses its natural electricity. Contractility passes off and rigor mortis comes on more slowly in electrotonus because the artificial charge associated with this state can take the place and do the work of the natural charge. This is all. Indeed, so far, the whole electrical history of muscle would seem to point to the view which led to the experiment with the elastic band, and to show that living muscle is kept in a state of elongation by the presence of an electrical charge, and that contraction is nothing more than the action of the fibres, by virtue of their elasticity, when liberated by discharge from the charge which kept them elongated previously—ordinary muscular contraction differing from rigor mortis in this only, that the charge which prevents contraction is suddenly withdrawn, and immediately replaced, in the former case, and gradually withdrawn, and not replaced, in the latter case.

Upon this view, also, it is possible to get a glimpse of the reason why contraction is more antagonised by anelectrotonus than by cathelectrotonus; and why contractility is slower in passing off, and rigor mortis slower in coming on, under the former state than under the latter. In anelectrotonus the artificial charge of the parts, muscle and nerve alike, is positive, and, being so, the sheaths are positive externally, and (by induction) negative internally, the manner of charging, which, there is reason to believe, is natural to the muscle. In cathelectrotonus, on the other hand, the opposite state of things obtains. Here the artificial change is negative, not positive. Here, consequently, the charging of the sheaths is negative on the outside and positive on the inside—a state of things which is not natural to the fibres, or which is only met with exceptionally, when these fibres are upon the point of passing into the state of rigor mortis. In anelctrotonus, therefore, the natural charge may co-operate with the artificial charge in a way in which it cannot do in cathelectrotonus; and which, without further comment, it is easy to see may explain in some degree why contraction is more antagonised by anelectrotonus than by cathelectrotonus; and why contractility passes off and rigor mortis comes on more slowly under the former condition than under the latter.

As I have shown elsewhere,* the whole electrical history of muscle is in keeping with this view. The charges obtained from the common friction machine act in the same way as those associated with electrotonus. Everywhere, the question is not of polarisation and of changes in direction of a continuous current, but simply of charge and discharge. Everywhere it is charge preventing, and discharge permitting, action. In a word, the whole electrical history of muscle would seem to show that electricity may have much to do in what is commonly believed to be the work of contractility and tonicity, and that the way in which this work is done is that which is here pointed out.

Against this view, however, sundry objections may be urged. It may be said that the phenomena of muscular action in muscles with sheathed fibres cannot be explained after this fashion. It may be said that the proof of charge during rest and discharge during action is little more than a matter of imagination. It may be said that the force of the natural electricity of muscle is inadequate as force. But, in reality, these objections, when fairly looked into, prove to be of little value.

No doubt the fibres of involuntary muscles differ from those of voluntary muscles in having no proper sheaths. Instead of having those sheaths, indeed, they are made up of cells, mostly fusiform in shape, imbedded in a sort of homogeneous plasm or matrix; and these cells, there is reason to believe, are the contractile elements of the fibres. Still it is not easy to allow the force of any objection arising in this fact, for may it not be that the walls of these contractile cells, which, like the sheaths of the fibres of voluntary muscle, in the main consist of the material of elastic tissue, behave in the way the sheath is supposed to behave under the charge and discharge, that a charge developed on the

* See NATURE, Jan. 11, 1872.

* "Dynamics of Nerve and Muscle." Macmillan.

inside of these walls induces the opposite charge on the outside, that the walls elongate under the compression arising from the mutual attraction of these charges, and shorten when this charge is discharged, because their elasticity is then left free to come into play? Nay, may it not be that this action of the cell membrane is not excluded in those long voluntary muscles in which the fibres seem to be made up of several cells or fibres over-wrapping at their ends, rather than of a single sheathed fibre? And, certainly, this idea is not contradicted by facts remaining in the background; for, as will be seen in due time, these go to show that the walls of all cells and fibres are affected electrically in the same way as that in which the sheath of the fibre of voluntary muscle is supposed to be affected. So that, after all, the phenomena of rest and action in sheathless muscular fibres may supply no valid objection to the view which has been taken of these phenomena as presented in muscular fibres with proper sheaths.

And surely the evidence supplied by the new quadrant electrometer is a sufficient contradiction to the objection that the charge during muscular rest and the discharge during muscular action are mere matters of imagination, for this evidence shows unequivocally that there is a charge during this state of rest and a discharge during this state of action. It is not a question of inference merely, such as it might be if the evidence supplied by the galvanometer were alone available; for here, as has been pointed out, the current during rest, and the comparative disappearance of this current during action, may in reality point to charge and discharge when traced to their causes: it is a question of simple fact. Moreover, the anatomical and physiological analogies existing between the muscular apparatus and the electrical apparatus in the torpedo and the phenomena of secondary contraction, make it more than probable that muscular action is accompanied by a discharge analogous to that of the torpedo. Like the nerves of the muscle, the nerves of the electric organs originate in the same track of the spinal cord, and terminate in the same manner. Like the muscles, the electric organs are paralysed by dividing their nerves. Like the muscles, the electric organs, after being thus paralysed, may be made to act by pinching the nerve below the line of section. Like the muscles, the electric organs are thrown into a state of involuntary action by strychnia. Like the muscles, the electric organs cannot go on acting without intervals of rest. And lastly, the nerves of the electric organs, like the nerves of the muscles, when somewhat exhausted, respond in the same curiously alternating way to the action of the "inverse" and "direct" current, if only discharge be taken as the equivalent of contraction. In a word, these analogies may be said almost to necessitate the conclusion to which Matheucci was led in regarding them, namely this—that muscular action is accompanied by a discharge of electricity analogous to that of the torpedo. And certainly this conclusion is borne out rather than contradicted by the phenomenon of secondary contraction which is exhibited in a prepared frog's leg, when, after laying its nerve upon the muscle of another such limb, contraction is produced in the latter limb; for here the only sufficient explanation would seem to be that offered by Becquerel, namely this—that contraction happens in the first limb because its nerve is acted upon by an electrical discharge developed in and around the muscles of the second limb during action—a discharge which may not indirectly show that there was a charge to be discharged during the previous state of rest. In a word, the evidence, direct and indirect, must surely suffice to show that the idea of charge during rest and discharge during action is something more than a mere matter of imagination.

Nor can it be fairly urged that the force of the natural electricity of the muscle is too feeble to produce the results attributed to it. On the contrary, after what has been said respecting the analogies between muscular action and the action of the electrical organs of the torpedo, it is quite fair to suppose that the force of the discharge in muscular action, instead of being feeble, may be equivalent to that of the torpedo; and that the reason why it cannot be detected in the same way may be that it is short-circuited, and so mainly out of reach, within the body.

3. *How in nervous action electricity may do much of what is commonly believed to be the work of a vital principle.*

There is good reason to believe* that the electrical law of nerve-fibre differs in no wise from that of muscular fibre.

There are also similarities between the principal structural elements of the nervous system from which it would appear that what holds good of one part of this system electrically may hold good of the other parts also. Nay more, there is in these facts reason for believing that what holds good of nerve-tissue generally may hold good of muscle also, for the typal element of nerve and muscle is evidently one and the same.

Looking at the different parts of the nervous system—ganglionic cells, and the peripheral nerve-organs—and at muscle cells and fibres, it is easy to trace the same structural plan.

Central ganglionic cells, as seen in the ganglia of the sympathetic system, and in other small ganglia of the kind, consist of a round, oval, or pyriform mass of soft translucent, granular substance, with which two or more nerve-fibres communicate, and of an enclosing capsule formed of a transparent membrane with attached or embedded nuclei. The central granular substance, with which the nerve-fibres communicate, and the investing capsule, are unmistakeable in the ganglionic cells of the minute ganglia, but not so in the brain and spinal cord. In the brain and spinal cord there is the same central substance, but the proper cell wall is doubtful. Moreover, the central substance, instead of being a round, oval, or pyriform mass, with which the nerve-fibres are connected at one point only, branches out into several processes, which seem to be continuous with the nerve-fibres. At the same time, these cells and fibres are surrounded and supported by connective tissue, called reticulum by Kölliker, and neurologia by Virchow—a tissue which, as Dr. Sharpey points out, "is not merely an open mesh-work, but consists of fine laminæ formed of a close investment of finest fibrils, disposed as membranous partitions and tubular compartments for supporting and enclosing the nervous bundles;" so that, in the brain and spinal cord, as in the smaller ganglia, there is good reason for believing that the structure of the ganglionic cell is virtually the same, namely, a central granular mass, with which nerve-fibres are connected, and a membrane, with nuclei, investing this mass.

The peripheral nerve organs, of which the principal forms are three in number—the end-bulbs, the touch-corpuscles, and the Pacinian bodies—agree in having (1) an inward part or core of soft, translucent, finely granular matter, in which one or more nerve-fibres end by bulbous, or knobbed extremities; and (2) an outer investing capsule of ordinary connective tissue, with nuclei. In the end-bulbs and touch corpuscles this capsule is simple; in the Pacinian body it is made up of many concentric layers, from forty to sixty in number, with nuclei, these layers, "encasing each other, like the coats of an onion, with a small quantity of pellucid fluid included between them," being strung together where the nerve passes through. The structural plan is still that of the ganglionic cell—a central mass of granular matter, with which nerve fibres are intimately connected, and an investing capsule, simple or complex, as the case may be; and this would seem to be the plan of all the peripheral parts of the nervous system without exception, for it is a question

* See NATURE, Jan. 4, 1872.

whether nerves do ever *terminate* in plexuses or meshes of any kind.

The fibre of voluntary muscle is said to consist of a large number of extremely fine filaments enclosed in a transparent, homogeneous, elastic (the composition agrees with that of elastic tissue), tubular sheath, called the sarcolemma or myolemma, in which are nuclei, called muscle-corpuscles. It might, however, be more correct to say that this fibre consists of a mass of soft granular matter (the granules being the *sarcous elements* of Bowman), agreeing in the main with the granular core of the ganglionic cells and peripheral nerve-organs, enclosed in the sheath which has been described; for the contents of the fibre, instead of splitting up longitudinally into filaments, may split up horizontally into discs—may split either way or any way, in fact, as they would do if they were made up, neither of fibrils nor discs, but of granules which may, as it happens, aggregate into fibrils or discs. The fibre of involuntary muscle, on the other hand, is made up of elongated fibre-cells, connected together by a homogeneous, transparent uniting medium, without any sarcolemma. Each of these fibre-cells has a core of finely granular matter, sometimes arranged so as to form imperfect fibrils, and of a distinct cell-membrane, with nuclei, the shape of the cell being fusiform, with ends sometimes pointed, sometimes truncated, sometimes simple, sometimes branched. The cell-membrane in reality takes the place of the sarcolemma, for each cell is nothing more or less than a rudimentary fibre. Indeed, in long voluntary muscles there are fibres which seem to partake somewhat of the character of voluntary and somewhat of the character of involuntary fibres—fibres which, instead of running continuously from one end of the muscle to the other, are made up of several elongated fusiform cells, overlapping each other at the ends, and which therefore may consist of cell-membrane and sarcolemma both. Nor is the connection of the nerves with the muscular fibres or cells peculiar. Beale and Kölliker think that the nerves belonging to voluntary muscle end in meshes of pale fibres outside the sarcolemma. Rouget, Kühne, and others are of opinion that this ending is in peculiar organs—motorial end-plates continuous with the axis-cylinder of the nerve, oval or irregular in shape, within the sarcolemma and between it and the proper muscular substance, the primitive nerve-sheath fusing with the sarcolemma, and one end-plate being devoted to each muscular fibre. And thus it may be that the muscular fibre or cell may agree in structure with the ganglionic cell, and the peripheral nerve organ, in having a soft granular core, with which one or more nerve-fibres are connected, and an investing membrane of connective tissue with one or more nuclei. It may be, indeed, that the muscular fibre and cell are only varieties of the peripheral nerve-organ.

The nerve-fibres by which these several bodies—ganglionic cells, peripheral nerve organs of various kinds, and muscular fibres and cells—are connected together, are of two kinds, the tubular, which are white with dark borders, and those which are grey, pale, non-medullated or gelatinous. The white or tubular fibres, when quite fresh, appear perfectly homogeneous like threads of glass, but afterwards, when coagulation has taken place, they are found to consist of an axis, or primitive band, as it is called, a white medullary coating strongly refractive of light, and giving them the appearance of having dark borders, and an outer membranous sheath or tube, with nuclei in it, agreeing in composition with elastic tissue, and being analogous to the sarcolemma. The grey, pale, gelatinous fibres would seem to consist of the axis or primitive band of the others, with obscure sheaths in which are nuclei, but without medullary coating. They belong chiefly to the ganglionic system, but not exclusively; at all events the finer subdivisions of the white dark-bordered nerves of the other systems are found to have lost their dark borders, and to have become undistinguishable from those which have no dark borders naturally. In nerve-fibres, therefore, as in nerve-cells, there would seem to be a central core, and a membranous investment containing nuclei; and, all things considered, the connection of these fibres with ganglionic cells, with peripheral nerve-organs, and with muscular fibres and cells, would appear to be by one and the same method, the axis or primitive band being continuous with the central soft granular core of the central and peripheral elements of the nervous system, and of the muscular fibres and cells (for with so many points of analogy it is difficult not to believe with Rouget, Kühne, and others who agree with them in this matter), the primitive sheath, when there is one, being continuous with the membranous investment of this core, neurilemma, sarcolemma, or other, as the case may be.

Instead of being peculiar, therefore, the voluntary muscular fibre may be no more than a modified form not only of the contractile cell of the involuntary muscular fibre, but also of the nerve-fibre, and of the central and peripheral cell-elements of the nervous system. The same type of structures is to be traced out in each case. There is in each case the same central, granular, soft, substance, but slightly changed protoplasm probably, in the molecular change of which an electrical change may originate. There is in each case outside this central substance a membrane which may become charged leyden-jar-wise as the neurilemma and sarcolemma are supposed to be charged. And, therefore, it is not altogether begging the question to conclude that in each case one and the same electrical law may bear rule.

And certainly the adoption of this idea is calculated to elucidate much that is obscure in the structure and action of the nervous and muscular systems.

Upon this view a use is found for the contents and walls of the fibres and cells of which the nervous and muscular systems are made up. The contents are wanted for the generation of the charge; the walls are wanted for receiving and holding this charge. Their leyden-jar office, indeed, explains why it is that the nervous and muscular systems should be made up of cells and fibres.

Upon this view one use is found for the nucleus in the walls or sheath of cell or fibre. The nucleus may represent the spot at which the development of this wall or sheath is arrested—the spot at which the original, moist, *conducting* protoplasmic matter is not transformed by drying, or in some other way, into *non-conducting* wall or sheath, and, therefore, as I think, the nucleus may have a very definite function to fulfil. As I think, indeed, the case may be this: that the molecular changes in which the charge of the cell or fibre originates (those in the contents of the cell or fibre) depend upon the continual ingress of fresh and egress of used-up aërated matter; that this ingress and egress is, not through the wall or sheath anywhere or everywhere, but only through the nucleus; that the one charge not wanted for charging the inner surface of the wall or sheath may escape to earth through the nucleus; and that the channel of the discharge which happens when the cell or fibre passes from the state of rest into that of action may also be through the nucleus. Without such opening as may be supposed to exist in the nucleus, indeed, it is difficult to understand how the cell or fibre should be charged and discharged; and thus, upon the view in question, a use is found (not the only use, of course), for the nuclei present in the walls of the cells and in the sheaths of the fibres of the nervous and muscular systems.

Upon this view, too, the infinite number of these cells and fibres may in some degree be accounted for. For may it not be that each cell and fibre acts as a condenser to every other cell or fibre, so that a charge or discharge which is feeble without being multiplied becomes anything but feeble when multiplied? And may not this function of a condenser be the one function of the Pacinian bodies?

Other cells and fibres have other functions as well; these bodies may have this one function only. They may, in fact, be rudiments of the electric organs of the torpedo, with a sphere of action, not without the body, but within it. And this may be the reason why these bodies are placed on the trunks of nerves at points where it may be supposed that special means are wanted for keeping up the requisite degree of elastic tension, their use in this case being analogous to that of an ordinary leyden condenser in connection with a telegraph wire conveying a minimum amount of electricity.

Nor does this view fail to elucidate in some degree the way in which nerves tell upon muscle and react upon each other. Let the contents of the muscular fibre or cell be connected with the contents of the corresponding ganglionic cell by the axis cylinder of the nerve, and a charge or discharge in the nerve centre must tell upon the muscular tissue, just as in the case of two leyden jars with their inner coatings connected by a conductor, the charge or discharge of the one involves corresponding changes in the other. Let the case be that of a sensory peripheral cell and a central ganglionic cell, similarly connected, and a charge or discharge in the former will involve a charge or discharge in the latter, the discharge producing sensation. The case is simply that of a leyden battery, with all possible space economised by making the conductors, where they may, do the work of the jars. The case is plain as regards the charge, for the molecular charges are ever at work by which it is kept up and renewed; and the case is not altogether obscure even as regards the discharge, for it may well be that discharge happens when the charge increases until it overleaps the barrier of insulation presented in the dielectric walls of the fibres and cells—a result which, for want possibly of a sufficiently insulating barrier somewhere, happens more easily than it ought to do in the case of involuntary nervous action, such as is seen in convulsion, neuralgia, and the rest.

Viewed in this way, too, it is easy to see that the nervous system may do its work, not by discharge only, but by charge also. It is easy to see that the discharge may be all that is wanted to cause contraction; indeed, according to the premises, all that is wanted for this purpose is that the charge which kept the muscular fibre elongated should be discharged, and the fibre so left to the play of its own natural elasticity. It is easy to see, also, that discharge may be the mechanical agent which may call the various nerve-centres into action—by shaking the veil which separates the visible from the invisible in the higher mental processes, perhaps. And for charge no less than discharge it is also easy to see that there may be a definite work to do—a work of which the end is, not to cause action in the muscles and in the various nerve-centres, but to prevent it. Indeed, after what has been said, it is to be supposed that all nerves, through their electricity, have during rest an action which Pflüger supposes to be peculiar to certain nerves only, and to which he gives the name of *inhibitory*.

And here opens out a question of paramount interest.

It has been seen that the electric law of nerve and muscle is one and the same. It has been seen that the state of contraction in muscle is antagonised by the presence of a charge of electricity in muscle—that a state of actual elongation is produced by the action of this charge. It has been seen, not only that the state of contraction is antagonised and a state of elongation set up by the presence of the natural charge of electricity in muscle, but that more marked changes of the same kind are produced by the action of an artificial charge of electricity, provided this charge be greater in amount than the natural charge. The facts, indeed, are calculated to justify the notion that the degree of elongation produced by the conjoint action of the charge belonging to the muscle itself and the charge imparted to the muscle from its nervous system is greater than that produced by the action of the former charge singly; or, in other words, that the charge imparted to the muscle by its nervous system may cause a degree of elongation in the muscle which is over and above that caused by the charge belonging to the muscle itself—which surplus may have much to do in explaining rhythmical action in hollow muscles.

Take the case of a hollow muscle—a capillary vessel, for example. This vessel has its special nervous system, vasomotor nerves, efferent and afferent, vasomotor centre; and the question is as to how this system acts upon the vessel. May it be that a charge of electricity is continually being developed upon the cell-walls and fibre-sheaths of this system by the action of the oxygen of the blood and other causes upon the contents of the cells or fibres; and that this development goes on until, the bounds of insulation being overpassed, discharge happens? May it be that the muscular fibres forming the walls of the vessel elongate, and in so doing cause the vessel to dilate as long as this charge is imparted to them? May it be that the vessel passes from the state of dilatation into that of contraction when the discharge of this charge happens, in consequence of the muscular fibres being then liberated from the condition of extra-elongation caused by the charge imparted to them from the nerves, and so left to the play of their natural elasticity? May it be that thus there are diastolic and systolic changes in the vessel by which the blood is alternately drawn into and driven out of the vessel, changes which may supply the key to the mystery of "capillary force"? Nay, more; may it not be that the diastolic and systolic movements of the heart itself may have to be explained in the same way? To all these questions I answer, unhesitatingly, yes, it may be so. Indeed, after what has been said, the only explanation which seems to be called for concerns the movements of the auricles of the heart, and this is easily given: for, as it seems to me, the auricles must be looked upon chiefly as cisterns formed of dilated veins, and their movements chiefly as passive consequences of the movements of the ventricles, the systole of the auricles being little more than the passive falling-in of the auricular walls upon the blood being suddenly sucked away by the ventricular diastole, the diastole of the auricles being little more than the passive bulging-out of the auricular walls, caused at one and the same time by the stream of blood which is ever flowing in from the valveless openings of the great veins, and by a forcing back of this stream, consequent upon the sudden closure and recoil of the auriculo-ventricular valves at the moment of the ventricular systole. In this way the seemingly diastolic and systolic movements of the auricles must alternate with the true diastole and systole of the ventricles, and, at the same time, the absence of valves at the opening of the great veins into the auricles is accounted for—an absence altogether inexplicable if the auricular systole had to play the *active* part in the circulation which is played by the ventricular systole. And much to the same effect may be said of rhythmical movements in other hollow muscles, the chief difference between one such movement and another being perhaps this—that contraction follows upon dilatation more slowly in consequence of the cell-walls and fibre-sheaths of the special nervous systems being constructed differently as regards the capacity for quick charging and discharging; but these hints must suffice for what might be said upon this subject.

Nor can it be urged as an objection to this view of nervous action—the only objection which may be urged, so far as I know—that the state of action in nerve-fibre is unattended by the contraction which attends upon action in muscular fibre. The electrical law of nerve and muscle being one and the same, it might be expected, perhaps, that this particular difference should not exist; but this difficulty, if it be one, is soon disposed of. Thus, the success of the experiment with the elastic band depends upon the band being of a certain thickness, and upon the weights being so adjusted as to balance without overbalancing its elasticity. Failing these conditions charge and discharge may not tell in causing elongation and contraction. And, therefore, the absence of perceptible elongation and contraction in the nerve-fibre under the charge and discharge may be simply owing to the fact that the thickness and stretching of the neurilemma have not been adjusted for the production of these results. Besides, it is by no means certain that there are not in some nerve-fibres slight changes which are strictly parallel to the elongation and contraction witnessed in muscular fibres.

In a word, there seems to be good reason for believing that in nerve as in muscle electricity may have to do much of what is commonly regarded as the special work of an inherent vital principle.

4. *How in maintaining the "tone of the system" electricity may have to do much of what is commonly regarded as the special work of a vital principle.*

After what has been said little remains to be added under this head. The conclusion arrived at is that each perfect fibre and cell of living muscle and nerve (and, by implication, every living fibre and cell), is a charged leyden-jar while at rest. It is that the membranous portion of the fibre or cell is at this time compressed by the mutual attraction of the two opposite charges disposed leyden-jar-wise upon its two surfaces. It is that the effect of this compression is to elongate the fibre or cell by squeezing out this membrane lengthwise. What then? May it be that this compression, this squeezing out, is sufficient to account for what is called the "tone of the system"? This state, no doubt, is indefinite enough, but it becomes more definite when viewed in this way—so definite, in fact, that here also, in the maintenance of the "tone of the system" that is to say, electricity may have to do much of what is commonly believed to be the work of a vital principle.

5. *How in certain processes of growth electricity may do much of what is commonly regarded as the special work of a vital principle.*

A cell or fibre is at first a mass of protoplasm without any investing membrane. Later, this membrane makes its appearance, and how is this? Is it that the surface of the protoplasmic mass, except at the part or parts where the nucleus is afterwards met with, hardens by desiccating, or dying, or changing in some other way, and, so hardening, acquires dielectric properties? Is it that the molecular changes ever going on in the protoplasmic matter beneath this crust, develope a charge on the inside of this crust, which, acting inductively, leads to the development of the opposite charge on the outside? Is it that the compression arising from the mutual attraction of these opposite charges, causes the crust to stretch out every way, and so separate from the underlying protoplasmic mass, leaving thereby in some instances a vacuole, which may be filled with a thin liquid or even air? Is this the way in which the sarcolemma and neurilemma, the cell-walls, and all membranes more or less analogous to them, may be formed? After what has been said such an idea is by no means improbable. Nay, such an idea may be looked upon as the natural consequence of the premises. And if so, then electricity may have to do much of what is commonly believed to be the work of a vital principle in these phenomena of growth, as well as in the various processes which have been already passed in review, and upon which so much has been said as to leave only room now for these bare hints of what might be said upon the subject.

C. B RADCLIFFE

MERCURY PHOTOGRAPHS

AN entirely novel method of photographic printing has just been discovered by M. Merget of Lyons. Although akin in some respects to the daguerreotype process, it differs essentially therefrom in the fact that exposure to light is not necessary to the formation of every separate image. It is difficult indeed just now to apply any distinguishing name to M. Merget's invention, for the methods hitherto discovered—and the number of these has, we all know, increased of late beyond all calculation—are all of them divisible into two very distinct classes. Thus we have those processes broadly termed chemical, in which every print is secured by the aid of light, as for instance, the nitrate of silver and carbon methods; and those again where a matrix, or printing block, having been prepared, the copies are struck off in the ordinary lithographic or printing press; photographs prepared in this latter manner are usually termed photo mechanical prints. M. Merget's invention partakes singularly enough of the nature of both classes; for while the prints are undoubtedly formed by chemical action, the question of light is of no moment at all, and the manipulations involved are to some extent of a mechanical nature.

The experiments of Faraday upon the diffusion of gases will be remembered by many, and it was the results arrived at by that distinguished philosopher that incited M. Merget, the Professor of Physics at the Faculté des Sciences of Lyons, to take up the investigation he has so successfully carried through. Faraday had already found out that the vapour of mercury acted very sensibly upon gold-leaf, and the first task undertaken by M. Merget was to discover whether this same action also took place upon other metals or their compounds. The investigation, it should be stated, was designed to be of a purely theoretical nature, and was not undertaken, in the first instance at any rate, with a view of working out any practical processes such as may eventually result from the research. The principal points discovered by M. Merget may be thus summarised:—

1. The vaporisation of mercury is a continuous phenomenon; that is to say, the metal emits vapour at all times, even at a very low temperature, and when in a solidified form.

2. Mercury vapour may be condensed upon certain substances, such as carbon, platinum, &c., without these latter being chemically affected.

3. Mercury vapour will pass with exceeding facility through porous bodies, such as wood, porcelain, &c.

4. The salts of all precious metals when in solution are very sensitive to the action of mercury vapour, which has the effect of rapidly reducing them.

The most sensitive to mercury of the precious metal salts are nitrate of silver and the soluble chlorides of gold, palladium, and iridium, and paper prepared with any of these forms at once a most delicate test for the volatile metal; but the solutions must contain some hygrometric body to prevent complete desiccation, so that the surface coated with them will always remain in a moist condition. To demonstrate how exceedingly sensitive this test-paper is to mercury, we may state that its contact with any body containing but a slight trace of amalgam suffices to darken the surface, while it is affirmed that any workman who has been employed for some time in a looking-glass or other similar factory, may produce an impression of his hand by simply laying the same upon a sensitive surface of this kind, the minute traces of mercury in the pores of the skin being amply sufficient to bring about a reduction of the salt, and to produce consequently an imprint of the fingers. In the same way a section of wood exposed to mercury vapours, and afterwards pressed in contact with a sheet of sensitive paper, prints off upon the surface all the rings and markings it possesses, the mercury being deposited in the pores of the wood in a more or less condensed form.

In the event of nitrate of silver being used for preparing the paper, it is necessary, obviously, to exclude the light, as otherwise a reducing action will be already set up by solar means alone, but with the salts of palladium or platinum no such action need be feared. According to the kind of metallic salt employed, so the tint of the impression varies, but in most cases an intense black may be obtained where the action has proceeded far enough.

Having described M. Merget's discoveries thus far, it is easy to guess how that gentleman employs them in the carrying out of a photographic process. An ordinary glass negative, possessing an image which has been formed by the deposition of silver particles, is prepared in a suitable manner to protect it from injury by contact with the mercury (such, for instance, as coating it in some way with platinum or carbon particles), and the picture is then exposed to the action of mercury vapour. The vapour condenses, in a more or less concentrated form, upon the image—in the same way, pretty well, as it becomes deposited upon, and develops, the latent image in the daguerreotype process—and subsequently the plate thus treated is brought into contact with the sensitive paper. The consequence is that the minute particles of mercury deposited all over the image exercise a reducing action upon the salts on the surface of the paper, and a print results of the original photograph, possessing the same gradation of tint as the original. Indeed, when nitrate of silver is employed for sensitising the paper, the photograph secured is in every respect similar to that produced by light in the ordinary silver printing process, and the picture is forthwith toned and fixed in the same way, in fact, as one of these; in the one case, however, the reduction of the silver salts has been brought about by mercury vapour, while in the other light alone has been the reducing agent. Impressions obtained by means of platinum and palladium salts need simply to be washed in water in order that they may be permanently fixed. These latter, in truth, are so indestructible and inalterable that they cannot be destroyed except by a chemical agent which would at the same time radically injure the paper, or other basis, upon which they rest.

This process of photography is not yet in such an advanced state as to be of any practical importance; but, nevertheless, it is certainly one of the most ingenious and interesting discoveries made of late in this branch of Science. The great advantage it possesses is that of printing without the aid of light, and yet producing prints with detail and half-tone dependent upon delicate chemical reaction—such rare gradation being secured as our present light-printed pictures (silver and carbon prints) alone possess. A mechanical printing process could, of course, easily be worked out from these data, if considered desirable; and, indeed, it is by no means improbable that this will be the most successful way of applying the discoveries in a practical form. But even in the event of no practical use at all being made of the process—for this is indeed questionable—the research, regarded from a purely scientific point of view, is deserving of the highest eulogium.

H. BADEN PRITCHARD

NOTES

IN another column will be found full details of the observations of the Total Eclipse of December 12, made at Bekul, by Mr. Norman Lockyer and Captain Maclear. In future numbers we hope to give similar reports from the observers at the other stations. The weather was very favourable at all the stations, with only one exception.

M. JANSSEN writes as follows to the French Academy of Sciences, under date Sholoor, Neelgherry, 12th of December, 1871, 10 A.M.:—"I have just observed the eclipse, only a few moments since, with an admirable sky, and whilst still under the emotion caused by the splendid phenomenon of which I have just been a witness, I address a few lines to you by the Bombay courier, who is to start instantly. The result of my observations at Sholoor indicates without any doubt the solar origin of the corona, and the existence of matter beyond the chromosphere." And in a letter to M. Faye, written half an hour later, he says:—"I have just seen the corona, as it was impossible for me to do in 1868, when I was entirely occupied with the spectrum of the protuberances. Nothing can be finer, nothing more luminous, with peculiar forms which exclude all possibility of a terrestrial atmospheric origin. The spectrum contains a very remarkable brilliant green line already indicated; it is not continuous, as has been asserted, and I have found in it indications of the obscure lines of the solar spectrum (especially D). I believe the question whether the corona is due to the terrestrial atmosphere is settled, and we have before us the prospect of the study of the extra-solar regions, which will be very interesting and fertile."

PROF. HUXLEY'S friends, and the scientific world generally, will learn with great regret that he has been compelled to relinquish all work for the present, his medical advisers having ordered him complete rest for two months, for which purpose he has just started for Egypt. There is every prospect that at the end of the time he will return to his old work with renewed vigour.

THE Regius Professorship of Physic in the University of Cambridge has become vacant by the resignation of Dr. Bond, who has held the office since 1851.

THE Council of the College of Preceptors has arranged for the delivery of a series of three lectures to the members of the college and their friends, on the teaching of science in secondary schools. The first lecture of the series, "On Teaching Physics," was delivered at the rooms of the College, 42, Queen Square, on Saturday evening, the 13th instant, by Professor G. C. Foster, F.R.S.; the second, "On Teaching Mechanics," was delivered yesterday (Wednesday) evening by Prof. W. G. Adams; and the third, "On Teaching Botany and Geology," is to be delivered on Monday evening, 22nd inst., by Mr. J. M. Wilson, of Rugby. The point mainly insisted on by Prof. Foster in his lecture, was the necessity, in order to make the study of Physics of much use as a training for the mind, that the pupils should not only see, but actually make experiments for themselves, so that the principal facts and phenomena discussed may be known to them as matters within their own experience.

A SERIES of lectures will be delivered in Gresham College, Basinghall Street, by Mr. E. Symes Thompson, M.D., F.R.C.P., as follows:—Thursday, January 18, 1872, On the Digestive Organs in Health and Disease (continued from last course); Friday, January 19, 1872, On the Blood Vessels; Saturday, January 20, 1872, On the Pulse.

AT the first Anniversary Meeting of the Anthropological Institute, held January 15, Sir John Lubbock, Bart., M.P., F.R.S., president, in the chair, the president delivered an address, and the officers and councils to serve for 1872 were elected as follows:—President—Sir John Lubbock, Bart., M.P., F.R.S.; Vice-Presidents—Mr. W. Blackmore, Prof. Busk, F.R.S., Dr. Charnock, Mr. John Evans, F.R.S., Mr. George Harris, Prof. Huxley, F.R.S.; Director—Mr. E. W. Brabrook; Treasurer—Mr. J. W. Flower; Council—Mr. H. C. Bohn, Captain R. F. Burton, Mr. James Butler, Mr. A. Campbell, M.D., F.R.S., Mr. Hyde Clarke, Mr. J. Barnard Davis, M.D., Mr. Robert Dunn, Mr. David Forbes, F.R.S., Colonel A. Lane Fox, Mr. A. W. Franks, Sir Duncan Gibb, Bart., M.D., Mr. Joseph Kaines, Mr. Richard King, M.D., Mr. A. L. Lewis, Mr. Clements R. Markham, Captain Bedford Pim, R.N., Mr. F. G. Price, Mr. C. Robert des Ruffières, Mr. Spottiswoode, V.P.R.S., Mr. C. Staniland Wake.

MR. SAMUEL SHARPE has presented the sum of 4,000*l.* to University College towards the building fund, and Mr. J. Pemberton Heywood has given a donation of 1,000*l.* towards the same object. The executors of the late Mr. Felix Slade have given 1,600*l.* towards the cost of the fine-art buildings and to provide casts and other appliances for the students.

AT a recent session of the Council of University College, it was decided to admit ladies attending the class of political economy to compete for the prizes and also for the Hume and Ricardo Scholarships awarded for proficiency in that science.

THE young Hippopotamus, which we announced as having been born on Tuesday last week at the Garden of the Zoological Society, died the following day. The body has been sent for dissection to Prof. Humphry at Cambridge. We may hope therefore to hear more of him in the pages of the *Journal of Anatomy and Physiology*, which is edited by the professor.

WE are informed that the next number of the *Quarterly Journal of Science* will contain a detailed account by the editor of the scientific principles involved in the A B C Sewage Company's process, of which, according to the *Times*, Mr. Crookes, F.R.S., has accepted the scientific direction.

THE first part is just published at Leipzig of a new edition of Pritzel's "Thesaurus Litteraturæ Botanicæ," or index of works on the various branches of botany, published in all languages, from the earliest times. As it is more than twenty years since the publication of the last edition, the additions are very numerous.

THE President of the Medical Society of the county of New York, Dr. Abraham Jacobi, has placed in the hands of its treasurer 400 dols., to be awarded for the best essay on "A History of the Diseases of Infancy and Childhood in the United States, and of their Pathology and Therapeutics." Competitors will send their essays in English, with motto attached, and address of the writer, with the same motto, in a sealed envelope, to the present Secretary of the Society, Dr. Alfred E. M. Purdy, 123, East Thirty-eighth Street, on or before January 1, 1873. The committee are authorised by the society to withhold the prize if the essays submitted should not merit it.

DR. J. W. FOSTER, President, and Mr. William Stimpson, Secretary of the Chicago Academy of Sciences, have issued a circular informing the scientific world of the extent of the losses suffered by the Institution through the calamitous fire in that city. These comprise, besides a very large number of other collections of great value, the Audubon Club collection, consisting of very finely mounted specimens of the game birds and mammals, both of America and of Europe and Asia, about 400 in number; the State collection of Insects, recently purchased by the State of the heirs of the late State Entomologist, Mr. B. D. Walsh, for 2,000 dols., but of great scientific value from the number of types it contained; the splendid series of specimens illustrative of the natural history of Alaska, collected in 1865-69 by Bischoff and the naturalists of the W. U. Telegraph Expedition; the Smithsonian collection of Crustacea, undoubtedly the largest alcoholic collection in the world, which filled over 10 000 jars, and contained the types of the species described by Prof. Dana and other American authors, besides hundreds of new species, many of which were described in manuscripts lost by the same fire; the Invertebrates of the U.S. North Pacific Exploring Expedition, collected in great part in Japanese seas by the secretary in 1853-56, which besides Crustacea, included in the last item, embraced great numbers of Annelides, Mollusca, and Radiata, most of which remain as yet undescribed, except in manuscripts also lost; the collection of the marine shells of the coast of the United States, made by the secretary and his correspondents during twenty years of dredgings and general research on every part of the coast from Maine to Texas; nearly every species was illustrated by specimens from every locality in which it occurs, not only on our own shores, but on those of Europe and the Arctic Sea, and in the Tertiary and Quaternary formations, showing the effect of climatic influences, geological age, &c.; this collection embraced about 8,000 separate lots of specimens; the deep-sea Crustacea and Mollusca, dredged in the Gulf Stream by M. Pourtales, of the U. S. Coast Survey, in the years 1867, '68, and '69, which had been placed in the hands of the secretary for description; the results of the deep-sea dredgings in Lake Michigan, by the Academy in 1870 and 1871, the work of the latter year having been performed by Mr. J. W. Milner; the Arctic collections of the late Director of the Academy, Robert Kenicott, made during the years 1859-61. The general collection contained about 2,000 mammals, 30 mounted skeletons, including two mastodons, an African elephant, sea otter, elephant-seal, &c., 10,000 birds, 1,000 nests of eggs, and a great quantity of eggs without nests, 1,000 reptiles, 5,000 fishes, including many large sharks and rays, 15,000 species of insects and other articulates, 5,000 species of shells, with immense numbers of duplicates, 1,000 jars of molluscs in alcohol, 3,000 jars or "lots" of radiates, including several hundred corals, 8,000 species of plants, 15,000 specimens of fossils and 4,000 minerals. In Archæology there were about 1,000 specimens, all American; and the Ethnological collection embraced a very fine series of the clothing and implements of the Esquimaux of Anderson River, collected by Robert Kenicott and his Arctic friends, and presented by the Smithsonian Institution. The Academy desires to announce that although now laid prostrate by the terrible disaster it has suffered, it will soon rise to refill its place among its sister institutions. The trustees have determined to build up again the material interests of the Institution, notwithstanding the terrible losses which they, in common with all of its patrons, have suffered. The publication of its Transactions will soon be resumed. The Academy would therefore take this opportunity to appeal to its correspondents for the donation of their own publications of the past few years, to replace those lost, for which it was also indebted to their generosity.

PROF. NATHAN SHEPPARD, of the University of Chicago, has written to the papers to state the present position of the University of Chicago, and of the Observatory, which is well known in the astronomical circles of Europe. The buildings fortunately escaped, but the fire has left the University in very serious financial difficulties. Many of the gentlemen upon whom the University, and especially the Observatory, was dependent are so reduced in circumstances as to be unable to meet their engagements. The consequence is that the resources of the University are suddenly and greatly abridged. In fact, its income, aside from its tuition fees, is entirely cut off. The Observatory is the first department to feel this loss. A letter just received from Chicago says it is feared that the eminent director, Prof. Truman H. Safford, would be obliged to leave his post for want of support. This will be sad news to the professional correspondents of Prof. Safford in Europe. When the University was founded, about fifteen years ago, a few public-spirited gentlemen rallied around it, and under their self-sacrificing care it has been housed in a commodious and elegant (although unfinished) building, at a cost, including Observatory, telescope, &c., of about forty thousand pounds; and now has, in all departments of study (preparatory, classical, scientific, and law), twelve professors and about 250 students. In conclusion, the Professor, dating from 77, Upper Thames Street, London, asks any reader who would care to lend a helping hand to do so, and to follow in the wake of a Scotch gentleman who has generously offered to head the subscription list with 50*l.*

INOCULATION has by the Indian Legislature been forbidden in the districts of the twenty-four Pergunnahs, Nuddwa, Burdwan, Hooghly, and Howrah in Bengal.

*THE FOUNDATION OF A TECHNOLOGICAL EDUCATION**

TECHNOLOGICAL education is taken up by many writers on the subject at the time when a youth is supposed to enter the School of Technology; and scientific men, as a rule, do not seem to set sufficient stress upon the necessity of laying the foundation for it at a much earlier age. It is not indeed scientific men alone who are interested in this question, but they are the authorities who should speak out upon it, for they alone are competent to pronounce an opinion upon the value of scientific education. It cannot be expected that men who themselves know nothing of science, care nothing for its progress, and recognise none of the obligations under which they lie to it, should favour its introduction into our schools, and thus depart from the stereotyped and antiquated system of education, that brings up our youth but partially fitted or altogether unprepared for a majority of the occupations they are destined to pursue, and exposed at every point to suffer from their own ignorance and the impositions of others. Every one now-a-days should have such a knowledge of scientific principles and methods as will enable him to form a just idea of the value of science, and to distinguish between knowledge and pretence—between science and quackery. The political economist, who has to legislate regarding the natural resources of the country; the capitalist, who invests in their development and manufacture; the lawyer, who has to conduct the numberless suits into which scientific questions enter; the journalist, who claims to enlighten and direct the masses; every one who uses manufactured products liable to adulteration; every one who values his health, or has to consult a medical man or other scientific expert; every father, and, what is still more important, every mother of a family; every youth that is making choice of an occupation for life; or, in other words, every member of a civilised community, ought to be acquainted with the elementary facts and principles upon which all the applications of science are based.

This knowledge, which should thus form an essential feature of general education, is also that which will form the very best foundation for technological purposes. In the first place, it will bring into technological schools a vast amount of excellent material that is now wasted elsewhere; for numbers of youths, with minds well adapted to such pursuits, would take to the practical applications of science, if they knew anything at all of science itself. Nor need there be any fear that the field will thereby be overcrowded; for so long as quacks and pretenders abound there is room for good men, and the difficulty at present is to obtain students who have a natural aptitude, or rather, we should say, an aptitude developed by early education for scientific work.

Secondly, and this is the really important aspect of the case, educators will have to deal with material prepared for their purposes, instead of, as now, receiving it not merely unprepared, but actually warped out of proper condition. For it is not too much to say that a youth who has had a purely academic education, on entering a technological institute has to devote a large portion of his time to mastering elementary ideas and principles, that he should have learned as a child; whilst the erroneous methods of instilling knowledge to which he has been subjected, will be a hindrance to him for years, if not for life. It is but a few days since that a freshman in such an institute gravely asked the writer "if a fish was not an animal," thus displaying, at the age of seventeen, a doubt of the meaning of a term that he should have accurately understood at the age of seven. Of a term, did we write? We mean of a fact; of one of the broadest generalisations of science. Now, what has not such a youth to learn of first principles? How utterly unprepared in the simplest rudiments of knowledge is he for a technological course! But when we come to the system of thought induced by the vicious methods of preparatory study, the case is still worse Here we have the labour of driving practical instruction into the brain of a young man who, after having passed perhaps brilliantly through college, is now laboriously pushing his way through a technological course; he is now nominally near its close, yet three years of steady application have not divested him of the habit of learning by rote on the authority of others. He has no reliance on his own experiences, seeks no explanations by questioning his own reasoning powers, but prefers always to take another's opinion, instead of elaborating a judgment of his own. He is still in fact utterly devoid of the first essentials of self-help in education, so completely have his natural abilities been misdirected in that first course, in which the amount of evil accomplished may be judged by the very brilliancy of his success in it. Such a student will never make a reliable scientific expert. We should not like to trust him even as a druggist's clerk; he should never have entered a technological institute, because he has never had any foundation laid for a technological education.

But in what is such a foundation to consist? and when is it to be commenced? What alterations are to be made in our recognised systems of instruction? Already there are more subjects to be taught than the child has time to learn. We reply, let this education commence in the very infant school; let the methods of instruction be rational, because natural ones; let the subjects be taught in their natural order; and we may very easily teach, or rather "educate," vastly more than we do now. At present beyond mere reading, writing, some mathematics, and something of languages, this child learns absolutely little, and that little superficially. It wastes its time largely in learning the theoretical use of these tools without being made to apply them in building up an education. This is not the way in which the carpenter instructs his new apprentice; if he did, neither would ever reap much benefit from his instruction.

Let the elements of the natural and physical sciences form a part of general education; let physical geography go before political; let the child learn that a history of the world precedes that of man; and at every point let him be familiarised with the intimate dependence between the truths of science and the fact of his own existence. Let these things be taught by a rational method of object teaching, not used to convey desultory information, but as a system of training, whereby the reasoning faculties may be rightly educated, at the same time that the memory is taxed with a stock of useful, because elementary and connected ideas. Let reading and writing sink to their proper rank, as means of education and not as objects of it; and let them, whilst being taught, be used to aid in the acquirement of real knowledge.

This may seem to demand a radical change in our system of preparatory education public and private; but if the technologist wishes to make the most of young minds, he must bend them to his purpose from their earliest years; nor will the community at large, when it understands that its interests in the matter are identical with its own, be averse to the change proposed, which is in accordance with its needs and the progressive spirit of the age. If the advocates of a liberal and enlightened system of popular education in England can succeed in tiding over the shortsighted opposition of sectarianism, as above sketched out, inaugurated there by the aid of its scientific men; the result will be, that the technological schools of Great Britain will be supplied with materials trained from their very infancy in science. Are there no scientific men in the country who will take up the subject here in the same wide-awake spirit?

* By Mr. E. C. H. Day, reprinted from the *New York Technologist*.

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*MECHANISM OF FLEXION AND EXTENSION IN BIRDS' WINGS**

DR. COUES' proposition is, that flexion of the forearm upon the humerus produces flexion (adduction) of the hand upon the forearm, by osseous mechanism alone, and conversely: extension of the forearm causes extension (abduction) of the hand. The point of the article consists in a demonstration of the fact that, in spreading and folding the wing, the radius slides lengthwise along the ulna to a certain extent. Recapitulating certain points in the anatomy of the elbow and wrist. the author shows that this sliding is produced by the relative size, shape, and position of the humeral surfaces with which the radius and ulna respectively articulate; these being such, that in flexion of the forearm the radial surface is nearest the wrist-joint, and in extension the ulnar one; and consequently the two bones of the forearm occupy different relative positions in flexion and extension. In flexion, the radius is pushed forward, and projects somewhat beyond the end of the ulna, impinging upon the radio-carpal bone (scapholunar), and pushing the pinion around the centre of motion of the wrist-joint so that it is more or less flexed. In extension, the reverse motion takes place, from the pulling back of the radius. The proposition is carefully demonstrated, illus-

* Abstract of a Paper read at the Indianapolis Meeting of the British Association for the Advancement of Science, August 1871. By Dr. Elliott Coues. From the *American Naturalist*.

strated with three figures, and likewise shown to be susceptible of ocular proof by direct experiment. Several interesting corollaries are also drawn. Some such mechanism is shown to be an anatomical necessity, from the structure of the wrist-joint, to provide for the extremes of adduction and abduction that take place in the wrist, without straining the joint. Another obvious purpose subserved is equalisation of muscular power, by relegating a part of the work, that the hand muscles would otherwise have to perform, to the larger flexors and extensor of the upper arm; and an actual saving of a certain amount of muscular effort, this being replaced by automatic movements of the bones themselves. Having seen no account of this mechanism, the author is inclined to think it may be unnoticed.* It is is at any rate a new explanation of the design of the peculiar shape and position of the radial articulating surface of a bird's humerus, far more important than that hitherto assigned—viz., its causing simply the well-known obliquity of flexion of the forearm.

* It is indeed not mentioned in the works of Cuvier, Meckel, Tiedemann, Wagner, and other distinguished authors: but Dr. Bergmann, of Göttingen (*Archiv. fur Anat.*, 1839. 296), speaks of essentially the same thing, although the results of the mechanism are not so fully shown. —*Eds. Am. Nat.*

SCIENTIFIC SERIALS

THE number of the *Geological Magazine* for Dec. 1871 (No. 90) contains an unusual abundance of important interesting papers. The first is an article by Prof. Traquair on the genus of fossil fishes to which Prof. Huxley has given the name of *Phaneropleuron*, with the description of new species (*P. elegans*) from the Lower Carboniferous limestone of Burdiehouse. The author describes some new points in the structure of the type-species of this genus (*P. Andersoni*) from the Devonian yellow sandstone of Dura Den), the most important being that the dorsal fin was in that fish continued as a "dorso-caudal" to extremity of the body as in *Lepidosiren* and *Ceratodus Forsteri*. Prof. Traquair gives a restored outline of *P. Andersoni* in accordance with his views, and also figures of two specimens of his new species.—Mr. T. G Bonney contributes an interesting paper on a double "cirque" in the syenite hills of Skye, with remarks upon the formation of cirques, in continuation of his paper read before the Geological Society some time since.—From Mr. Carruthers we have descriptions of two previously unknown coniferous fruits from the Gault of Folkestone; one of them a magnificent cone, described and figured under the name of *Pinites hexagonus*; the other a smaller form called *Sequoites ovalis*. To this paper the author has appended a note on the structure of the scales of his *Araucarites sphærocarpus*, with some judicious remarks on the caution which ought to be exercised by the student of fossil plants in determining the affinities of the often fragmentary remains with which he has to deal.—Mr. James Geikie publishes a first paper connected with that apparently inexhaustible subject, the climate of the glacial epoch. In this the author discusses the evidence furnished by the glacial deposits of Scotland with regard to the occurrence of warm interglacial periods, during which all or nearly all the snow and ice may have disappeared from the face of the country.—Mr. A. H. Green's notes on the geology of part of the county of Donegal contain an interesting account of the structure of the county, especially with regard to the relations of the granites and stratified rocks and to the glaciation of the surface.—And lastly, Mr. A. J. Browne, from an examination of the valley of the Yar in the Isle of Wight, throws out the suggestion that that valley and the other river-valleys of the island were originally occupied by continuations of the Hampshire rivers before the excavation of the Solent.—Among the miscellaneous notices we may call attention to an article by Prof. T. Rupert Jones and Mr. W. K. Parker on the Foraminifera from the chalk of Meudon, figured by Ehrenberg in his "Mikrogeologie."

Quarterly Journal of Microscopical Science, January.—"Notes of a Course of Practical Histology for Medical Students," given at King's College, London, by Dr. Wm. Rutherford, F.R S.E., &c. This paper illustrates the author's method of teaching, the students preparing for themselves the series of specimens of the various tissues. After an enumeration of the tissues so prepared follow some general observations on Examination of Tissues, How to Harden Tissues, How to Soften Tissues, How to make Sections of Tissues, How to render Tissues Transparent, How to Stain Tissues, How to Inject, and How to Preserve Tissues, with notes on cells and cements.—"On the Peripheral Distribution of Non-medullated Nerve-fibres," by Dr. E. Klein. Part II. This is the continuation of the paper commenced in the last number of this journal, and to be concluded in the next. It deals with the Nerves of the Nictitating Membrane and Nerves of the Peritoneum.—"Remarks on Prof. Schulze's Memoir on *Cordylophora lacustris*," by Prof. Allman, F.R.S.; "Size of the Red Corpuscles of the Blood of the Porbeagle, or Beaumaris Shark, *Lamna cornubica*," by George Gulliver, F.R.S. The mean long diameter of the corpuscles measured $\frac{1}{923}$ of an inch, and the short diameter $\frac{1}{1600}$, nearly alike in magnitude to those of the small dog-fish and other Selachii.—"A Note on some Circumstances affecting the Value of Glycerine in Microscopy," by Mr. W. M. Ord. This note suggests that from the action of glycerine on murexide and oxalate of lime, mounted for the microscope, it is impossible not to have some misgivings as to the results of its use in the preparation of tissues for the microscope.—"On Remak's Ciliated Vesicles and Corneous Filaments of the Peritoneum of the Frog," by Dr. E. Klein.—"On the Structure of the Stem of the Screw Pine," by Prof. W. T. Thiselton Dyer. Scalariform ducts were detected by the author in the branches of a *Pandanus*, and crystalline forms of two kinds in the tissues.—"On Students' Microscopes," by Mr. J. F. Payne, with a table of English and foreign microscopes, their features, powers, accessory apparatus, and prices.

Journal of the Quekett Microscopical Club, January.—"Notes on Podisoma," by Mr. M. C. Cooke. After describing the minute structure and mode of germination in these fungi, the author proceeds to detail the experiments of Prof. Oersted, from which it has been supposed that the identity of *Podisoma* with *Rœstelia* has been established. The paper concludes with a critical examination of all the known species, one of which it referred to a new genus, and a different order, under the name of *Sarcostroma Berkeleyi*.—"On the so-called Boring or Burrowing Sponge (*Cliona*)," by Mr. J. G. Waller. The object of this paper is to call in question the burrowing proclivities of the sponges belonging to the genus *Cliona* of which *Hymeniacidon celata*, Bowerbank, is the type. This number completes the second volume of the journal.

SOCIETIES AND ACADEMIES

LONDON

Geologists' Association, January 5.—The Rev. J. Wiltshire, president, in the chair. "On the overlapping of several Geological formations on the North Wales border," by Mr. D. C. Davies, of Oswestry. The author stated that the Geological formations of the district ranged upwards from the Llandeilo to the New Red Sandstone. Attention was directed to the way in which nearly every one of these overlapped the one below, hiding in its course many of the beds, amounting in some cases to 1,000 feet of strata, which at other points were exposed. The overlaps increase as a rule from north to south, except in that of the Bala and Caradoc beds by the Llandovery, which increase in an opposite direction. The author inferred that the conformability of strata at a given point did not necessarily prove the unbroken sequence or complete series of the beds at that point, and also that conformability between either two consecutive beds of the same formation, or between those of two distinct formations, was not to be expected to extend over a large area. Amongst other facts stated in this paper was the important one that coal seams occur in Permian strata in the neighbourhood of Ifton. The President remarked upon the enormous time required for the production of the phenomena described by Mr. Davies. Prof. Morris explained the geological and physical features of the district, and spoke of the high value of the paper.—"Report of the Proceedings of the Geological Section of the British Association at Edinburgh, 1871," by Mr. John Hopkinson, one of the deputation from the Geologists' Association. In this communication the author succinctly stated the more important features of the opening ddress by the president, Prof. Geikie, and of the many papers read before Section C at the meeting at Edinburgh last year, and gave interesting accounts of the two geological excursions under the direction of Prof. Geikie.—Mr. J. T. B. Ives communicated the interesting fact of an extensive bed of peat occurring under gravel between Finchley and Whetstone.—Fossils from the glacial deposits of Islington cemetery were exhibited by Mr. Caleb Evans.

Photographic Society, January 9.—Mr. J. R. Sawyer, in a paper entitled "Photography in the Printing Press," gave an account of the history of mechanical photographic printing. He ascribed to Mungo Ponton the discovery of the action of light upon the bichromates when mixed with certain organic bodies, and to Becquerel the first suggestion of employing gelatine and bichromate in conjunction for photographic printing; but to Poitevin is due the honour of having invented photo-mechanical printing. Mr. Sawyer proceeded to describe the improvements which have since been made, referring to the processes of Tessié de Motay, Lichtdruck, Heliotype, &c. He concluded with a description of photo-collographic printing as now practised.—Mr. J. W. Stillman exhibited and described some new Photographic apparatus.—Mr. Henry Whitfield and Mr. R. Phipps were elected members.

Glasgow

Geological Society, Dec. 14, 1871.—Mr. John Young, vice-president, exhibited specimens of coal from a thin seam, intercalated amidst beds of trappean ash at Glenarbuck, near Bowling. He referred to the discovery, by the late Mr. Currie of Bowling, of thin beds of coal amongst the traps of the Kilpatrick hills at Auchintorlie Glen, which clearly established the carboniferous age of these igneous rocks. He also alluded to his own subsequent observation of thin beds of indurated shale, containing fish remains of carboniferous genera, associated with and overlying one of the seams of coal in the same glen. Since then he had found another thin seam of coal cropping out at a high level in beds of trappean ash on the hillside above Glenarbuck, in the same neighbourhood. In the specimens of the coal exhibited, the woody fibre of the plants in a carbonised condition is clearly distinguishable; and although of a very foul quality, and considerably altered by the heat of the traps amongst which it is imbedded, yet it still gives off a little flame in the burning. From the same ash-bed he had also extracted a portion of the stem of a species of *Sigillaria*, and he believed the greater part of the woody structure observed in this Glenarbuck coal was derived from plants allied to *Sigillariæ* and *Lepidodendra*.—Mr. D. Bell submitted portions of the large pitchstone vein at Corriegills, Arran, and of the sandstone in which it occurs, showing that both rocks are much altered along the lines of contact.

Halifax, Nova Scotia

Institute of Natural Science, November 13, 1871.—"On a Lophioid Fish caught off Halifax Harbour," by Mr. J. M. Jones, F.L.S., president. The little Lophioid fish in the Provincial Museum collection was at first sight regarded by the writer as a Gurnard, but on closer examination it was found to be a Lophioid. The description in the paper, with a figure, were submitted to Dr. Theodore Gill, of Washington, who considered that in the description and figure he recognised the young of the *Lophius americanus* or Sea Devil. It was supposed, however, that the description was slightly defective, and that some characteristic features had been unobserved. The writer did not find the desiderated features in the specimen, and was assured that it never possessed them, as the specimen had been brought to the museum while living and unhurt, and was in the finest state of preservation when examined and described. It was very different from any of the young Lophioids described in Günther's Catalogue, and was, therefore, probably a new Lophioid. The writer referred to two fine specimens of *Lophius piscatorius* lately caught in the Halifax Harbour, one of which had a cod fish in its stomach. He could see no reason for the application of the term *americanus* by American naturalists, as the European and American forms are identical.—On Sir W. Logan and Hartley's Geology of the Precarboniferous Rocks underlying the Picton Coal Field, by Rev. D. Honeyman. Sir W. Logan, in his Report on the Picton Coal Field (*vide* Report of Progress from 1866 to 1869, page 7), says: "No evidence was observed by me on McLellan's mountain to show to what epoch these old rocks belong, but masses somewhat similar are noticed by Mr. Hartley on the west side of East River in a position where they have been mentioned in his Acadian Geology by Dr. J. W. Dawson, who considers them to be of Devonian age, and on his authority they will be so distinguished." By the Devonian colouring of Logan and Hartley's map, which accompanies the Report and illustrates it, it would appear that Sir W. Logan intends that the language should apply to a part of pre-carboniferous rocks in the district of Sutherland River as well as the northern part of McLellan's mountain. Now the rocks of the part of McLellan's mountain range indicated belong to the northern part of one of the great anticlinal Silurian series which extends to the south about nine miles is generally metamorphic and non-fossiliferous. The author was, however, fortunate enough to discover the fossiliferous localities in the series, viz., at Fraser's mountain, the southern extremity of McLellan's mountain, and Blanchard, celebrated in Danzer's Eulogy and elsewhere for its iron deposit. In the former he found Middle Silurian fossils in the western side of the anticlinal, and in the other Middle Silurian fossils on the eastern side of the same anticlinal, of one or both of these Sir W. Logan's Devonian Rocks must be the extension and northern terminus. In this series the author found Lower Heldesberg or Upper Ludlow fossiliferous strata overlying the Clinton and Redina fossiliferous of Fraser's mountain, and this is the most recent of the pre-carboniferous rocks of McLellan's mountain. The other part of Sir W. Logan's Devonian area, the Sutherland river containing the Middle Silurian bend which changes the direction of the Silurians, or connects the N. and S. anticlinals of McLellan and Irish mountains with the Silurians to the east, viz., French River, Barney's River, Antigonish, Arisaig, and Lochaber. In this band there are two monoclinal Middle Silurian series: the one commencing in McLellan's mountain, its greenstone forming Blackwood's mountain, the northern extremity of McLellan's mountain range; overlying this to the south is a metamorphic Medina band. Overlying the greenstone of the second monoclinal on the south is a partially metamorphosed band of Medina age, containing abundance of fossils. The lower part overlying the greenstone at St. Mary's Road contains abundance—beds of Orthids and Athyrus. At Sutherland's River Bridge I found indifferently preserved Lingulæ in the same strata.

Paris

Academy of Sciences, January 2.—After the election of officers and the reading of the report for 1871, M. Delaunay communicated a note on the movements of the perigee and node of the moon.—M. E. Vicaire read a note on the temperature of the solar surface, in which he arrives at the conclusion that this temperature is below 3000° C. (= 5432° F.). M. Faye, M. H. Sainte-Claire Deville, M. E. Becquerel, and M. Fizeau, spoke upon this subject, all of them agreeing in opinion with M. Vicaire. Father Secchi, however, in a third note on the solar temperature, maintained his previous estimate of 10,000,000° C.—M. Chasles read a continuation of his theorems relating to the harmonic axes of geometrical curves; General Morin presented a note by General Didion on the expression of the relation of the circumference to the diameter, and on a new function; and M. Chasles communicated a further note by M. Halphen, on the straight lines which fulfil given conditions.—A note on the electrical currents obtained by the flexion of metals, by M. P. Volpicelli, was read, in which the author enlarged and corrected the results obtained by Peltier and De la Rive.—M. W. Fonvielle read an explanation of the appearance, during balloon ascents, of rings which do not exhibit chromatic decomposition.—A letter was read from M. de Bizeau, of Entre-Monts, near Binche, in Belgium, giving the extreme cold at that place on the 8th December, 1871, at −21·5° C. (= −6·7° F.) at half-past 7 A.M.—M. Pasteur presented a note upon a previous communication of M. Trécul on the origin of lactic and alcoholic ferments, in which he stated that he saw nothing in M. Trécul's results to impugn the exactitude of former experiments or the conclusions which he had drawn from them.—M. A. Trécul read a paper, in which he described the cells of beer-yeast becoming mobile like monads.—M. Berthelot communicated a further paper on the state of bodies in solutions, in which he treated of certain salts of peroxide of iron (sulphate, nitrate, and acetate).—M. Balard presented a third note by M. C. Saint-Pierre on the spontaneous decomposition of certain bisulphites (of lead and baryta).—M. Robin communicated a note by MM. Rabuteau and Massul, on the physiological properties and metamorphoses of the cyanates in the organism, in which the authors state as the result of their researches that the cyanates of potassa and soda are not poisonous, and that in the animal economy they give origin to carbonates.—A note by M. S. Jourdain, containing materials towards the history of *Gymnetrus gladius*, was presented by M. Blanchard. The author describes the anatomy of a specimen of this rare fish, which was stranded near Palavas (in Hérault.)—A note on the heat absorbed during incubation by M. A. Moitessier was communicated by M. Balard. The author finds that the specific heat of fecundated is less than that of unfecundated eggs when treated in the same manner, and infers that a portion of the heat absorbed by the former during incubation is transformed.—M.

Decaisne presented a note by M. A. F. Marion on the fossil plants of Ronzon in the department of the Haute Loire. The flora of the marly limestones of Ronzon includes only fifteen species belonging to the same number of genera; eleven of the species are said to be new. These belong to the genera *Equisetum*, *Podostachys*, *Myrica*, *Celtis*, *Litsœa*, *Bumelia*, *Myrsine*, *Pistacia*, *Mimosa*, *Echiteuium*, and *Ronzocarpum*. The facies of the flora is African or Asiatic.—A note by M. Bleichen on the discovery of *Posidonia minuta* in the Trias of the department of the Gard, and on a deposit of schists containing *Walchia* in the Permian formation of Aveyron, was presented by M. de Verneuil; and a note by M. Sanson on an equine skull from the turbaries of the Somme by M. de Quartrefages. The author of the last-mentioned paper refers the skull obtained by Boucher de Perthes from the ancient turbaries of the Somme to the African variety of the common ass.

January 8.—M. Martin de Brettes presented a memoir on the motion of oblong projectiles in resisting media, and on the explanation of the wounds produced in living creatures by the oblong balls of rifled guns.—M. E. Rolland read a memoir on the effects of variations of work transmitted by machines, and on the means of regulating them.—Three letters from M. Janssen were read, giving an account of the position selected by him at Sholoor, in Neilgherry Hills, for the observation of the solar eclipse of Dec. 12, and a brief statement of his results, the latter will be found in another column.—M. S. Meunier read a note on the transition types in meteorites. In this paper the author indicated certain transitions between the constituents of meteorites analogous to those occurring in terrestrial lithology—namely, between luceite and montrejite, mesminite and canellite, montrejite and limerickite, montrejite and stawoopolite, and between aumalite and tadjerite.—A memoir was presented by M. C. A. Valson on a relation between capillary actions and densities in saline solutions, in which he showed by a table of results that in nearly all cases the amount of capillary action is dependent on the density of the fluid.—M. H. Sainte-Claire Deville presented a note by MM. Troost and P. Hautefeuille on the action of heat upon the oxy-chlorides of silicium.—M. Berthelot read the conclusion of his memoir on the state of bodies in solutions, which related to persalts of iron.—M. S. de Luca communicated some investigations of a complex alum, obtained from the thermomineral water of the Solfatara of Puzzuoli; it consists of sulphuric acid combined with alumina, ammonia, protoxide and sesquioxide of iron, lime, magnesia, and potass, with traces of soda and manganese.—A note by M. D. Tommasi on the action of iodide of lead upon some metallic acetates was read.—M. Dubrunfaut presented a note on the combustion of carbon in carbonic acid in presence of water, in which he indicated the importance of the presence of aqueous vapour in many phenomena of combustion. M. Dumas spoke in opposition to to the views of Dubrunfaut.—M. Pasteur communicated a note by M. J. C. de Seynes on the asserted transformations of Bacteria and Mucedineæ into alcoholic ferments; and M. F. Béchamp a paper on the development of alcoholic and other ferments in fermentescible media, without the direct intervention of albuminoid substances.—M Boussingault presented a note on saccharine matter which appeared in the leaves of a lime tree.—The author stated that the saccharine fluid observed by him was not, as is generally supposed, the production of *Aphides*, but apparently a morbid secretion of the tree; it was found to be identical in saccharine constitution with the manna from Sinai analysed by Berthelot.—M. C. Dareste read a note in which he described the presence of bodies presenting the characters of starch-grains in the testes of birds, before the appearance of the spermatozoids.—M. Decaisne presented a note by M. J. E. Planchon, on the characters and systematic position of the Chinese spiny elm (*Hemiptelea Davidii*); and M. Daubrée some observations by M. H. Magnan, on two recent notes by M. Cayrol, on "The Lower Cretaceous formation of La Clape and Les Corbières."

BOOKS RECEIVED

ENGLISH.—Text Books of Science; Arithmetic and Mensuration: C. W. Merrifield (Longmans).—The Elements of Plane Geometry. 2nd edition: R. P Wright (Longmans).—Concerning Spiritualism: Gerald Massey (Burns).—Catalogue of Transactions, &c, Radcliffe Library, Oxford.

AMERICAN.—Approved Plans and Specifications for Ports, Hospitals, &c.,—Reports on Barracks and Hospitals, &c.—Elements of Chemistry and Mineralogy, Vol. ii.: J. Hinrichs.

DIARY

THURSDAY, JANUARY 18.

ROYAL SOCIETY, at 8.30.—Investigations of the Currents in the Strait of Gibraltar, made in August 1871, by Capt. Nares, of H M S. *Shearwater*: Admiral Richards, F.R S.—On the Absolute Direction and Intensity of the Earth's Magnetic Force at Bombay, and its Secular and Annual Variations: C. Chambers, F.R.S.

SOCIETY OF ANTIQUARIES, at 8.30.—On Neolithic and Savage Implements: A. W. Franks, M.A., and Col. A. H. Lane Fox.

CHEMICAL SOCIETY, at 8.

ROYAL INSTITUTION, at 3.—On the Chemistry of Alkalies and Alkali Manufacture; Prof Odling, F.R.S.

LINNEAN SOCIETY, at 8.—On the Anatomy of the American King-Crab (*Limulus polyphemus*, Lat.): Prof. Owen, F.R.S. (*Continued.*)

FRIDAY, JANUARY 19.

ROYAL INSTITUTION at 9.—On the new metal Indium: Prof. Odling, F.R.S.

SATURDAY, JANUARY 20.

ROYAL INSTITUTION, at 3.—On the Theatre in Shakespeare's Time: Wm. B. Donne.

SUNDAY, JANUARY 21.

SUNDAY LECTURE SOCIETY, at 4.—On King Arthur; the legend and its significance in relation to English life, past and present: Sebastian Evans.

MONDAY, JANUARY 22.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.

VICTORIA INSTITUTE, at 8.—On the Influence of Colloid Matters upon Crystalline Form: Dr W. M. Ord.

ENTOMOLOGICAL SOCIETY, at 7.—Anniversary Meeting.

LONDON INSTITUTION, at 4. Elementary Chemistry: Prof. Odling, F.R.S.

TUESDAY, JANUARY 23.

ROYAL INSTITUTION, at 3.—On the Circulatory and Nervous Systems: Dr. W. Rutherford, F.R.S.E.

WEDNESDAY, JANUARY 24.

GEOLOGICAL SOCIETY, at 8.—On the Foraminifera of the family Rotalinæ (Carpenter) found in the Cretaceous formations, with Notes on their Tertiary and Recent Representatives: Prof. T. Rupert Jones, F.G.S., and W. K. Parker, F.R.S.—On the Infra-Lias in Yorkshire: Rev. J. F. Blake, F.G.S.—Further Notes on the Geology of the Neighbourhood of Malaga: M. D. M. d'Orueta

SOCIETY OF ARTS, at 8.—On Improvements in the Process of Coining: Ernest Seyd

ROYAL SOCIETY OF LITERATURE, at 8.30.—On Excavations at the Site of the Homeric Pergamus: Dr. J. G. Von Hahn.

THURSDAY, JANUARY 25.

ROYAL SOCIETY, at 8.30.

SOCIETY OF ANTIQUARIES, 8.30.

ROYAL INSTITUTION, at 3.—On the Chemistry of Alkalies and Alkali Manufacture: Prof. Odling, F.R.S.

CONTENTS

NOTICE

We beg leave to state that we decline to return rejected communications, and to this rule we can make no exception. Communications respecting Subscriptions or Advertisements must be addressed to the Publishers, NOT *to the Editor.*

THURSDAY, JANUARY 25, 1872

THE SOLAR ECLIPSE

ACCOUNT OF OBSERVATIONS MADE AT POODOCOTTAH

THE spectral observations of recent total eclipses of the sun had plainly demonstrated the existence of an incandescent gaseous stratum or atmosphere, surmounting the chromosphere or stratum of hydrogen which envelops the body of the sun, but they had not sufficed to determine its true conformation and extent. This question, therefore, constituted one of the principal problems remaining to be solved by observations of the eclipse of the 12th of December, 1871.

The slit-spectroscope applied to large telescopes doubtless affords the best means of verifying the existence, in the circumsolar regions, of this gaseous stratum, which may be termed the superior chromosphere, and of determining the materials of which it is composed; but from the shortness of the time available in an eclipse, the spectroscope can furnish only partial and local results, insufficient, therefore, to reveal the true structure, form, and dimensions of this upper chromosphere.

Preceding observations having shown that the light of the solar corona is composed for the most part of a small number of elementary rays differing considerably in refrangibility, it appeared to me that the form and dimensions of the higher chromosphere might be much more conveniently studied by means of a large prism fixed in front of the object-glass of the telescope, whereby the several chromatic images of the corona would be distinctly formed in the focal plane. If the prism has but little dispersive power, and the eye-piece does not magnify too much, all the chromatic images of the corona may in this manner be observed simultaneously in the same field, and their form and dimensions directly investigated.

Towards the end of the year 1868, a small flint-glass prism was made for me by Signor Merz, of Monaco, to be fitted to the object-glass of the equatorial belonging to the Observatory of Campodoglio, for observations on the spectra of the stars; and this apparatus, in consequence of the dispersion of the prism, and the goodness of this prism and of the object-glass, was found to be admirably adapted for observing the eclipse in the manner just described.

The dispersion of the prism from the lines C to H of Fraunhofer is about 32′; the free aperture of the object-glass is 4⅓ French inches; the field of the telescope is about 1°, with a magnifying power of 40.

My conviction of the great advantages which would be afforded by this instrument in the observation of the approaching eclipse, induced me to carry it to India for that purpose; and I was glad to learn that Mr. Lockyer, the chief of the expedition, had in like manner resolved to observe the corona by means of a spectroscope without a slit, being persuaded that this would be the most convenient method of solving the questions relating to the corona itself. With this instrument, then, I prepared to observe the eclipse, proposing to myself the following problems:—

1. To ascertain whether, just before the beginning, and at the end of totality, the solar spectral lines are reversed —a phenomenon observed by Prof. Young in the eclipse of 1870.

2. Amidst the several chromatic images of the prominences, to observe especially whether the image given by the yellow line D^3 coincides with that of the lines of hydrogen gas.

3. To define the form and dimensions of the chromatic images of the corona.

The day before the eclipse, I delineated, by means of the direct-vision spectroscope applied to the telescope, the profile of the solar disc, in order to ascertain the state of the chromosphere at the several parts of the limb, and the protuberances existing there. But the picture did not come out with sufficient exactness, in consequence of the cloudy state of the sky, and the strong wind which prevailed throughout the day. This picture, however, clearly showed that both on the eastern and on the western limb, at the point where contact would take place between the lunar and solar discs in the total eclipse, the chromosphere was in that abnormal condition which is generally observed in the neighbourhood of solar spots.

The number of the prominences was, however, rather small, and their dimensions moderate; conditions which appeared to me to be favourable for the examination of the corona.

From the 5th to the 11th of December, the state of the sky at Poodocottah was somewhat variable; and generally, in the early hours of the day, great masses of mist and cloud predominated in the east, leaving but little hope in favour of our station for observing the eclipse. On the morning of the 12th, indeed, the sky was almost wholly covered with dense masses of mist and cloud, completely obscuring the sun till 7h. 53m., at which time the eclipse had already begun. Soon after this the sun was again covered with thick clouds, but fortunately they began to break a few minutes before totality, when the bright disc of the sun was already sufficiently reduced, and when consequently the time for observation was rapidly approaching.

To verify the phenomenon of the reversal of the spectral lines at the extreme edge of the sun, I had arranged the plane of dispersion at right angles to the edge at the point of second contact.

At thirty seconds before totality, the spectral image of the luminous crescent was already sufficiently weakened to allow of its observation by the naked eye without a dark glass; and it was then that the principal dark lines of the solar spectrum came out distinct, and even more strongly marked than before, and curved parallel to the bright edge of the sun; but a few seconds before totality these lines disappeared completely, and the spectrum became continuous, without however exhibiting, just before totality, the reversal of the lines, although I was watching most intently for this phenomenon. I would not, however, be understood as denying altogether the reversal of the lines, for it is not impossible that a thin film of mist, or the bright atmospheric light at that time diffused over the spectrum of the solar limb, may have concealed the bright lines.

At the very instant of totality, the field of the telescope exhibited a most astonishing spectacle. The chromosphere at the edge which was the last to be eclipsed—surmounted for a space of about 50° by two groups of prominences, one on the right the other on the left, of the point of contact—was reproduced in the four spectral lines, C, D^3, F and G, with extraordinary intensity of light and the most surprising contrast of the brightest colours, so that the four spectral images could be directly compared and their minutest differences easily made out.

In consequence of the achromatism of the object-glass, all these images were well defined, and projected in certain coloured zones, with the tints of the chromatic images of the corona. My attention was mainly directed to the comparison of the forms of the prominences on the four spectral lines, and I was able to determine that the fundamental form, the skeleton or trunk, and the principal branches, were faithfully reproduced or indicated in the images, their extent being, however, greatest in the red, and diminishing successively in the other colours down to the line G, on which the trunk alone was reproduced. In none of the prominences thus compared was I able to distinguish, in the yellow image D^3, parts or branches not contained in the red image C.

Meanwhile the coloured zones of the corona became continually more strongly marked, one in the red corresponding with the line C, another in the green, probably coinciding with the line 1474 of Kirchhoff's scale, and a third in the blue perhaps coinciding with F.

The green zone surrounding the disc of the moon was the brightest, the most uniform, and the best defined. The red zone was also very distinct and well defined, while the blue zone was faint and indistinct. The green zone was well defined at the summit, though less bright than at the base; its form was sensibly circular and its height about 6′ or 7′. The red zone exhibited the same form and approximately the same height as the green, but its light was weaker and less uniform. The height of the green zone was estimated by comparison with the moon's diameter, and from the observed distance of the spectral lines of the prominences.

These coloured zones shone out upon a faintly illuminated ground, without any marked trace of colour. If the corona contained rays of any other kind, their intensity must have been so feeble that they were merged in the general illumination of the field.

Soon after the middle of the total eclipse, there appeared on the eastern limb, at about 110° from the north point, a fine group of prominences formed of jets rather low but very bright, some rectilinear, others curved round the sun's limb, and exhibiting the intricate deviations and all the characters of prominences in the neighbourhood of solar spots. The brightness and colour of these jets were so vivid as to give them the appearance of fireworks.

The spaces between some of these jets were perfectly dark, so that the red zone of the corona appeared to be entirely wanting there. Perhaps, however, this was only an effect of contrast due to the extraordinary brightness of the neighbouring jets. I have thought it right to refer to this peculiarity, because the appearance of interstices, or dark spaces, between prominences of considerable brightness, is often observed by means of the spectroscope, independently of total eclipses.

The want of an assistant to note the time, and to write down the observations as they were made, occasioned me some loss of time, and the end of the total eclipse was already at hand before I was aware of it.

The green and red zones were well developed at the western as at the eastern limb, while the blue remained faint and ill-defined. Soon after the appearance of the chromosphere at the western edge, there was suddenly projected on the spectrum of the sun's limb, which then appeared beyond that of the moon, a stratum of bright lines, separated by dark spaces; but I could not determine whether they were due to a general or partial reversal of the spectral solar lines, or to a simple discontinuity in the spectrum, since they were too soon immersed in a flood of light, which put an end to the totality of the eclipse.

About half an hour after the total eclipse, the sun was obscured by clouds, so that I was unable to observe the end of the partial eclipse.

Later in the day, when the sky had become sufficiently clear, I observed with the spectroscope the state of the chromosphere, and of the protuberances existing upon it; but in consequence of the cloudy state of the sky, the violent wind which prevailed, and the shortness of the time at my command, the picture was not sufficiently distinct and detailed.

L. RESPIGHI

THE ZOOLOGICAL RECORD FOR 1870

The Zoological Record for 1870; *being Vol. VII. of the "Record of Zoological Literature."* Edited by Alfred Newton, F.R.S. (London: published by John Van Voorst, for the Zoological Record Association, 1871.) Pp. 523.

THE "Record of Zoological Literature" is already so well known to, and so well appreciated by, all students of zoology, that we need only remind our readers of the fact that, after five volumes had been published by Mr. Van Voorst, under the editorship of Dr. Günther, the publisher found it impossible to continue its publication, the actual yearly loss being something very considerable. It is true that the British Association for several years contributed 100*l.* towards this loss, and that three of the Recorders contributed, during the years that the British Association was so liberal, an equal sum out of their own pockets. Still, the expenses of such a work are so great, and the number of copies sold so small, that we were not surprised at Mr. Van Voorst's decision, nor to find that the present editor was compelled to look to the co-operation of zoologists generally to attain its continued publication; and it speaks much, not only for his energy, but also for the personal esteem with which he is regarded, that he could obtain in so short a time upwards of eighty friends who should guarantee 400*l.* between them towards any loss that might accrue on this, the seventh volume. While we do not pretend to be in the councils of the committee of the Zoological Record Association, nor have we received even so much as a hint on the subject from the secretary, yet we may venture to express our belief that the members, while they will have the consciousness of having furthered the publication of this work, will not have to pay very much more for the seventh volume than they had for each of the previous six.

Dr. Günther and M. E. von Martens are the only two of the original Recorders who take part in the production of this volume. Prof. Newton's section is taken by Messrs. Sharpe and Dresser; the Insecta are recorded by Messrs. Rye, Kirby, Verrall, M'Lachlan, and Scott; the Arachnida and Myriapoda are noticed by Mr. Cambridge; and the Worms and concluding orders by Mr. E. R. Lankester and Prof. Traquair. The editor stands up bravely in his preface for his staff, and we think he has a very good right to be proud of the work done by his assistants; though we somewhat fail to perceive "the new and perhaps improved modes of treatment" that he refers to.

In proceeding to offer a few friendly criticisms on this work, we would in the first place remark that both editor and Recorders deserve not only the thanks of the Association, but of all zoologists, for the excellent way in which they have accomplished their very difficult tasks, and that we trust that one and all of them will consider our comments as meant for suggestions, and not for fault-finding.

The two most novel features in the volume are "The List of Abbreviated Titles of Journals quoted," and "An Index to the Genera and Sub-genera Recorded as New." As to the List, until we looked over it, we confess that we had an idea that there was some law that guided one in abbreviating the title of a journal. The reader may, perchance, have looked over that corner of the journals of some of the Continental societies in which are recorded the various works sent to them in exchange; and if so he must have smiled to have seen the oftentimes funny attempts made to abbreviate the titles of the British societies. We promise him that, if ever he smiled on such occasions, he will smile still more when he just reads through the "concise forms of citation" given in the "Record," pp. 7—11; and he will, we think, exhaust his patience before he finds out on what principle these concise forms have been chosen. "Ibis" stands for "The Ibis;" while "J.F.O." stands for "Journal für Ornithologie." "P.L.S." stands, not for "Philip Lutley Sclater," as for a moment, in our innocence, we thought, but for "Journal of the Proceedings of the Linnean Society." While the "Journal of the Linnean Society" is very likely to be quoted in the future pages of the "Record," we fancy the "Proceedings" of the society—at least since 1867—will never more be referred to. Of course, any symbol might serve to indicate the journal of a society; but it is rather hard to compel a reader or a consulter of the "Record" to learn off some five pages of such before he can get along. The other novelty supplies a very great need, and one that we believe was often urged on the editor of the first series. The list of names of Genera and Sub-genera occupies in all but five pages, and we would suggest that a little additional space would, in future years, be well spent in indicating where, when, and by whom any of these names had been used before. In the present instance a symbol is affixed to some of the names, indicating that the name to which it is affixed has been used before. But the list has not been properly, or even very carefully, scrutinised for this purpose. On just reading it over, and without referring to such valuable indices as those published each year by the Zoological Society of London, or without pausing at names as familiar as household words to a botanist, we quote the following:—Argyritis, Hein.; Brachyleptus, Mots.; Cadmus, Theob.; Ceratophora, Hein.; Chelaria, Hein.; Dorvillia, Kent; Eucharia, Boisd.; Eurypus, Semp.; Euteles, Hein; Gonia, Hein.; Hellerin, Czern.; Lamprotes, Hein.; Lucina, Wlk.; Pephricus, Pasc.; Perideris, Fieb.; Plicatella, Sdt.; Pœcilia, Hein; Psammobates, Günth.; Rhinosia, Hein.; Thysanodes, Ramb.; Trichocyclus, Günth.; Trinella, Gray; Zetobora, Wlk.; as names all in previous use, not to say that a query might well be affixed to such as Cephalobares, Camb., as being too near to Cephalobarus, Schönk; and if Ceratonia, Rond, is pronounced to resemble too closely Ceratomia, Harr., which, however, we do not quite see, then is there not greater danger of Euplecta, Semp., being confounded with Euplectus, Kirby? It is quite possible that some of these names may, though once used, have since fallen into disuse; and it is very probable that others in the list, unnoticed by us, may have been in use before. To be certain about this would take more time than is at our disposal; but we feel quite sure enough has been said to induce the editor to extend this valuable portion of the "Record," and to make it more exact in the next volume.

May we venture also to say that to certain zoologists who are in some measure ignorant of the mysteries of the Bird Regions, however important from an educational point of view the present arrangement of this part of the Record may be, it would be more generally useful if the titles of papers were all thrown into one series. This would at all events avoid the trouble of cross references, which savour too much of a library catalogue. When we come to the Mollusca, we find a novel practice which, as far as we can find, is not attempted among the Birds, and which we could not fancy being adopted by the Recorder of the other Vertebrates—viz., of not giving the pages on which the descriptions of new species are to be found. This is certainly a most mistaken economy of space, and very materially detracts from the value of these portions of the Record, for one great use of the Record is to enable one to quote an exact reference to a species the history of which one may be quite familiar with, and yet not have the volume containing that history at hand. There is, however, no uniformity in the matter in the present Record, and the Recorders that sin most in this result are those of the Mollusca, Crustacea, Arachnida, and among the Insecta, the Recorders of the Lepidoptera and of the Diptera.

We have been very much struck by the excellent way in which the Records of the Arachnida and Insecta have been executed, save that they too often quote from reprints. Mr. Cambridge and Mr. Rye's portions are quite models of such work. While we acknowledge the thoroughness of the work to be found in the Record of the Neuroptera and Orthoptera, we regret to see the criticisms on Mr. Walker's Catalogues, on p. 451. It is, we take it, not the province of a Recorder to indulge in such criticism, however well deserved it might be; and there are many who will remember how damaging such kind of remarks, made by a certain gentle entomologist, were to the Insect portion of Leuckart's "Bericht."

In his Record of the Vermes, Mr. Ray Lankester has neither done himself nor his subject justice. His mode of arrangement is novel and without precedent; but he has forgotten to give the number of pages to which each memoir extends, and, stranger still, he overlooks quoting the new genera or species described, and this notably in the case of the last work of the illustrious Claparède, and again in the case of Van Beneden's memoir, where we are told, simply enough, that "a number of new and little-known cestoid and other parasitic worms are described and figured." A whole page is taken up with a list of the Annelids referred to in a paper by Prof. Grube, but the list is quite useless, as it wants the remarks as to their synonymy.

Prof. Traquair's portion of the Record appears to have been very well executed. We wish he had given us the list of the Echinoderms from the Dutch East Indies, as described by Herklots. It would have been much more valuable than the list given of very common species from the East Frisian coast; and although we notice an omission of a paper or two among a group (the Cœlenterates) somewhat familiar to us, yet this portion of the volume leaves very little to be desired.

No one individual could write an exhaustive criticism on such a work as this Zoological Record. We have not even attempted it. The moment the volume reached us we cut its pages, and in noting its contents the remarks that we have now made recurred to us; but in addition to these there was also present to us the thought of how much we owed for the successful publication of this work to its accomplished editor and his well-qualified and trusty staff of friends. E. P. W.

OUR BOOK SHELF

I. *Earthquakes, Volcanoes, and Mountain-building*, three articles published in the "North American Review," 1869—1871. By J. D. Whitney. 8vo, pp. 107. (University Press, Cambridge, United States, 1871.)

II. *Historical Notes on the Earthquakes of New England*, 1638—1869. By William T. Brigham, A.M., A.A.S. 4to, pp. 28. (Boston, 1871.)

THE first of these works is a small volume containing three reviews, or essays, as they might be more correctly termed, reprinted from the "North American Review," and written by the well-known geologist Mr. Whitney, formerly director of the Geological Survey of California. They are well worthy of perusal, not only from the easy, somewhat popular style in which they are written, but more especially from their containing a tolerably fair summary of the opinions held by most of the later scientific writers who have treated of the phenomena of earthquakes, volcanoes, and mountain-building, as it is here termed, drawn up by one who is evidently well-read in the literature of these subjects.

To give in its turn a summary of the author's opinions as far as we are able to understand them from a perusal of these three essays, we might state, in the first place, that he lays considerable stress on the geographical data, which show that the area within which the greater earthquakes have been mainly confined is also to a great extent coincident with that of the greatest displays of active volcanic forces; and on the observations showing the action which the moon, or rather of the sun and moon combined, exert on the number and intensity of earthquakes, which, if accepted, indicate an internal condition of fluidity in our globe; he believes both in the chronological succession of volcanic rocks, and in their having proceeded from some common or connected source within the earth, but does not agree with those who regard the access of water as the great agent in volcanic cataclysms; disbelieving (in opposition to some elaborate calculations to the contrary) that the force capable of being developed by steam at such immensely high temperatures, could be sufficient to account for the phenomena of ejection; and although admitting the proximity of volcanoes in general to the sea, points out that some of those in South and North America are situated inland, several hundred miles distant from the ocean.

Regarding the differences in texture between the granitic rocks and those of recent volcanic origin as due mainly to the different conditions of our globe in the early periods in which they were erupted, Mr. Whitney protests against the hypothesis, so much brought forward of late, that the former are merely sedimentary deposits, brought within the action of, and softened or liquefied in, some unaccountable way by internal heat, and with respect to the origin of mountains, regards the external action of rain and rivers, now so all-absorbing in the minds of most English geologists, as altogether secondary to more powerful internal forces, believing, whilst mountain-building is to a great extent the result of an antagonism between subsiding and stationary masses of the earth's crust, that in all the great chains of mountains we have ample proof that this is at the same time accompanied by the intrusion of eruptive rocks from below, as a necessary consequence.

The second *brochure* by Mr. Brigham is reprinted from the memoirs of the Boston Society of Natural History; it appears to be the first part of a more lengthy communication to the Society, and is entitled "Volcanic Manifestations in New England;" it is an apparently exhaustive catalogue of all the principal earthquakes which have taken place, or rather been recorded, since the discovery and settlement of the country until the commencement of last year, bearing evidence of much industry, and appearing to be a valuable contribution to the records of American Seismology. D. F.

Astronomische Tafeln und Formeln. Herausgegeben von Dr. C. F. W. Peters, Assistant der Sternwarte in Altona. (Hamburg: W. Mauke, 1871; London: Williams and Norgate.)

A USEFUL collection of auxiliary astronomical tables compiled by the son of the well-known editor of the *Astronomische Nachrichten.* It brings under one cover many tables for which the computer has ordinarily to resort to different books; and in some cases the tables are exhibited in a more expanded form than that in which they are usually printed. It contains copious tables for converting time into arc, sidereal into solar time, hour and minute intervals into decimals of the day, refraction and hypsometric tables, tabular data referring to the figure of the earth, tables of squares and trigonometrical functions, and many others for facilitating the reduction of astronomical observations. It has also a collection of formulæ in common request, goniometrical, trigonometrical, and astronomical. The collection is based upon, and is in many respects closely similar to that made by Schumacher in 1822, and which was re-edited and enlarged by Warnstorff in 1845. Dr. Peters has, however, added many new tables, and modernised others where necessary. We could wish that a little more care had been bestowed upon the printing; the figures on some of the pages are very indistinct, and would tease a computer sorely. The defect is not accidental to a single impression of the work, for two copies have come before us, and in both the same pages are faulty. J. C.

LETTERS TO THE EDITOR

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

Zoological Statistics and the Hudson's Bay Company

AMONG the "Notes" in NATURE of December 28, there is one in which mention is made of the great dearth of martens imported into London this last season from Hudson's Bay, also of the death of 3,000 Indians from small-pox in the Saskachewan district. It is then added "that martens that are not killed, and Indians that die, mean reduced dividends to the Hudson's Bay shareholders and traders."

Having lived a good many years in the Hudson's Bay Territory, perhaps you will permit me to mention a curious circumstance which I noticed, in illustration that martens may abound yet comparatively very few be killed.

In all parts of the fur country east of the Rocky Mountains, where there is timber, hares (*Lepus americanus*), or "rabbits," as they are commonly, but wrongly, called, are found in greater or less numbers, and they congregate in certain favourite localities. The Indian pitches his tent near one of these places, and by setting snares (which his wife and children attend to), easily supplies himself and family with food, whilst the skins of the hares are worked up into most comfortable blankets.

The hunter all the while is trapping the marten and other fur-bearing animals that assemble to prey upon the poor rabbits, and is thus enabled to secure without much labour a large and valuable stock of furs, chiefly martens.

The hares are, however, liable to a very fatal epidemic,* which usually attacks them when they have become very numerous, and they gradually die off, so that in two or three years there is scarcely one to be seen. This scarcity continues for a couple of winters or so, after which the hares again begin to increase, so that at periods of eight or ten years they are at their maximum.

During this dearth of hares, the Indian has to go to a fishery, or is obliged to travel about in search of buffalo, deer, or other game as a means of support, and has little time for trapping the marten; and if he had the time, he would still be under great disadvantage, for the marten, lynx, and fisher have also to scatter themselves all over the country to pick up a precarious living on lemmings, partridges, and other odds and ends, instead of feasting in luxury and ease, as they do, on the hares when abundant. Thus, when hares are numerous, many marten skins are obtained, when hares are few marten skins are also few, not necessarily because martens are scarce, but that they are difficult to get.

The death of even 3,000 prairie Indians in one season, however injurious it might be to the trade of the Hudson's Bay Company in other kinds of furs, would not particularly affect the number of marten skins obtained.

I may here record a striking instance of the efficacy of vaccination as a preventative of small-pox. Nearly forty years ago this dreadful disease spread like a scourge from the Missouri river all over the prairies, being carried by bands of horse-stealers from one tribe to another; for these amiable "children of nature" no sooner heard of any of their neighbours being attacked by the terrible disorder, than parties went immediately to rob the sufferers of their most valuable property. They got the horses, but they also caught the disease, and many hundreds died. The Crees, a tribe of many thousands, having nearly all been vaccinated by the Hudson's Bay Company's officer in charge of the district, escaped with the loss of only two of their number.

JOHN RAE

* It is quite as fatal in its effects as the grouse disease, and the causes are little known. The hares are found sitting in their forms dead. The Indians say they can tell when the disease is about to commence by a peculiar growth found in the abdomen.

Ripples and Waves

THE article by Sir William Thomson upon Ripples and Waves in the November part of NATURE, which has just reached me, reminds me of a little capillary wave, the examination of which used to be a source of amusement to me some years ago; and as I have never seen any description of it, my observations may not be without interest to some of your readers.

I had long noticed this little wave, winding about, like a hair upon the surface, amongst the eddies which formed in a deep river below a considerable fall, which I used to frequent; but I first got an insight into its nature in a very different situation. I was in a canoe in a sheltered bay, with just enough wind overhead, without any ripple on the water, to make my canoe drift broadside on at the rate of, perhaps, half-a-mile an hour, when I saw my little wave formed about three feet in advance of the canoe. Being in the neighbourhood of a marsh the water was very impure, and the behaviour of the little particles floating in it attracted my attention. Any objects reaching to the depth of from an eighth to a quarter of an inch below the surface passed on to the canoe unaffected by it; but smaller particles were suddenly agitated on passing the wave, and after getting a few inches within it, they were arrested at distances varying with their size, the larger ones penetrating farther than the smaller ones. If the wind died away the wave was maintained at a greater distance from the canoe, and it was still perceptible at a distance of fully eight or nine feet from it, after which it became fragmentary and disappeared. If the pace of the canoe increased, the wave came nearer to it, and the particles, which had been brought to rest at various intervals according to their sizes, were driven up together, forming at last a sort of scum in advance of the canoe. If the wind increased suddenly, the wave disappeared, and the slightest ripple on the surface obliterated it at once; but if the wind freshened very gradually, the wave approached nearer and nearer, becoming at the same time more strongly defined, until it came to within about nine inches from the canoe, and was maintained there under its lee, even after there was breeze enough to make a considerable ripple outside. If pressed beyond that, ripples of quite another character would form just in advance of the wave, and it would break up, and the canoe would pass over the scum which had collected within it.

With this clue as to its nature, I frequently examined the wave in the situations where I had first seen it. Wherever there was any impediment to the stream, as a tree stretching out into it from the bank, there was the little wave ahead of it, at distances from the impediment varying with the force of the current. In the spring, when the water was high, a good deal of foam would be brought down from the falls above, and would collect against these obstructions, but always leaving an inch or two of clear water within the wave. Upon clearing away the foam the wave would soon again be formed, and the next patch of foam which came down would experience a little jerk, as it passed the wave, and penetrate a few inches within it, when it would be arrested, and there would start out from underneath it little particles of sawdust, or other substances, which had been entangled in it, and would range themselves beyond it, in the order of their sizes. Presently more foam would come down, pushing on what had arrived before, till soon there would be an accumulation of it, as at first.

Where the wave was found winding about amongst the eddies there was no solid obstacle, but only one stream meeting another, and it was not at first sight easy to distinguish which was the front and which the back of the wave. The accumulation of scum, however, on one side showed this, and much more so the behaviour of the wave itself, according to the side from which you approached it. If you came down upon it with the stream, with your canoe broadside on, no effect was produced on the wave; but if you passed over it, it was almost immediately reformed on the other side. But if you approached it from the other side, you pushed it on before you; and by careful handling I have often succeeded in detaching a portion of the wave, and carrying it on before me for ten or fifteen yards; whilst after awhile another would be formed in the same place. Sometimes, where the water boiled up from below, there would be an irregular circular patch, surrounded by one of these waves, which you might drive up till the two sides met; or if you approached it stern on, you would cut the circular patch into two, in which case each would run up rapidly to their centre into a little conical jet, and if your pace was at all rapid there would be a drop projected upwards from it.

The wave is so minute that it was not easy to come to any conclusion as to its shape and size; but from the distorted reflection of an object held above it I satisfied myself that under ordinary circumstances it could not be more than one-twentieth of an inch high, the distortion not extending beyond half-an-inch on each side of the sharp cusp, and that it was convex towards the stream, with a very slight trace of concavity on the side of the obstacle generating it. It seemed as if the wave itself was a little elevated above the surface, and that it sloped back very slowly towards the obstacle. This is in accordance with the description above given of a narrowing circular patch running up to a jet; for, although the motion in that case was too rapid to permit of any precise observation, just before it closed in the patch had the appearance of a little table land elevated above the general surface. Upon one occasion, when a boom had been stretched across the river, running at the time fully five or six miles an hour, the wave was only about nine inches from the boom, against which a dense scum was collected, but still with about an inch of clear water between it and the wave. The wave in this case must have been fully an eighth of an inch high, and on its farther side were a succession of ripples, very much exceeding the capillary wave in height and amplitude, and differing from it in not being cusped, though otherwise imitating its general form.

It would appear, therefore, as if a wedge-shaped film of water were pushed ahead of the canoe, or other obstacle, the lower surface of which must, from the arrangement of the particles arrested, have been of rapidly-increasing curvature. Two difficulties, however, present themselves to this explanation—it is difficult to see how the film could have extended to the wave itself, as no particles, however small, appeared to be arrested within an inch or two of it; and my recollection is that upon the occasion of my first examining the wave driven before my canoe, light objects merely resting upon the water, like thistle down, seemed to be not at all affected by it, but to pass on towards the canoe unimpeded. Such objects, however, are so easily affected by the wind, or even the resistance of the air, that it was not easy to verify the observation.

Some other facts may be mentioned. The depth of the obstruction in the water seemed to have no sensible effect on the wave formed. Whether it was a log a foot through, or an inch board floating on the water, or whether it was the middle of the canoe drawing five or six inches, or the bow and stern barely touching the surface, the effect seemed almost the same. I have often, indeed generally, failed in my attempts to generate a wave with a canoe, and although upon the occasion when I first saw it so formed, I could trace it at fully eight feet from the canoe, I never found such a wave naturally formed at anything like that distance. The explanation appears to be that it requires very even and steady action to generate the wave; but that when once established it can be maintained under circumstances in which it would not be otherwise produced. As I stated before, if you approach it in one direction, you may take a canoe over it and it emerges on the other side unimpaired; the irregular currents of an eddy have no effect upon it except to give it an undulating motion, and I have seen it maintaining its place amongst the standing waves of a rapid when they have been several inches high. I have even raised considerable swells by rocking a canoe close to it, and it rides over them without disturbance; but the slightest ripple caused by the wind makes it disappear in a moment; and if spirits of turpentine be dropped on the water a little above it, the whole wave is instantly obliterated to a distance apparently far beyond that to which the oily film extends. JOHN LANGTON

Ottawa, Canada, Dec. 28, 1871

The Rigidity of the Earth

ALTHOUGH, as he truly says, Sir W. Thomson's arguments for the rigidity of the earth have never been attacked, yet they have undoubtedly been too long ignored; and it is gratifying to see them asserted by their author in NATURE. Allow me, however, to remark on one sentence near the end of his quotation from the "Natural Philosophy," where Mr. Hopkins's observation is given, that the distribution of fluid matter within the earth is "probably quite local." Unless I am mistaken, Mr. Hopkins's opinion was, that its distribution is, as one might say, fortuitous. But, as I have elsewhere observed, the trains of volcanoes which accompany many of the great lines of elevation for enormous distances render the motion of such local distribution of fluid matter highly improbable, unless it be admitted that its presence is due to mountain elevations as a cause. I have suggested that this fluidity may arise from a diminished pressure beneath mountain ranges, owing to their mass being partly supported by the lateral thrust which has upraised them—a supposition which Mr. Scrope had already applied to account for an increased fluidity in the heated rock underlying a volcanic vent, when from any cause the pressure became less.

If any of your correspondents can propose another explanation of this remarkable coincidence compatible with the supposition of a rigid globe, it would be interesting to know it.

Harlton, Cambridge O. FISHER

English Rainfall

IN reply to the letter of Mr. Vernon, in NATURE of the 18th inst., permit me to say that the confusion between the two Seathwaites is *his*, not *mine*. In the article to which he refers there is not a word about either Cockley Bridge or the Valley of the Duddon. His topographical knowledge of the districts is, apparently, as inexact as his manner of reading; for he does not seem aware that "the Stye," of which he speaks, is the name, not of a *place*, but of a *rain-gauge*, in, as I said before, the immediate neighbourhood of Stockley Bridge.

J. K. L.

Circumpolar Lands

IN the last number of NATURE (Jan. 18), Mr. J. J. Murphy asks, "Can any mathematical reason be assigned why the contraction of the earth should be least in the direction of the polar direction? This would account for the rising of the land at the poles."

In the Proceedings of the Literary and Philosophical Society of Liverpool for Nov., 1857, there is a paper on a probable change in the earth's form, in which the rising of the land at the poles is inferred as a necessary result of the cooling and contraction of the earth.

The following is the substance, though not the exact words, of a portion of the paper; the precise words would not be intelligible without a diagram.

If a spheroid of equilibrium, in motion about an axis, contract uniformly in the direction of lines perpendicular to its surface, a new spheroid is produced, having a greater degree of eccentricity, because if equal portions are taken off the two diameters, the ratio of the equatorial to the polar diameter is increased. This is equivalent to a heaping up of matter around the equator in excess of what is due to the velocity of rotation, an increased pressure on the interior, in that region, must be produced, and a consequent transmission of pressure towards the poles. "A change of form is then necessary to restore equilibrium. This may not take place uniformly *per gradum*, for if there be a resistance from a rigid external crust, the force must accumulate until it exceeds the resistance, and thus frequent adjustments *per saltum* may ensue. It is probable, therefore, that the earth's form is undergoing a slow progressive change."

GEORGE HAMILTON

Queen's College, Liverpool, Jan. 21

The Kiltorkan Fossils

MR. BAILY'S letter needs only a word or two from me.

I must protest against my reference to an error made by Mr. Baily being considered a "personal attack" upon him, or an "accusation" against him. Has Mr. Baily ever consulted a systematic work which did not contain corrections of the real or supposed errors of former workers? And did he consider such corrections as "personal attacks"?

On two points Mr. Baily has misunderstood or misread the plain statements of my letter:—1. I did not say that his drawing in "Explanation of Sheets 187, &c.," was made on the spot at Kiltorkan, but that it was a drawing of the fossil he had named *Sagenaria Veltheimiana*; 2. The qualifying phrase, "coal measure," was used, as it often is, as the equivalent of "carboniferous." How Mr. Baily could make it mean anything else perplexes me; seeing the Upper Carboniferous beds have no connection with the question. To have used it in the limited sense he suggests, and elaborately argues against, would have been absurd.

The remainder of Mr. Baily's letter is occupied with reference to private letters as evidence in the case. That written by Mr. Baily to Prof. Heer confirms the statement I made at the Geological Society, and repeated in your pages; but, in as far as it declares that the specimens sent to Prof. Heer from Kiltorkan were named *S. Bailyana*, it differs from the statement made by Prof. Heer at the Geological Society, who, on the evidence of these fossils, included *S. Veltheimiana* among the Kiltorkan fossils, and never mentioned *S. Bailyana*!

The reference to the other private letters is equally unhappy; for Mr. Baily is quite wrong in supposing my "accusation" was made because I could not persuade him to join me in work. My letter, if he will look at it again, bears a date some time after the "accusation" was made. And if at the same time he will read his reply, he will find that the reason he gave for declining to work with me is somewhat different from those he records in your pages. But the fact is, the letters have nothing whatever to do with my declaration, now more than ever confirmed by Mr. Baily's letter, that his giving to the Irish Lepidodendroid plant the name of a carboniferous species misled Prof Heer. If Mr. Baily's letter indicates the "facts" contained in his paper, I can only conclude that it was the patriotism of your reporter that induced him to characterise them as "strong." W. CARRUTHERS

Condurango

I HAVE read in No. 104 (October 26, 1871) of your scientific and highly-interesting journal, a few words on "Condurango," the new Ecuadorian plant that has lately called so much general attention in Europe and America to its supposed properties of curing cancer.

The want of exactitude in the description of the plant will doubtless give an erroneous idea of it to your readers, and with the desire of effacing such errors as those published in the "Andes" of Guayaquil, and in Bogota by Mr. Buyon, to whom you make reference, allow me to present to you and your readers the botanical description of the Condurango twining plant, very useful, indeed, in some rheumatisms and secondary syphilitic disorders, but of very doubtful medicinal properties in cancer, so far as my own experience goes.

The Condurango belongs to the order *Asclepiadaceæ*, 3rd tribe, which corresponds to *Aclepiadeæ veræ;* 1st division *Astephanus*, whose characters are that the limb of the corolla is without scales, and the stamens without appendage or corona.

This division comprehends only five genera viz., *Mitostigma*, *Astephanus*, *Hæmax*, *Hemipogon*, and *Nantonia*. In none of these genera can the Condurango be classed.

The genus *Mitostigma*, as a distinguishing character, has two long filaments at the end of the stigma, and this is not the case in Condurango. The genus *Astephanus* has the sepals acute, the corolla subcampanulate, and the stigma elongated; characters that do not belong to the Condurango. The genus *Hæmax* has the divisions of the corolla hooded, and other characters not observed in the Condurango. The genus *Hemipogon* has the sepals of the calyx acute, hard, and with a curved extremity. The corolla is campanulate, which is not the case in Condurango. The genus *Nantonia* has the sepals striated and curved, which also is not the case in Condurango.

The flowers of the Condurango have a calyx of five divisions, obtuse, ovate, and villose in their inferior part, and of quincuncial præflorescence. The corolla is rotate, of five divisions, lanceolate, hairy at the base on the inside, and somewhat fleshy, with a membranous margin. Its æstivation is imbricated. The stamen has no appendage or corona; the anthers are terminated by a membrane, and the pollen-masses are elongated and suspended. The stigma is pentagonal and conical. The flowers are numerous and disposed in umbelliferous inflorescence.

As aforesaid, the Condurango forms a new genus. It is absurd to speak of Condurango as if it were the same as *Mikania huaco.*

In the importance of the subject I hope to find ample apology for asking room in your columns for these few lines.

A. DESTRUGE

Guayaquil, Ecuador, Dec. 13, 1871

Ocean Currents

IT appears to me that the numerical data adduced by Mr. Croll in his letter (NATURE, Jan. 11) disprove his conclusions.

The doing of 9 foot-pounds of work upon a pound of water should give it a velocity (in feet per second) of

$$\sqrt{2 \times 32 \times 9} = 24;$$

and the doing of one foot-pound of work upon a pound of water should give it a velocity of eight feet per second. These are much greater than the observed velocities, so that a margin is left for friction.

The following passage in Mr. Croll's letter also calls for some remark:—"But it must be borne in mind that the deflecting power of rotation depends wholly on the rate at which the body is moving. If difference of specific gravity be regarded as the impelling cause of any current, the deflecting power of rotation will certainly be infinitesimal."

The deflecting force does indeed vary directly as the velocity of the body acted on; but the curvature of path which the deflecting force tends to produce, is proportional to the quotient of the deflecting force by the square of the velocity, and therefore varies inversely as the velocity. In latitude 45°, a velocity of a foot per second would give a radius of curvature of less than two miles. Here, then, again, there is a wide margin left for resistance. The expression for the radius of curvature in feet, supposing that there are no resistances, is

$$\frac{6850\, v}{\sin \lambda},$$

λ being the latitude, and v the velocity in feet per second.

Belfast, Jan. 13 J. D. EVERETT

Mock Sun

I THUS name the phenomenon I am about to describe, but without regard to scientific accuracy. Last evening, a little before sunset, I observed a dark bank of clouds couched on the horizon, just beneath the sun, and a long mass of cirro-stratus above him. A band of light, of about half his width, stretched up and down to the clouds. This remained visible, with remarkable changes, till 25min. after the sun's total immersion. On his disappearance the band gradually widened (or seemed to do so), and assumed the form of a table flower-vase, *i.e.*, bulged at the base and cylindrical above. At ten minutes after sundown the band, which had been about 10° in length, stretched to 20°, being superposed on the cirro-stratus, where it was rose-coloured, the bulged portion being orange. At twenty minutes after sundown a slight collapse occurred, and the band almost disappeared, the bulged portion becoming an orange disc, just like a second sun setting in fog. Soon afterwards this became elongated, and the band reappeared, stretching over an arc of 40°. A few minutes later all disappeared. I witnessed this beautiful phenomenon from a carriage on the L. and N.W. Railway, on both sides of Blisworth.

C. M. INGLEBY

Edgbaston, Jan. 20

Solar Eruptions and Magnetic Storms

AT a recent meeting of the Astronomical Society a paper was read by Mr. Ranyard, in which some suggestions were put forward concerning the possibility of accounting for the solar prominences on the supposition that they may be caused by the projection of matter from a lower level, and that such an uprush into and through the layers above, emerging into the lighter envelope of the chromosphere, might lift before it a cone of compression of the gaseous matter, producing an elevation on the surface, visible to us as a prominence. And the solid particles or masses thus projected might form meteorites, the shape of the prominence being afterwards modified by other causes.

This theory, offering as it does a possible account of the genesis of prominences and meteorites, appears to contain the germ of another hypothesis respecting the cause of the connection between solar eruption and terrestrial magnetism.

If it be legitimate to suppose that in and near the photosphere we have a circuit of conducting matter (viz. incandescent metallic vapours), according to well-known facts any cause tending to effect an unequal distribution of heat, and at the same time a want of homogeneity of structure, such as a difference of pressure or density, would establish thermo-electric currents in such a circuit.

Now such a difference would arise from an upward burst of matter from below the photosphere. If, therefore, the prominences have their origin at great depths below the photosphere, we may expect currents of considerable intensity to circulate round the equatorial region of the sun. In the equatorial region rather than in any other, because it is there that the greatest disturbance is manifested, as shown by observations on the limits of spots and prominences; and, therefore, *there* that the necessary *differences* of temperature are most likely to occur, the effects of such currents being to create secondary or reduced currents in the adjacent layers, and, if of sufficient intensity, in the earth itself.

Provided that this be so, this supposition will suffice to reconcile some observed facts. Secchi has deduced,* in treating of the periodical variations of the magnetic elements, the law that "The annual disturbances are at a maximum at the equinoxes, and at a minimum at the solstices."

Knowing then that the plane of the sun's equator passes through the earth on June 11th and Dec. 12th, and that therefore the equator as seen from the earth presents its widest ellipse in March and September, it follows that such thermo-electric currents, if they exist, are able to exert their maximum inductive effect on the earth at or near the equinoxes.

The case is analogous to the experiment in which terrestrial magnetism is made to cause induced currents in a closed circuit rotated round an axis at right angles to the magnetic meridian.

In this case the ring is placed successively in positions variously inclined, but always keeps its plane perpendicular to the meridian, and the maximum induced current then occurs.

Similarly, solar equatorial currents would produce *their* maximum effect when the plane of the sun's equator has its aspect most nearly in the direction of the earth, and although any

* De La Rive's Electricity, tom. iii. p. 780.

variations in the intensity of these solar currents may be followed by a disturbance in the terrestrial magnetism at any time, yet such disturbance should be at a maximum at the equinoxes (as is the case by Secchi's law), because then the sun is most favourably situated for causing such effects.

In this hypothesis the source of the earth's permanent magnetism is not included, but simply the cause of the close connection between solar eruption and the disturbance of the terrestrial magnetic elements. F. A. FLEMING

Mechanism of Flexion and Extension in Birds' Wings

UNDER the above heading in your issue of January 18, 1872, Dr. Elliot Coues describes the peculiar movements made by the bones of the wing of the bird in flexion and extension. It may interest some of your readers to know that those movements were minutely described and elaborately illustrated in a paper by Dr. J. Bell Pettigrew, communicated to the Linnean Society in June 1867, and published in vol. xxvi. of the Transactions of that body. MILLEN COUGHTREY

Edinburgh University, Jan. 22

Elisée Reclus

A MEMORIAL addressed to the "Commission des grâces," sitting at Versailles, and most influentially signed by many of the leading scientific men in London, was presented at Versailles on the 3rd inst.

It is an appeal for commutation of sentence of deportation passed on Elisée Reclus, the well known French geographer, author of "La Terre," an admirable popular work on physical geography (now being introduced as an English work* by Messrs. Chapman and Hall), and various other books.

A paragraph having appeared in several of the daily papers announcing that M. Reclus's sentence had been already commuted to simple banishment, I regret to state that he is still a prisoner at Versailles, although it is hoped the appeals made in his favour may produce the desired result.

The petition to the Commission in favour of Elisée Reclus was signed by the president of the Geological Society (Mr. Prestwich), Sir Charles Lyell, Bart., Mr. G. Poulett Scrope, Profs. Owen, Ramsay, Williamson, Duncan, Atkinson, Morris, Rupert Jones, Tennant, Messrs. Evans, Forbes, Gwyn Jeffreys, Drs. Carpenter, Richardson, and many others.

A second petition signed by Sir Henry Rawlinson, Sir John Lubbock, Bart., Mr. Darwin, and other men of eminence, was addressed to M. Thiers in favour of Elisée Reclus.

Surely the time for an amnesty has arrived.

British Museum, January 23 H. WOODWARD

* Sections I. and II. of this work are already published; Sections III. and IV. are now in the press.

NOTES ON MICROSCOPY

MOUNTING IN GLYCERINE.—It is often found desirable to mount very thin objects in glycerine, for which no special cell is requisite, and in which the thickness of a cell would be a disadvantage. To accomplish this was often a work of difficulty, since the presence of the smallest amount of glycerine outside the thin glass cover prevented the adhesion of the luting by means of which the cover was to be secured to the slide. Since the introduction of gum dammar dissolved in benzole to the attention of microscopists, this disadvantage has almost wholly vanished. It is now comparatively easy to mount such objects in the following manner. A small drop of glycerine, just enough for the purpose, is let fall in the centre of an ordinary cleaned slide, the object is then placed in the glycerine, having been previously soaked in benzole if any difficulty was likely to be experienced on account of contained air; a cover (say three quarters square) of thin glass is placed over the object and pressed down, taking care that the object remains in the centre; a wire clip then applied holds the cover in its place. If too much glycerine has been used, blotting-paper or a camel-hair pencil will remove all that issues beyond the edge of the cover. If too little, the addition of more at one edge will supply the deficiency, and the superfluous remainder may be wiped away. Thus secured by the clip the edges of the cover may be painted round with gum dammar in benzole, and when dry and firm (in a day or two) the clip may be removed, and the surface of the slide carefully washed to remove any trace of glycerine. The clip may be replaced, and a second thin coating of dammar laid over the first, or old gold size may be used instead. When this is dry "papering" the slide in the usual way helps to provide against accident. The advantages derived from the use of this method are chiefly the facility with which the cover is attached, notwithstanding the presence of a trace of glycerine on the slide and cover, which it is not easy to avoid; and, so far as the experience of two years can vouch, freedom from leakage afterwards, especially when covered with paper. This plan succeeds best with objects as thin as the minute spores of fungi, delicate hairs, &c., and a one-eighth objective may be employed in their examination.

THE ASCI IN PEZIZA.—Having left a specimen of *Peziza humosa* for a long time in water until it became quite soft and pulpy, I was curious to examine it in such condition, and found that the hymenium presented a singular appearance. All the paraphyses had become dissolved into a granular mass, retaining still some of their original colour. Amongst these the asci were free, and there were some free sporidia. In their normal condition the asci are cylindrical, and the sporidia are arranged in a single series, but in the present case the asci had become perfectly spherical, from the absence of all lateral pressure, and the sporidia were clustered in the centre. The line of the external surface of the asci was very distinct amongst the orange-tinted granular mass, and the eight sporidia could be counted within. There could be no doubt of the presence of an investing membrane, but of a much more elastic nature than has been supposed. This fact seems to suggest the probability that more or less lateral compression in the hymenium may influence the character of the asci, and that cylindrical, or clavate and elliptical asci, indicate more or less of lateral pressure during development.

SACRED THREAD.—The sacred thread, or at least one kind of thread held sacred to religious purposes by the Brahmins in India, is derived from the stem of a species of water lily—some say the *Nelumbium speciosum*, others *Nymphæa edulis*. At any rate under the microscope it exhibits a mass of spirals, unwinding in ribbons of four or five threads laterally united. There is no trace of cells mixed up with it, and the spiral threads are as clean as if they had been removed with special care for microscopical purposes.

HOP MOULD.—A new mould has made its appearance during the past autumn on the spent hops so common about Burton-on-Trent. It formed large dense patches of a bright salmon colour, sometimes several inches in length and breadth, upon the sombre hops, and could not have escaped notice had it appeared in previous years. The structure of this mould seems to be closely allied to that of *Oidium*, whilst in many respects it reminds one of *Sporendonema casei*. The creeping mycelium gives rise to branched threads, which become divided into strings of oval conidia or spores. The mould refuses to develop itself artificially, so that the mode in which the beaded spores were produced was not absolutely determined. Directly the threads come in contact with fluid of any kind they are resolved into a mass of oval cells or spores. Specimens of this mould have been published and distributed in Cooke's "Fifth Century of British Fungi" under the name of *Oidium aurantium*, a rather unfortunate specific name, since another member of the same genus which appeared nearly simultaneously on the Continent has been called *Oidium aurantiacum*.

M. C. C.

*HUXLEY'S MANUAL OF THE ANATOMY OF VERTEBRATED ANIMALS**

THIS long-expected work will be cordially welcomed by all students and teachers of Comparative Anatomy, as a compendious, reliable, and, notwithstanding its small dimensions, most comprehensive guide in the subject of which it treats.

To praise or to criticise the work of so accomplished a master of his favourite science would be equally out of place. It is enough to say that it realises in a remarkable degree the anticipations which have been formed of it; and that it presents an extraordinary combination of wide, general views, with the clear, accurate, and succinct statement of a prodigous number of individual facts. The extreme brevity, indeed, takes one in some degree by surprise; and it is only on repeated reading that one feels assured that the facts exposed have been stated with sufficient fulness.

It is a wholesome and encouraging sign of the scientific literature and teaching of the day, that men of the highest eminence devote a portion of their time to the composition of elementary manuals or short guides in their respective sciences. The abuses to which such short manuals are subject are well known, and have been often commented on; and they are no doubt serious when leading to the formation of imperfect knowledge and the exclusion of more extended study. The objections, however, have weight chiefly as applied to the inferior class of such treatises, which, certainly, have too much abounded in this country. A thoroughly good manual, even though strictly elementary, besides forming the first secure basis of correct knowledge, excites a desire for fuller reading, and serves at later periods for useful revisal of more complete information; while its small size obviously places it within the reach of many whose means do not enable them to become possessed of larger treatises, and has thus considerable influence in extending the study of the branch of science to which it is related.

Nor is Prof. Huxley's manual so very short as might at first be supposed from the unpretending form given to it; but rather the abundance of facts is surprising which the author has contrived to compress into the space, without any loss of that clearness and comprehensiveness of statement for which he is so well known. The amount of printed matter, indeed, is very nearly the same as that comprised in the portion devoted to vertebrate animals in the second edition of Gegenbaur's "Outlines," the most approved recent German elementary treatise on Comparative Anatomy.

It is also deserving of note that there is an entire absence of speculation and theory, as well as of any vague generalities. The words "teleology," "design," "type of organisation," "descent," "natural selection," "genesis of species," find no place in this manual, which deals simply with ascertained facts and principles. In most instances, where uncertainty prevails, the grounds of doubt are stated, or the subject is altogether omitted; but on the whole, as is perhaps right in a manual, the author leans to the side of positive statement of his own views, when he has made up his mind on any disputed point.

So much for the manner of the book. As regards the matter, it may be said that, while it presents a masterly and decided statement of the great principles of Vertebrate Morphology, the most characteristic and important feature which pervades the whole, is the constant reference of all anatomical description and zoological distribution to the facts and laws of organogenesis, as ascertained from the observation of fœtal development. This is well known to be one of the great merits of Prof. Huxley's researches and writings, and he has made it truly the key-note and whole tenor of the manual, so as assuredly to prove one of its most valuable qualities in its future influence on the study of Comparative Anatomy.

The first two chapters of the manual, extending to one hundred and eleven pages, are devoted to an exposition of the general organisation of the Vertebrata, as exhibited in the skeleton (endoskeleton and exoskeleton), the muscular system, the nervous system with the organs of sense, the alimentary canal including the teeth, the sanguiferous and lymphatic systems, the respiratory organs, and the renal and reproductive organs. This is premised by a statement of the distinctive characters of the vertebrate organisation, in which the double cavity of the body, neural and visceral, is taken as the most marked basis of distinction between vertebrate and invertebrate morphology; and a clear short sketch is added of the most prominent phenomena of fœtal development.

The remaining six chapters contain a systematic exposition of the classification, organisation, and distribution of the several classes of vertebrate animals, under the three provinces of 1, Ichthyopsida, 2, Sauropsida, and 3, Mammalia; thus recognising the important approximations now established between Fishes and Amphibia under the first, and between Reptiles and Birds under the second of these provinces. In each class the position and organisation of extinct and fossil animals is also given.

The third chapter begins with the statement of the anatomical characters of the three great provinces; after which the organisation of fishes is described under an arrangement which is a modification of Johannes Müller's in the following groups, viz., 1, Pharyngobranchii (Amphioxus); 2, Marsipobranchii (the Myxines and lampreys); 3, Elasmobranchii (the sharks and rays); 4, Ganoidei (Lepidosteus, sturgeons, &c.); 5, Teleostei (osseous fishes); and 6, Dipnoi (Lepidosiren, transitional).

In Chapter 4 the structure of the class Amphibia is similarly given, under the following distribution—viz., I. Saurobatrachia, including, 1, Proteida (Siren, Axolotl, &c.), 2, Salamandrida (newts, &c.); II. Labyrinthodonta; III. Gymnophiona (Cæcilia, &c.); and IV. Batrachia (Anura, frogs and toads).

In Chapter 5, after giving the distinction between Reptiles and Birds as included under the province of Sauropsida, the class Reptilia is distributed under the following groups—viz., I. Chelonia; II. Plesiosauria; III. Lacertilia; IV. Ophidia; V. Ichthyosauria; VI. Crocodilia; VII. Dicynodontia; VIII. Ornithoscelida (Megalosaurus, Iguanodon, &c., transitional; IX. Pterosauria (Pterodactyles); and the comparative osteology of these groups is described.

In Chapter 6 Birds are distributed, and their Osteology is described under the following classification—viz., I. Saururæ (Archæopterygidæ, the metacarpals not ankylosed together); II. Ratidæ, including birds with more or less rudimentary wings, and in which the sternum is without a keel; III. Carinatæ, the large tribe in which the sternum is keeled, including four groups, viz., 1, Tinamomorphæ (Tinamous), 2, Schizognathæ, (the Plovers, Gulls, Penguins, Cranes, Hemipods, Fowls, Sand Grouse, Pigeons, Hoazin); 3, Ægithognathæ, (the Passerines, Swifts, and Woodpeckers); 4, Desmognathæ (the Birds of Prey, Parrots, Cuckoos, Kingfishers, Anserinæ, Flamingoes, Storks, Cormorants).

In Chapter 7 the Muscles and Viscera of the Sauropsida are described together.

Chapter 8 (180 pages) is devoted to the Mammalia, distributed in three great groups, as follows:—

I. Ornithodelphia (1, Monotremata).

II. Didelphia (2, Marsupial animals).

III. Monodelphia, divided provisionally into twelve orders as follows—3, Edentata, 4, Ungulata, 5, Toxodontia, 6, Sirenia, 7, Cetacea, 8, Hyracoidea, 9, Proboscidea, 10, Carnivora, 11, Rodentia, 12, Insectivora, 13, Cheiroptera, 14, Primates. The first of these twelve orders is separated

* "A Manual of the Anatomy of Vertebrated Animals." By Thomas H. Huxley, LL.D., F.R.S. (London: J. and A. Churchill. 1871.)

from the rest by the absence of middle incisor teeth, the next four (4, 5, 6, 7) being reputed nondeciduate, the 8th, 9th, and 10th presenting a zonary placenta, and the remaining orders a discoidal placenta.

It was not to be expected that Professor Huxley should have here departed from the placental classification for which he has elsewhere shown so much favour.

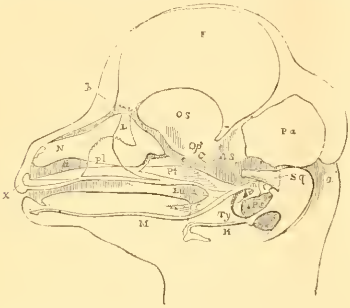

Fig. 1.—The head of a fœtal Lamb dissected so as to show Meckel's cartilage, *M*; the malleus, *m*; the incus, *i*; the tympanic, *Ty*; the hyoid, *H*; the squamosal, *Sq*; pterygoid, *Pt*; palatine, *pl*; lachrymal, *L*; premaxilla, *pmx*; nasal sac, *N*; Eustachian tube, *Eu*.

But however important the distinctions established upon that basis may be in themselves, it may fairly be doubted how far characters derived from parts which do not belong to the permanent organisation of the adult animal, the application of which is not yet fully known in one or two orders, and in which, too, there is much of a transitional nature, are preferable to signs of a more marked and easily observable kind deducible from other parts of the organisation.

In the description of structure all these orders are referred to; but in several of them particular familiar ani-

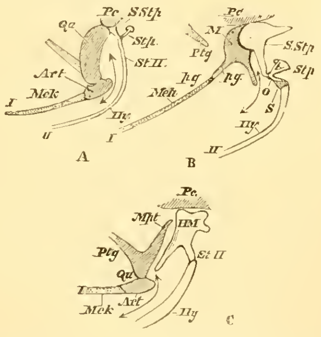

Fig. 2.—Diagram of the skeleton of the first and second visceral arches in a Lizard (A), a Mammal (B), and an Osseous Fish (C).
The skeleton of the first visceral arch is shaded, that of the second is left nearly unshaded. *I.* First visceral arch. *Mck* Meckel's cartilage. *Art.* Articulare. *Qu.* Quadratum. *Mpt.* Metapterygoid; *M.* Malleus; *p.g.*, Processus gracilis. *II.* Second visceral arch. *Hy.* Hyoidean cornu. *St. H* Stylohyal. *S.* Stapedius. *Stp.* Stapes. *S. Stp.* Supra stapedial. *H.M.* Hyomandibular. The arrow indicates the first visceral cleft. *Pc.* The periotic capsule. *Ptg.* The pterygoid.

mals are happily selected for the fuller illustration of the more important systems; as for example, the horse, pig, dog, rabbit, hedgehog, seal, ox, porpoise: thus suggesting to the student the means by which a more practical and thorough knowledge of the organisation may be obtained by actual observation, than by the mere description of varieties in a wider series of animals less within his reach.

In regard to the order to be followed in so extensive a range of description as the comparative anatomy of any large tribe of animals involves, it may be remarked that, however interesting in a physiological point

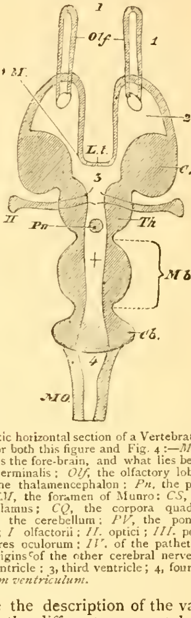

Fig. 3.—Diagrammatic horizontal section of a Vertebrate Brain. The following letters serve for both this figure and Fig. 4:—*Mb*, Mid-brain. What lies in front of this is the fore-brain, and what lies behind, the hind-brain. *L. t.* the lamina terminalis; *Olf*, the olfactory lobes; *Hmp*, the hemispheres; *Th.E*, the thalamencephalon; *Pn*, the pineal gland; *Py*, the pituitary body; *F.M*, the foramen of Munro; *CS*, the corpus striatum; *Th*, the optic thalamus; *CQ*, the corpora quadrigemina; *CC*, the crura cerebri; *Cb*, the cerebellum; *PV*, the pons varolii; *MO*, the medulla oblongata; *I* olfactorii; *II*. optici; *III*. point of exit from the brain of the motores oculorum; *IV*. of the pathetici; *VI*. of the abducentes; *V-XII*, origins of the other cerebral nerves. 1, olfactory ventricle; 2, lateral ventricle; 3, third ventricle; 4, fourth ventricle; +, *iter a tertio ad quartum ventriculum*.

of view may be the description of the variations of form and structure in the different organs taken separately in the whole series of animals, the results of this mode of teaching and study are inferior to those obtainable from the method of description of the whole organisation in successive groups or individual animals, as regards pro-

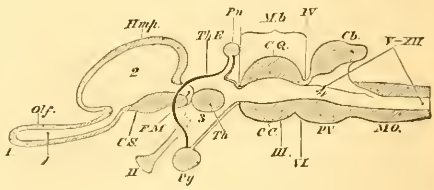

Fig. 4.—A longitudinal and vertical section of a Vertebrate Brain. The letters as before. The *lamina terminalis* is represented by the strong black line between *FM* and 3,

gress in morphological attainments, the determination of zoological affinities, and their application to the solution of the great biological problems of the day.

The latter part of this chapter treats of the Primates, which are divided into—1, the Lemuridæ, 2, the Simiadæ, and 3, the Anthropidæ. The Simiadæ are thrown into three groups, viz., 1, Arctopithecini, or marmosets; 2, Platyrrhini, or American monkeys; and 3, Catarrhini, or monkeys of the Old World, including two sub-groups, viz., *a*, Cynomorpha (with ischial callosities), and *b*, Anthropomorpha. In this last the author recognises with certainty as distinct the genera Hylobates or Gibbons, Pithecus or Orang, and Troglodytes or Chimpanzee, and is inclined to separate Gorilla as a fourth genus.

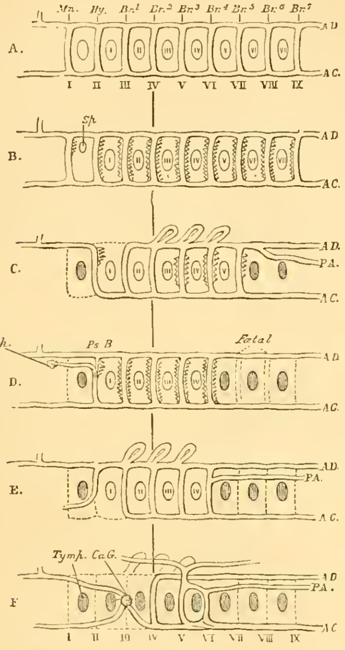

FIG 5.—A diagram intended to show the manner in which the aortic arches become modified in the series of the *Vertebrata*.
A. A hypothetically perfect series of aortic arches, corresponding with the nine postoral visceral arches, of which evidence is to be found in some Sharks and *Marsipobranchii*. *A.C.* Cardiac aorta; *A.D.* Dorsal or subvertebral aorta. I.—IX. the aortic arches, corresponding with *Mn.*, the mandibular; *Hy.*, the hyoidean, and *Br.*1—*Br.*7, the seven branchial visceral arches. I. II. III. IV. V. VI. VII., the seven *branchial* clefts. The first *visceral* cleft is left unnumbered, and one must be added to the number of each branchial cleft to give its number in the series of visceral clefts.
B. Hypothetical diagram of the aortic arches of the shark *Heptanchus*, which has seven branchial clefts. *Sp.* The remains of the first visceral cleft as the spiracle. Branchiæ are developed on all the arches.
C. *Lepidosiren.*—The first arch has disappeared as such, and the first visceral cleft is obliterated. Internal branchiæ are developed in connection with the second, fifth, sixth, and seventh aortic arches; external branchiæ in connection with the fourth, fifth, and sixth. *P.A.* the pulmonary artery. The posterior two visceral clefts are obliterated.
D. A Teleostean Fish.—The first aortic arch and first visceral cleft are obliterated, as before. The second aortic arch bears the pseudo-branchia (*Ps. B.*), whence issues the ophthalmic artery, to terminate in the choroid gland (*Ch.*). The next four arches bear gills. The seventh and eighth arches have been observed in the embryo, but not the ninth, and the included clefts are absent in the adult.
E. The Axolotl (*Siredon*), a perennibranchiate amphibian. The third, fourth, fifth, and sixth aortic arches, and the anterior four branchial clefts, persist. The first visceral cleft is obliterated.
F. The Frog.—The three anterior aortic arches are obliterated in the adult. The place of the third, which is connected with the anterior external gill in the Tadpole, is occupied by the common carotid and the *rete mirabile* (carotid gland, *Ca.G.*) which terminates it. The fourth pair of aortic arches persist. The fifth and sixth pair lose their connection with the subvertebral aortic trunk, and become the roots of the cutaneous and pulmonary arteries. The first visceral cleft becomes the tympanum, but all the others are obliterated in the adult.

An interesting synopsis is given of the anatomical peculiarities of these animals, and of the circumstances in which they most differ from, or resemble, man. Among these the author has inadvertently overstated the proportion of the volume of the brain of the orang and chimpanzee to that of man, when he rates it at about half the minimum size of the normal human brain. Taking thirty-three ounces as the lowest weight of the latter consistent with a natural condition in the adult male, the brain of the orang and chimpanzee may be stated at a third of that weight.

At p. 487 this subject is summed up as follows:—"Of the four genera of the Anthropomorpha, the gibbons are obviously most remote from man, and nearest to the Cynopithecini.

"The orangs come nearest to man in the number of the ribs, the form of the cerebral hemispheres, the diminution of the occipito-temporal sulcus of the brain, and the ossified styloid process; but they differ from him much more widely in other respects, and especially in the limbs, than the gorilla and chimpanzee do.

"The chimpanzee approaches man most closely in the character of its cranium, its dentition, and the proportional size of the arms.

"The gorilla, on the other hand, is more man-like in the proportions of the leg to the body, and of the foot to

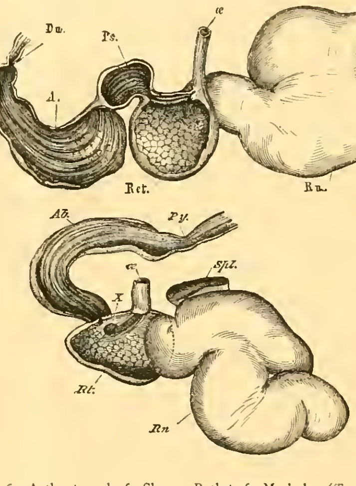

FIG. 6.—A, the stomach of a Sheep. B, that of a Musk-deer (*Tragulus*). *æ*, œsophagus; *Rn*, rumen; *Ret.*, reticulum; *Ps.*, psalterium; *A.*, *Ab* abomasum; *Du.*, duodenum; *Py.*, pylorus.

the hand; further, in the size of the heel, the curvature of the spine, the form of the pelvis, and the absolute capacity of the cranium."

The work is concluded with a brief statement of the characteristics of the human organisation. Among these the superior size of the head of the male infant at birth might perhaps have received a more prominent place. The short statement of variations in structure connected with difference of race is of peculiar interest. The various races of mankind are placed in two groups according to the character of the hair, viz., *a*, the Ulotrichi (crisp or woolly-haired), who are almost all dolichocephali, and *b*, Leiotrichi (straight-haired), who are distributed in four sets, viz., 1, Australioid, 2, Mongoloid, 3, Xanthochroic, or blue-eyed whites, and 4, Melanochroic, or dark whites.

It will now be proper to place before the reader some illustrations, taken from the "Manual," of Prof. Huxley's mode of treatment of individual topics.

The first of these which is selected (Fig. 1) relates to the intricate but deeply interesting subject of the homology of the *os quadratum* of birds and reptiles, a bone which was compared by Cuvier to the tympanic bone of mammals, but which more lately, in consequence of the embryological researches of Reichert and Rathke, was held to correspond rather with the incus,—a view in which Prof. Huxley formerly concurred. Later observations, however, (detailed in a paper published in the Proceedings of the Zoological Society for 1869) have led him to alter his opinion, and to form the opinion that the *os quadratum* may, with the greatest probability, be regarded as representing the malleus.

In explaining this morphological point, Prof. Huxley refers as follows (at p. 27) to the osteogenetic process connected with the formation of the lower jaw and hyoid bone, or mandibular and hyoid arches.

"Two ossifications commonly appear near the proximal end of Meckel's cartilage, and become bones moveably articulated together. The proximal of these is the quadrate bone found in most vertebrates, the malleus of mammals; the distal is the *os articulare* of the lower jaw in most vertebrates, but does not seem to be represented in mammals. The remainder of Meckel's cartilage usually persists for a longer or shorter time, but does not ossify. It becomes surrounded by bone, arising from one or several centres in the adjacent membrane, and the ramus of the mandible thus formed articulates with the squamosal bone in mammals, but in other vertebrata is immoveably united with the *os articulare*.

"Hence the complete ramus of the mandible articulates directly with the skull in mammals, but only indirectly, or through the intermediation of the quadrate, in other vertebrata. In birds and reptiles, the proximal end of the quadrate bone articulates directly (with a merely apparent exception in Ophidia), and independently of the hyoidean apparatus, with the periotic capsule. In most if not all fishes, the connection of the mandibular arch with the skull is effected indirectly, by its attachment to a single cartilage or bone, the *hyomandibular*, which represents the proximal end of the hyoidean arch."

This last "is often united, more or less closely, with the outer extremity of the bone, called *columella auris*, or *stapes*, the inner end of which, in the higher vertebrata, is attached to the membrane of the *fenestra ovalis*."

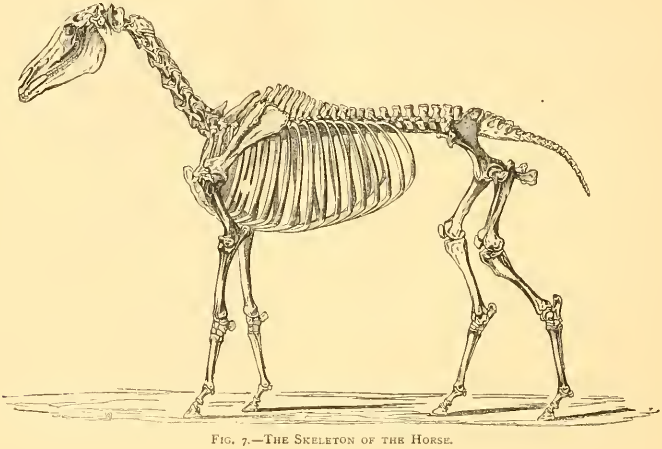

FIG. 7.—THE SKELETON OF THE HORSE.

A fuller and most interesting account of the origin and relations of these bones in connection with the changes occurring in the development of the first and second visceral arches, and with the formation of the external and middle parts of the ear is given at pp. 83—85; but there is only space here to reproduce the diagrammatic figure of the Manual (Fig. 2), which places very clearly in comparison their probable homology in fishes, reptiles, and mammals.

The main point on which the question hinges seems to be the separate connection ascertained to exist between the periotic capsule and the two rods contained respectively in the first and second visceral arches; the proximate part of the first becoming the *quadrate bone*, or *malleus;* that of the second becoming the *incus* in the part above the attachment of the *stapes* to the rod, and *stapedius* muscle below; while the *stapes* itself, or *columella auris*, is an offset, as it were, from the second rod proceeding to the *fenestra ovalis.* The subject, however, is one of so great difficulty, especially as connected with the existence and relations of the tympanic bone in birds and reptiles, to the proof of which the researches of Peters have been directed, that it would be hazardous to attempt any decision of the question at issue without an opportunity of going very fully and minutely into the whole investigation.

The third illustration from the earlier part of the Manual (Figs. 3, 4) gives a clear view of the relations now very generally recognised between the rudimentary parts of the fœtal brain and those forming the adult encephalon of the different classes of *Vertebrata;* and it is enough to refer to the diagrammatic figures, with their description.

The exposition which follows of the modifications in the form and organisation of the brain in different animals, together with the comparative views of the brains of the rabbit, pig, and chimpanzee, in figs. 21 and 22 of the Manual, is most instructive and worthy of attentive study.

The next illustration extracted from the Manual (Fig. 5) is diagrammatic, like the preceding ones, being intended to explain the changes by which, in fishes and amphibia, the permanent arterial vessels nearest the heart are derived from the common typical aortic arches, which, in the fœtus of all vertebrate animals, surround the first part of the alimentary canal.

The illustration might advantageously be extended to show the parallel changes in reptiles, birds, and mammals; these, however, may be better given from the ventral than from the lateral aspect.

The figure here inserted of the skeleton of the horse (Fig. 7) is a very good example of the wood engraving, in which, notwithstanding the small scale, there is remarkable clearness of detail; and the succeeding figures, representing several details of the osteology of the same animal, are all to be commended for beauty and delicacy of execution.

The illustration given in Fig. 6 is one in explanation of the structure of the stomach of the ruminant, in connection with which the following statement of recently-established points regarding rumination may be quoted (p. 381):—

"1. Rumination is altogether prevented by paralysis of the abdominal muscles, and it is a good deal impeded by any interference with the free action of the diaphragm.

"2. Neither the paunch nor the reticulum ever becomes completely emptied by the process of regurgitation. The paunch is found half full of sodden fodder, even in animals which have perished by starvation.

"3. When solid substances are swallowed, they pass indifferently into the rumen or reticulum, and are constantly driven backwards and forwards, from the one to the other, by peristaltic actions of the walls of the stomach.

"4. Fluids may pass either into the paunch and the reticulum, or into the psalterium, and thence at once into the fourth stomach, according to circumstances.

"5. Rumination is perfectly well effected after the lips of the œsophageal groove have been closely united by wire sutures.

"It would appear, therefore, that the cropped grass passes into the reticulum and rumen, and is macerated in them. But there is no reason to believe that the reticulum takes any special share in modelling the boluses which have to be returned into the mouth. More probably, a sudden and simultaneous contraction of the diaphragm and of the abdominal muscles compresses the contents of the rumen and reticulum, and drives the sodden fodder against the cardiac aperture of the stomach. This opens, and then the cardiac end of the œsophagus, becoming passively dilated, receives as much of the fodder as it will contain. The cardiac aperture now becoming closed, the bolus thus shut off is propelled by the reversed peristaltic action of the muscular walls of the œsophagus into the mouth, where it undergoes the thorough mastication which has been described."

In connection with this it may be remarked that fuller illustration by figures of the organs of digestion, circulation, and respiration in different animals seems desirable in the Manual.

Of the 110 woodcuts contained in the Manual, two-thirds are original, while the remaining third (37) are borrowed from other authors, whose names are mentioned in the preface.

For so complex a subject as the osteology of the skull, as well as perhaps in several other parts, some extended table of the bones, with the letters of reference employed throughout the work, would afford considerable assistance to the student.

It might also be advantageous in an elementary work of this kind to have added select references to works for fuller study, and a glossary of (at least unusual) terms.

In concluding this notice we repeat that the Manual is in every way worthy of its learned author, and calculated to be extremely useful in promoting the study of Comparative Anatomy and Zoology on sound principles. The work cannot fail soon to go to a second edition, when the author will have considered the expediency of such additions as we have ventured to suggest, or of others of which he approves, and which he has doubtless been deterred from including in the present work from the desire to bring it within as narrow a compass as possible. We may also express the hope that the publishers have made arrangements for the speedy publication of a similar Manual of the Anatomy of the Invertebrate Animals.

ALLEN THOMSON

NOTES

M. JANSSEN has addressed to the French Academy of Sciences the following letter, on the principal consequences to be drawn from his observations on the solar eclipse of 12th December last; it is dated Sholoor, December 19, 1871:—"I had the honour," he says, "of sending you on the very day of the eclipse a few lines to inform the Academy that I had observed the eclipse under an exceptional sky, and that my observations led me to assume a solar origin for the Corona (see NATURE, vol. v. p. 190). Immediately after the eclipse I was obliged to busy myself with the personal and material arrangements for my expedition into the mountains, and hence I have been unable to complete any detailed account, but I take advantage of the departure of this courier to give some indispensable details as to the announced results. Without entering into a discussion, which will form part of my narrative, I shall say, in the first place, that the magnificent Corona observed at Sholoor showed itself under such an aspect that it seemed to me impossible to accept for it any cause of the nature of the phenomena of diffraction or reflection upon the globe of the moon, or of simple illumination of the terrestrial atmosphere. But the arguments which militate in favour of an objective and circumsolar cause, acquire invincible force when we inquire into the luminous elements of the phenomenon. In fact, the spectrum of the Corona appeared in my telescope, not continuous, as it had previously been found, but remarkably complex. I detected in it, though much weaker, the brilliant lines of hydrogen gas, which forms the principal element of the protuberances and chromosphere; the brilliant line which has already been indicated during the eclipses of 1869 and 1870, and some other fainter ones; obscure lines of the ordinary solar spectrum, especially that of sodium (D); these lines are much more difficult to perceive. These facts prove the existence of matter in the vicinity of the sun; matter which manifests itself in total eclipses by phenomena of emission, absorption, and polarisation. But the discussion of the facts leads us still further. Besides the cosmical matter independent of the sun which must exist in its neighbourhood, the observations demonstrate the existence of an excessively rare atmosphere, with a base of hydrogen, extending far beyond the chromosphere and protuberances, and deriving its supplies from the very matter of the latter—matter which is projected with so much violence, as we may ascertain every day. The rarity of this atmosphere at a certain distance from the chromosphere must be excessive; its existence, therefore, is not in disagreement with the observations of some passages of comets close to the sun."

WE earnestly call the attention of all men of science who may have influence with the French Government, to the letter on behalf of Elisée Reclus by Mr. H. Woodward, which will be found in another column.

WE have to record the death of the Rev. Canon Moseley, F.R.S., on Saturday last in his 71st year. Born in 1801, he went to St. John's College, Cambridge, where he graduated seventh wrangler in 1826. He was for a time Professor of Natural Philosophy and Astronomy at King's College, London, and was afterwards appointed one of Her Majesty's Inspectors of Schools, and was a member of the Ordnance Select Committee. Canon Moseley was well known for his writings on various physical subjects, in particular on the phenomena connected with the freezing of water, and the molecular constitution of glacial ice.

THE *Photographic News* notices the death of one of the most eminent continental photographers, Johannes Grasshoff, of Berlin, at the early age of thirty-six. At the recent exhibition of the Photographic Society in Conduit Street, his studies were among those most admired in the whole collection, and not least his group of thirty different pictures from one and the same model. Like some others of the most successful photographers, his education was that of an art student, and he was known as a clever painter before he became a skilful photographer.

It will be recollected what a warm discussion was raised in the French Academy of Sciences before the late war by the proposal to enrol Mr. Darwin among its corresponding members. The proposal was at that time postponed, but his name has now been placed first on the list for the forthcoming election of a Corresponding Member in Zoology, and he will, therefore, no doubt receive the honour. His supporters are MM. Milne-Edwards, Quatrefages, and Lacaze-Duthiers.

At the meeting of the Royal Geographical Society on Monday evening last, Lieutenant Dawson, R.N., was introduced as the leader of the party organised to attempt the relief of Dr. Livingstone. Mr. W. O. Livingstone, a son of the explorer, born in the neighbourhood of Lake N'gami, is to accompany the party. An application to the Treasury for a grant of money to aid the expedition has been unsuccessful. Should this decision be a final one, the undertaking must therefore depend entirely on private subscriptions; but we are happy to see that the subject is already being warmly taken up in many of the larger towns in the country, and the sum of 1,700*l.* was announced as having been raised by Monday evening last. Since then a public meeting has been held at Glasgow, at which 200*l.* was subscribed, and one will probably be held in London, under the auspices of the Lord Mayor.

The subscription raised as a Memorial Fund to the late Mr. Alder of Newcastle now amounts to about 300*l.* This is considerably less than the amount it was thought might have been raised, though sufficient to carry out in a limited form the original suggestions as to its appropriation. The Committee recommend that it should be invested in the names of trustees, and should serve as the foundation of a Scholarship in Zoology, or other branch of Biology, bearing Mr. Alder's name, in the New College of Physical Science in Newcastle; the transfer to be coupled with such stipulations as to the teaching of Biological Science as may be agreed upon.

The editor of *Les Mondes* calls attention to the manner in which scientific chairs have been disposed of in France, not so much with the object of "finding men to fill the vacant places as places for the *protégés* or favourites of the moment." On the death of M. d'Archiac, the chair of palæontology in the Museum of Natural History at Paris was given to M. Lartet, a palæontologist of world-wide renown, but too advanced in years and of too feeble health to permit him to give a single lesson. On the death of M. Lartet, although there are a large number of good palæontologists in France, it was all but decided, from motives of private convenience and patronage, to abolish the chair, its maintenance being secured by a majority of two votes only. The appointment has now been made to the professorship of M. Albert Gaudry, late assistant to Prof. d'Archiac, and author of "La Géologie et la Paléontologie de l'Attique," an appointment which will give general satisfaction.

The *Engineer* states that the French Government, impressed by the want of thorough geographical instruction, have under consideration a plan for a Geographical Institute, on a scale which has never before been attempted. The proposed Institute is to include all the means and accessories of geographical education in its widest acceptation—books, maps, charts, globes, instruments, collections of natural objects, &c.—and to include a staff of professors and teachers of the highest grades. The naval depôt of charts and plans will form one of the departments of the new Institute, which promises to be of eminent service, not only to France, but to the whole of Europe, for, should it be established on the scale proposed, there is little doubt that it will give an impulse to geographical study throughout the civilised world.

The Massachusetts Society for Promoting Agriculture will award on the 1st of March next two prizes of 300 dols. and 200 dols. respectively to the two best establishments in the State for the culture of fishes for food, all competitors for which must send in their names and addresses to the secretary of the Society, Edward N. Perkins, 42, Court Street, Boston. The committee of award will consider the number of species of fishes cultivated, the number of individuals, and their size and condition, the number of eggs hatched in the establishment, and of young reared from them, the neatness and economy of the establishment, and the excellence of the fixtures.

Dr. Stimpson, the secretary of the Academy of Sciences of Chicago, left Baltimore in the steamer of the 15th of December for Key West, for the purpose of making explorations and collections in the Florida waters, partly with the object of replacing that portion of the collection of the Chicago Academy lost by the fire. It is expected that he will take charge of the dredging operations of the United States Coast Survey steamer *Bibb*, while she is employed in selecting a line for the submarine cable which is to be laid for the International Cable Company between Cape San Antonio, Cuba, and some point on the coast of Yucatan.

We learn from the *Gardeners' Chronicle* that among the disastrous losses occasioned by the Chicago fire, the very valuable Entomological Collection of the late Dr. Walsh was totally destroyed. The *Canada Farmer* states that after the death of the eminent entomologist, the collection became by purchase the property of the State. It was not only very extensive, but the specimens were arranged and labelled with great care and accuracy; and it will be many years before another can be collected to replace it.

The first number of the Journal of the Anthropological Institute of New York, an institution newly organised upon the base of the former Ethnological Society of that city, is published. In the change the scope of the society has been greatly enlarged, and many of the difficulties attendant upon the maintenance of the old organisation have been obviated. Several papers of more or less interest are to be found in this first number, and there is little doubt that the new society will occupy a prominent place in advancing knowledge in the world.

Mr. Stephen T. Olney, a well-known botanist, resident at Providence, Rhode Island, has just published a list of the Algæ of Rhode Island, as collected and prepared by himself. In this he enumerates twenty-four species of melanosperms, or olive-coloured algæ; forty-four of rhodosperms, or red algæ; and twenty-five of the chlorosperms, or green algæ, making ninety-three species in all. The remaining forms, principally microscopic, enumerated by him, and including zygnemaceæ, desmideæ, and diatomaceæ, bring the number up to 189. Of most of these Mr. Olney possesses duplicates, which he will be happy to dispose of in exchange.

The second volume of the "Annals of the Dudley Observatory," edited by its director, G. W. Hough, has just made its appearance, and consists of a report of the meteorological observations made at the observatory from 1862 to 1871. Its value is enhanced by its embracing the hourly records of the barometer (automatically printed) for a continuous period of five years, made by means of a very efficient apparatus invented by the director, and now used in numerous places, among others, in the office of the Signal Service at Washington. An appendix to the report contains miscellaneous communications upon the galvanic battery, the total eclipse of the sun of August 2, 1869, and the meteoric showers of 1867, &c.; and the whole book must be considered a very valuable contribution to physical science.

Serious apprehensions have been excited at Nantwich in

Cheshire by the repeated landslips which have occurred there. For several winters in succession large surfaces of ground have fallen in, it is supposed on account of the withdrawal of the salt from the salt-mines. The slip which occurred this winter is on the same spot where similar occurrences happened twelve months, two, and four years ago. The pit is about 300 yards in circumference, and about 100 feet deep, and the sides are almost perpendicular. It is feared that if these subsidences continue the town itself will be threatened, and the attention of the Government has been called to them.

MR. W. LAIRD CLOWES, in a letter to a contemporary dated The Cottage, Pinner, Monday, Jan. 8, writes:—"To-night, between 8.15 and 8.30, I noticed three beautifully luminous atmospherical phenomena on the northern horizon. They all took the form of an arc of fire of between 8° and 10° in height, the first two happening within a minute of one another, and the last about eleven minutes after the second. There were a slight breeze and light clouds at the time." This was most probably an aurora borealis, but we have not seen any other account of it.

THE Trinity Board have established an electric light at the South Foreland lighthouse, which is situated between Dover and Deal. It was formally opened on New Year's Day by Sir Frederick Arrow, the Deputy-Master of the Trinity Board, in the place of Prince Arthur, who was prevented from being present. This lighthouse establishes a triangle of electric lights, the other two being at Dungeness and Cape Grisnez

THE accounts furnished by the *Boston Advertiser* from the captains and crews of the vessels of the whaling fleet lately destroyed or ice-bound in the Arctic Ocean concur in describing the presence of peculiar meteorological phenomena during the past season. The prevailing summer wind on the north-west coast of Alaska is from the north, and this works the ice off from the land and disperses it, while the north-westerly winds close it up on the shore. As the ice moves off, the ships generally work up by the land, and in that situation find whales in plenty. By the end of the season, when north-westerly winds are prevalent, the ice becomes so broken up and melted that it has ceased to be an element of danger, and the vessels are compelled to retire to the northward by heavy ice drifting along the coast from the north, and not from a threatened closing in upon the land. But this season the easterly winds were not so strong and constant as usual, and the ice that had gone off from shore returned in a heavy pack, so that it was impossible to get a ship through, or even to hold against it at anchor. The heavy ice-fields are all composed of fresh-water berg-ice, not floe-ice of salt-water. The bergs are not of the immense proportions seen in Greenland seas, but are solid enough to be equally dangerous, many masses being so heavy as to ground in ten fathoms of water.

ON Nov. 15 the town of Oran, the second city in the province of Salta, was destroyed by a series of earthquakes lasting nine hours. Very little life was lost, the first shocks being light. The inhabitants had time to flee to the open camp of Monte Video.

*SCIENTIFIC INTELLIGENCE FROM AMERICA**

ADVICES from Lieutenant G. M. Wheeler, United States Engineers, whose movements during the past year we have had frequent occasion to chronicle, announce his arrival at Tucson about Dec. 4, with the men and animals nearly exhausted. The trip from Prescott to Camp Apache had been very severe, on account of the snow and high winds on the Colorado plateau. During their exploration one party had been sent to the San Francisco mountains, and made the ascent of the principal peak. These mountains consist of three prominences, grouping in the form of a crater, the north-eastern rim being wanting. The principal peak was occupied as a topographical, barometrical, and photographic station. It is believed to be nearly 1,000 feet higher than the peak usually ascended; and Lieutenant Wheeler was of the opinion that his party was the first to occupy its summit. This, however, was a mistake, as Dr. Edward Palmer, of the Smithsonian Institution, made the ascent in 1870, and obtained a number of new species of plants and insects.—A document which has been for some years in preparation, and toward which much expectation has been directed by agriculturists, has just appeared from the Government press, namely, the Report of the Commissioner of Agriculture upon the Diseases of Cattle in the United States. About the middle of June, 1868, a disease broke out at Cairo, Illinois, among a number of Texas cattle, known as the Spanish fever, or the Texas cattle disease. In consequence of the rapid extension of this disease, very serious alarm was excited, and the services of Prof. John Gamgee, a distinguished English veterinarian, then in the United States, were secured by General Capron, the Commissioner of Agriculture, for the purpose of instituting a careful inquiry as to its cause, course, and methods of treatment. The Professor immediately visited the infected districts in Illinois, and in the spring of 1869 examined that part of Texas on or near the Gulf coast, where the transportation of the native cattle begins. In this last journey he was accompanied by Prof. Ravenel, of South Carolina, a specialist among the fungi, and whose particular object was to determine what part such plants played in the infection. Dr J. S. Billings and Dr. Curtis, of the army, were also associated in the inquiry, having special reference to the microscopic investigations. A second investigation by Prof. Gamgee, under the authority of the Commissioner of Agriculture, had reference to the subject of pleuro-pneumonia, in the course of which numerous microscopic observations were made by Dr. Woodward, of the Army Medical Museum. Full reports on these various subjects made by the different gentlemen are embodied in the volume referred to, which appears in quarto form, with numerous well-executed plates in chromo-lithography. It is also accompanied by a report by Mr. Dodge, the statistician of the Agricultural department, upon the history of this Texas cattle disease, also known as splenic fever, in which the devastations of this peculiar native malady are traced back into the eighteenth century. This report was considered by General Capron as simply preliminary, and further investigations are indicated as important. Among those especially mentioned are inquiries as to the best mode of arresting the contagion, and the proper way of transportation of the cattle northward. He thinks that a general law of the United States, in the interest of public health, of an enlightened humanity, and of the cattle trade, should regulate this traffic, not only throughout the Gulf States, but on the great routes throughout the country.—A valuable document lately issued by the Surgeon-General's Office at Washington, prepared by Dr. G. A. Otis, consists of a report of surgical cases treated in the army of the United States from 1865 to 1871, covering almost every possible variety of injury, whether by gun-shot wounds, lacerations, fractures, dislocations, amputations, &c. The report, which is a quarto of nearly 300 pages, is illustrated in the same excellent style as its predecessors, and the woodcuts are especially worthy of all praise.—Bills have been introduced both in the Senate and House of Representatives providing for the reservation of that portion of the region about the Yellow Stone Lake, in which the wonderful geysers and hot springs occur, to which we have repeatedly called the attention of our readers. The thorough exploration of that country made during the past season by Dr Hayden has enabled him to define the limits within which these natural features occur, and the bill is based upon a plan prepared under his direction. The area proposed to be preserved is about sixty five miles in length by fifty-five in width, and it is suggested that the reservation be placed under the direction of the Secretary of the Interior, who is to be empowered to take such steps as may be required to protect the natural curiosities from injury or destruction. It is highly important that this should become a law at the present session, as the glowing accounts given by Dr Hayden will cause a great many persons to visit the country during the coming year, and with the natural iconoclasm of the Anglo-Saxon race, there is great danger that the wonderful water basins and formations of sulphur and of calcareous and siliceous rocks will be knocked to pieces for the purpose of securing mementoes of a visit.

* Communicated by the Scientific Editor of *Harper's Weekly.*

*THE LAWS OF ORGANIC DEVELOPMENT**

THE discussion of this subject divides itself into two parts, viz.: a consideration of the proof that evolution of organic types, or descent with modification, has taken place; and, secondly, the investigation of the laws in accordance with which this development has progressed.

I.—*On the Proof for Evolution.*

There are two modes of demonstration, both depending on direct observation. One of these has been successfully presented by Darwin. He has observed the origin of varieties in animals and plants, either in the domesticated or wild states, and has shown, what had been known to many, the lack of distinction in the grades of difference which separate varieties and species. But he has also pointed out that species (such, so far as distinctness goes) have been derived from other species among domesticated animals, and he infers by induction that other species, whose origin has not been observed, have also descended from common parents. So far I believe his induction to be justified: but when from this basis evolution of divisions defined by important structural characters, as genera, orders, classes, &c., is inferred, I believe that we do not know enough of the uniformity of nature's processes in the premises to enable us so regard this kind of proof as conclusive.

I therefore appeal to another mode of proving it, and one which covers the case of all the more really structural features of animals and plants.

It is well known that in both kingdoms, in a general way, the young stages of the more perfect types are represented or imitated with more or less exactitude by the adults of inferior ones. But a true identity of these adults with the various stages of the higher has, comparatively, rarely been observed. Let such a case be supposed.

In *A* we have four species whose growth attains a given point, a certain number of stages having been passed prior to its termination or maturity. In *B* we have another series of four (the numbering a matter of no importance), which, during the period of growth, cannot be distinguished by any common, *i.e.*, generic character, from the individuals of group *A*, but whose growth has only attained to a point short of that reached by those of group *A* at maturity. Here we have a parallelism, but no true evidence of descent. But if we now find a set of individuals belonging to one species, and therefore held to have had a common origin or parentage (or still better the individuals of a single brood), which present differences among themselves of the character in question, we have gained a point. We know in this case that the individuals *a*, have attained to the completeness of character presented by group *A*, while others, *b*, of the same parentage, have only attained to the structure of those of group *B*. It is perfectly obvious that the individuals of the first part of the family have grown further, and, therefore, in one sense faster, than those of group *b*. If the parents were like the individuals of the more completely grown, the offspring which did not attain that completeness may be said to have been *retarded* in their development. If, on the other hand, the parents were like those less fully grown, then the offspring which have added something have been *accelerated* in their development.

I claim that a consideration of the uniformity of nature's processes, or inductive reasoning, requires me (however it may affect the minds of others) to believe that the groups of species whose individuals I have never found to vary, but which differ in the same point as those in which I have observed the above variations, are also derived from common parents, and the more advanced have been accelerated or the less advanced retarded, as the case may have been with regard to the parents.

This is not an imaginary case, but a true representation of many which have come under my observation. The developmental resemblances mentioned are universal in the animal and probably in the vegetable kingdoms, approaching the exactitude above depicted in proportion to the near structural similarity of the species considered.

II.—*On the Laws of Evolution.*

Wallace and Darwin have propounded as the cause of modification in descent their law of natural selection. This law has been epitomised by Spencer as the "preservation of the fittest." This neat expression no doubt covers the case, but it leaves the origin of the fittest entirely untouched. Darwin assumes a "tendency to variation" in nature, and it is plainly necessary to do this in order that materials for the exercise of a selection should exist. Darwin and Wallace's law is, then, only restrictive, directive, conservative, or destructive of something already created. Let us, then, seek for the originative laws by which these subjects are furnished—in other words, for the causes of the origin of the fittest.

The origin of new structures which distinguish one generation from those which have preceded it, I have stated to take place under the law of acceleration. As growth (creation) of parts usually ceases with maturity, it is entirely plain that the process of acceleration is limited to the period of infancy and youth in all animals. It is also plain that the question of growth is one of nutrition, or of the construction of organs and tissues out of protoplasm.

The construction of the animal types is restricted to two kinds of increase—the addition of identical segments and the addition of identical cells. The first is probably to be referred to the last, but the laws which give rise to it cannot be here explained. Certain it is that segmentation is not only produced by addition of identical parts, but also by subdivision of a homogeneous part. In reducing the vertebrate or most complex animal to its simplest expression, we find that all its specialised parts are but modifications of the segment, either simply or as sub-segments of compound but identical segments. Gegenbaur has pointed out that the most complex limb with hand or foot is constructed, first, of a single longitudinal series of identical segments, from each of which a similar segment diverges, the whole forming parallel series, not only in the oblique transverse, but generally in the longitudinal sense. Thus the limb of the Lepidosiren represents the simple type, that of the Ichthyosaurus a first modification. In the latter the first segment only (femur or humerus) is specialised, the other pieces being undistinguishable. In the Plesiosaurian paddle the separate parts are distinguished; the ulna and radius well marked, the carpal pieces hexagonal, the phalanges well marked, &c.

As regards the whole skeleton, the same position may be safely assumed. Though Huxley may reject Owen's theory of the vertebrate character of the segments of the cranium, because they are so very different from the segments in other parts of the column, the question rests entirely on the definition of a vertebra. If a vertebra be a segment of the skeleton, of course the skull is composed of vertebræ; if not, then the cranium may be said to be formed of "sclerotomes," or some other name may be used. Certain it is, however, that the parts of the segments of the cranium may be now more or less completely parallelised or homologised with each other, and that as we descend the scale of vertebrated animals, the resemblance of these segments to vertebræ increases, and the constituent segments of each become more similar. In the types where the greatest resemblance is seen, segmentation of either is incomplete, for they retain the original cartilaginous basis. Other animals which present cavities or parts of a solid support are still more easily reduced to a simple basis of segments, arranged either longitudinally (worm) or centrifugally (star-fish, &c.).

Each segment—and this term includes not only the parts of a complex whole, but parts always subdivided, as the jaw of a whale or the sac-body of a mollusc—is constructed, as is well known, by cell-division. In the growing fœtus the first cell divides its nucleus and then its whole outline, and this process repeated millions of times produces, according to the cell theory, all the tissues of the animal organism or their bases from first to last. That the ultimata or histological elements of all organs are produced originally by repetitive growth of simple, nucleated cells with various modifications of exactitude of repetition in the more complex, is taught by the cell theory. The formation of some of the tissues is as follows:—

First Change—Formation of simple nucleated cells from homogeneous protoplasm or the cytoblastema.

Second—Formation of new cells by division of body and nucleus of the old.

Third—Formation of tissues by accumulation of cells with or without addition of intercellular cytoblastema.

A. In connective tissue by slight alteration of cells and addition of cytoblastema.

B. In blood, by addition of fluid cytoblastema (fibrin) to free cells (lymph corpuscles), which in higher animals (vertebrates) develop into blood-corpuscles by loss of membrane, and by cell development of muscles.

* Abstract of paper by Prof. E. D. Cope, read at the Indianapolis meeting of the American Association for the Advancement of Science: reprinted from the *American Naturalist.*

C. In muscles by simple confluence of cells, end to end, and mingling of contents (Kölliker).

D. Of cartilage by formation of cells in cytoblast which break up, their contents being added to cytoblast; this occurring several times, the result being an extensive cytoblast with few and small cells (Vogt). The process is here an attempt at development with only partial success, the result being a tissue of small vitality.

Even in repair-nutrition recourse is had to the nucleated cell. For Cohnheim first shows that if the corner of a frog's eye be scarified, repair is immediately set on foot by the transportation thither of white or lymph or nucleated corpuscles from the neighbouring lymph heart. This he ascertained by introducing aniline dye into the latter. Repeated experiments have shown that this is the history in great part of the construction of new tissue in the adult man.

Now, it is well known that the circulating fluid of the fœtus contains for a period only these nucleated cells as corpuscles, and that the lower vertebrates have a greater proportion of these corpuscles than the higher, whence probably the greater facility for repair or reconstruction of lost limbs or parts enjoyed by them. The invertebrates possess only nucleated blood corpuscles.

What is the relation of cell division to the forces of nature, and to which of them as a cause is it to be referred, if to any? The animal organism transfers the chemism of the food (protoplasm) to correlated amounts of heat, motion, electricity, light (phosphorescence), and nerve force. But cell division is an affection of protoplasm distinct from any of these. Addition to homogeneous lumps or parts of protoplasm (as in that lowest animal, *Protamœba* of Haeckel) may be an exhibition of mere molecular force, or addition as is seen in the crystal, but cell division is certainly something distinct. It looks to me like an exhibition of another force, and though this is still an open question, it may be called for the present *growth force*. It is correlated to the other forces, for its exhibitions cease unless the protoplasm exhibiting it be fed. It is potential in the protoplasm of both protoplasmic animal mass and protoplasmic food, and becomes energetic on the union of the two. So long as cell-division continues it is energetic; when cells burst and discharge the contained cytoblastema, as in the formation of cartilage, it becomes again potential.

The size of a part is then dependent on the amount of cell division or growth force, which has given it origin, and the number of segments is due to the same cause. The whole question, then, of the creation of animal and vegetable types is reduced to one of the amount and location of growth force.

Before discussing the influences which have increased and located growth force, it will be necessary to point out the mode in which these influences must necessarily have affected growth. Acceleration is only possible during the period of growth in animals, and during that time most of them are removed from the influence of physical or biological causes either through their hidden lives or incapacity for the energetic performance of life functions. These influences must, then, have operated on the parents, been rendered potential in their reproductive cells, and become energetic in the growing fœtus of the next generation. However little we may understand this mysterious process, it is nevertheless a fact. Says Murphy, "There is no act which may not become habitual, and there is no habit which may not be inherited." Materialised, this may be rendered—there is no act which does not direct growth force, and therefore there is no determination of growth force which may not become habitual; there is, then, no habitual determination of growth force which may not be inherited; and of course in a growing fœtus becomes at once energetic in the production of new structure in the direction inherited, which is acceleration.

III.—*The Influences Directing Growth Force.*

Up to this point we have followed paths more or less distinctly traced in the field of nature. The positions taken appear to me either to have been demonstrated or to have a great balance of probability in their favour. In the closing part of these remarks I shall indulge in more of hypothesis than heretofore.

What are the influences locating growth force? First, physical and chemical causes; second, use; third, effort. I leave the first, as not especially prominent in the economy of type growth among animals, and confine myself to the two following. The effects of use are well known. We cannot use a muscle without increasing its bulk; we cannot use the teeth in mastication without inducing a renewed deposit of dentine within the pulp-cavity to meet the encroachments of attrition. The hands of the labourer are always larger than those of men of other pursuits. Pathology furnishes us with a host of hypertrophies, exostoses, &c., produced by excessive use, or necessity for increased means of performing excessive work. The tendency, then, induced by use by the parent is to add segments or cells to the organ used. Use thus determines the locality of new repetitions of parts already existing, and determines an increase of growth force at the same time, by the increase of food always accompanying increase of work done, in every animal.

But supposing there be no part or organ to use. Such must have been the condition of every animal prior to the appearance of an additional digit or limb or other useful element. It appears to me that the cause of the determination of growth force is not merely the irritation of the part or organ used by contact with the objects of its use. This would seem to be the remote cause of the deposit of dentine used in the tooth, in the thickening epidermis of the hand of the labourer, in the wandering of the lymph-cell to the scarified cornea of the frog in Cohnheim's experiment. You cannot rub the sclerotica of the eye without producing an expansion of the capillary arteries and corresponding increase in the amount of nutritive fluid. But the case may be different in the muscles and other organs (as the pigment cells of reptiles and fishes) which are under the control of the volition of the animal. Here, and in many other instances which might be cited, it cannot be asserted that the nutrition of use is not under the direct control of the will through the mediation of nerve force. Therefore I am disposed to believe that growth force may be, by the volition of the animal, as readily determined to a locality where an executive organ does not exist, as to the first segment or cell of such an organ already commenced, and that therefore effort is in the order of time the first factor in acceleration.

Effort and use have, however, very various stimuli to their exertion.

Use of a part by an animal is either compulsory or optional. In either case the use may be followed by an increase of nutrition under the influence of reflex force or of direct volition.

A compulsory use would naturally occur in new situations which take place apart from the control of the animal, where no alternatives are presented. Such a case would arise in a submergence of land where land animals might be imprisoned on an island or in swamps surrounded by water, and compelled to assume a more or less aquatic life. Another case which has also probably often occurred, would be when the enemies of a species might so increase as to compel a large number of the latter to combat who would previously have escaped it.

In these cases the structure produced would be necessarily adaptive. But the effect would be most frequently to destroy or injure the animals (retard them) thus brought into new situations and compelled to an additional struggle for existence, as has, no doubt, been the case in geologic history. Preservation, with modifications, would only ensue where the changes should be introduced very gradually. This mode is always a consequence of the optional use. The cases here included are those where choice selects from several alternatives, thus exercising its influence on structure. Choice will be influenced by the emotions, the imagination, and by intelligence.

As examples of intelligent selection the modified organisms of the varieties of bees and ants must be regarded as striking examples of its exercise. Had all in the hive or hill been modified alike, as soldiers, queens, &c, the origin of the structures might have been thought to be compulsory; but varied and adapted as the different forms are to the wants of a community, the influence of intelligence is too obvious to be denied. The structural results are obtained in this case by a shorter road than by inheritance.

The selection of food offers an opportunity for the exercise of intelligence, and the adoption of means for obtaining it still greater ones. It is here that intelligent selection proves its supremacy as a guide of use, and consequently of structure, to all the other agencies here proposed. The preference for vegetable or for animal food determined by the choice of individual animals among the omnivores, which were, no doubt, according to the palæontological record the predecessors of our herbivores, and perhaps of carnivores also, must have determined their course of life, and thus of all their parts into those totally distinct directions. The choice of food under ground, on the ground, or in the trees would necessarily direct the uses of organs in those directions respectively.

Intelligence is a conservative principle, and always will direct effort and use into lines which will be beneficial to its possessor. Thus we have the source of the fittest—*i.e.*, addition of parts by increase and location of growth force directed by the will—the will being under the influence of various kinds of compulsory choice in the lower, and intelligent option among higher animals. Thus intelligent choice may be regarded as the originator of the fittest, while natural selection is the tribunal to which all the results of accelerated growth are submitted. This preserves or destroys them, and determines the new points of departure on which accelerated growth shall build.

Acceleration under the influence of effort accounts for the existence of rudimental characters. Many other characters will follow at a distance, the modifications proceeding in accordance with the laws here proposed, and retardation is accounted for by complementary or absolute loss of growth force.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, January 18.—"Investigations of the Currents in the Strait of Gibraltar, made in August 1871," by Captain G. S. Nares, R.N., of H.M.S. *Shearwater*, under instructions from Admiral Richards, F.R.S., Hydrographer of the Admiralty.

Geological Society, Jan. 10.—Mr. Joseph Prestwich, F.R.S., president, in the chair. "On Cyclostigma, Lepidodendron, and Knorria from Kiltorkan." By Prof. Oswald Heer. In this paper the author indicated the characters of certain fossils from the yellow sandstone of the South of Ireland, referred by him to the above genera, and mentioned in his paper "On the Carboniferous Flora of Bear Island," read before the Society on November 9, 1870 (see Q. J. G. S. vol. xxvii. p. 1). He distinguished as species *Cyclostigma kiltorkense*, Haught., *C. minutum* (Haught.), *Knorria acicularis*, Göpp. var. *Bailyana*, and *Lepidodendron Veltheimianum*, Sternb.—Mr. Carruthers was glad that he had made the observations which he did on Prof. Heer's former paper, as it had caused the Professor to give the reasons on which his opinions were based. He was doubtful whether the success which had attended Prof. Heer's determination of species from leaves justified the application of the same principles to mere stems. He could not accept the difference in size or distance of leaf-scars as a criterion of species, inasmuch as they were merely the result of the difference in the age and size of the parts of the plants on which they were observed. Even Prof. Heer himself had united together specimens presenting greater differences in this respect than those which he distinguished. He considered *Cyclostigma kiltorkense*, *C. minutum*, and *Lepidodendron Veltheimianum* to be founded on different parts of one species. In the Kiltorkan fossils the outer surface of the original stems was often broken up into small fragments, the phyllotaxy on which proved them to be portions of large stems, and not entire branches. As to *Knorria*, it was certainly the interior cast of the stem of *Lepidodendron*, with casts of the channels through which the vascular bundles passed with some cellular tissue to the leaves; and the specimen figured showed that it belonged to a branch similar to that represented as *C. minutum*. He considered that the four supposed species belonging to three genera were only different forms of the same plant.—"Notes on the Geology of the Plain of Marocco, and the Great Atlas." By Mr. George Maw. The author described first the characters presented by the coast of Marocco, and then the phenomena observed by him in his progress into the interior of the country and in the Atlas Chain. The oldest rocks observed were ranges of metamorphic rocks bounding the plain of Marocco, interbedded porphyrites and the porphyritic tuffs forming the backbone of the Atlas Chain, and the Mica-schists of Djeb Tezah in the Atlas. At many points in the lateral valleys of the Atlas almost vertical grey shales were crossed; the age of these was unknown. Above these comes a Red Sandstone and Limestone series, believed to be of Cretaceous age, and beds possibly of Miocene age, which occupied the valleys of the Atlas and covered the plain of Marocco, where vestiges of them remain in the form of tabular hills. The probable age of these beds was determined on the evidence of fossils. The author noticed the sequence of denuding and eruptive phenomena by which the arrangement and distribution of these rocks has been modified, and described the more recent changes resulting in the formation of enormous boulder-beds flanking the northern escarpment of the Atlas plateau, and of great moraines at the heads of the valleys of the Atlas, both of which he ascribed to glacial action. An elevation of the coast line of at least seventy feet was indicated by raised beaches of concrete sand at Mogador and elsewhere, and the author considered that a slight subsidence of the coast was now taking place. The surface of the plain of Marocco was described as covered with a tufaceous crust, probably due to the drawing up of water to the surface from the subjacent calcareous strata and the deposition from it of laminated carbonate of lime. Mr. Ball, as an Alpine traveller who had also visited the Atlas in company with Dr. Hooker and Mr. Maw, offered a few remarks. The plain of Marocco was not, in his opinion, a level, but an inclined plane, rising gradually in height up to the foot of the mountain, so that the base of the boulder ridges was at some height above the level of the plain near Marocco. He did not think that the boulder deposits could be safely attributed to glaciers, but thought rather that they had been carried into and deposited in a shallow sea. He thought also that Mr. Maw had somewhat over-estimated the thickness of some of the boulder deposits; and though there was one instance of an undoubted moraine in one of the higher valleys of the Atlas, yet he could not agree in the view that the glaciation of the Atlas was general. He could not accept such a great thickness of beds as that represented by the vertical shales in Mr. Maw's section. Prof. Ramsay was pleased that the author, though giving so many interesting details, had not assigned any definite age to many of the beds. He agreed with him as to the cause assigned for the great tufaceous coating of the country. He had already assigned the same cause for the existence of certain saline beds, and would attribute the existence of the great coating of gypsum at slight depth below the surface of the Sahara to the same cause. As to the existence of moraines, he was not surprised to find them in the Atlas, as they were already known in the mountains of Granada. As to the escarpments, it was now well known that, as a rule, they assumed a direction approximately at right angles to the dip of the strata; and he felt inclined to consider that the bulk of the mounds at the foot of the escarpment of the Atlas were rather the remains of a long series of landslips from the face of the cliffs than to an accumulation of moraine matter. Mr. D. Forbes commented on the similarity of the rocks to those of the Andes in South America. In the Andes the porphyritic tuffs appeared to belong to the Oolitic age; and the igneous rocks associated with them were of the same date. He thought that, so far as the author's observations had gone, the structure of the Atlas was much the same as that of the Andes. Mr. W. W. Smyth mentioned that in the district to the east of the Sierra Nevada, in the south part of Spain, where there was great summer heat, and also heavy occasional rainfall, the same tufaceous coating as that observed in Marocco was to be found. He had been led to much the same conclusion as to its origin as that arrived at by Mr. Maw. The upper part was frequently brecciated, and the fragments re-cemented by carbonate of lime. Mr. Seeley, though accepting Mr. Etheridge's determination as to the Cretaceous age of the fossils if found in England, could not accept it as conclusive in the case of fossils from Marocco. The genius *Exogyra*, for instance, which ranges through the Secondary to existing seas, might well belong to some other age; and even the fossils presumably Miocene might, after all, date from some other period. Mr. Maw, in reply, stated that he agreed with Mr. Ball as to the rise in the Marocco plain as it approached the Atlas, having taken it in one direction at 400 feet in 25 miles. He pointed out the resemblance between the moraines in the valley of the Rhone and those which he regarded as such on the flanks of the Atlas. As a proof of their consisting of transported blocks, he mentioned the fact that the Red Sandstone rock of which they were composed did not occur in the adjacent escarpments, but was not to be found within seven or eight miles There was, moreover, a mixture of different materials in the mounds.

Linnean Society, January 18.—Mr. Bentham, president, in the chair. "On the Anatomy of *Limulus polyphemus*," by Prof. Owen (continued). The author resumed and concluded the reading of this memoir. The nervous system of *Limulus* appeared to have occupied most attention, and was described in detail. From the fore part of the œsophageal ring, answering to the brain, were sent off the "ocellar," "ocular," "antennular," and "antennal" nerves; the latter supplying the second pair of articulate limbs—the homologues of the "external antennæ" of higher *Crustacea*. From the post- or sub-œsophageal part of the ring, proceeded large nerves to the four succeeding pairs of limbs; and also smaller nerves, having distinct origins, to the chilaria and to the opercular plate limbs. The neural axis then continues, as a pair of coalesced chords, to the middle of the thoracetron, developing five ganglions supplying the five gill-limbs. Beyond the fifth ganglion the chords separate; each forms a loop resembling a ganglion, beyond which each chord penetrates the base of the "pleon." To this it supplies five dorsal and five ventral nerves before being continued and resolved into a plexus toward the end of the tail and spine. The author remarked that, as the nervous system preceded the tegumentary in the order of development, it might thus manifest evidences of the more generalised segmental type of the pleon, more plainly than had been noticed in the formation of the chitinous walls of that division of the body, in the embryo, in which it first budded forth as a ninth segment of the thoracetron. Details of the organs of the senses, of the digestive, circulatory, respiratory, and generative systems were then given, and illustrated, like the nervous system, by minutely-finished drawings. The heart was elongate, vasiform, included in a pericardial-like sinus: besides an anterior and posterior aortic trunk, there were seven pairs of lateral primary branches. The arteries soon lose their tubular form, and, as they expand, lose likewise much of their fibrous walls, and seem reduced to delicate membranous sinuses which follow the shapes of the parts or interstices along which the blood meanders as it returns by the venous sinuses to the general pericardial one. The most remarkable of the arterial prolongations are those which the author had previously described in his "Lectures on Invertebrata" (8vo ed., 1855, p. 310) as expanding upon, and seeming to form the neurilemma of, the central axis and branches of the nervous system; so that injection of the anterior aorta coats the neurine and demonstrates a great part of the nervous system by its colour. (A drawing showing this effect of fine red injection was exhibited.) Finally the author cited the chief results of the observations of Lockyer, Packard, and Dohrn on the development of the king-crab. There was neither a nauplius stage nor a trilobite stage. A superficial resemblance to trilobites is shown by the absence of the pleon in the embryo king-crab; but the very fact of the late appearance of this terminal division was decisive against any real representative resemblance of the embryo *Limulus* to the trilobites; on the acceptance, at least, of Barrande's observations of the successive and later appearance of the segments of the "thoracetron" in the space between the head ("cephaletron") and "pygidium" (pleon and tail-spine) of the embryos of *Sao*, *Agnostus*, and *Trinucleus*. The author here recalled attention to Newport's observations of the like development of successive segments, anterior to the caudal one, in Iulidæ, and remarked that with other facts noted in the anatomical sections, especially the fusion of the pair of cephalic ganglia, and the short and thick crura connecting these with the subœsophageal mass, giving the condition of that part of the nervous system in *Scorpio* and *Iulus*, *Limulus* manifested in an instructive and interesting way the more "generalised type" of articulate structure, in which arachnidan and myriapodal characters were associated with crustaceous ones. But, in the development of *Limulus*, the pleon, pygidium, or tail-spine was the last to appear, and, at its first budding, looked like a ninth segment of the thoracetron. Packard speaks of indications, transitory indeed, of segmentation of the crust; and such indications, as the author had shown in the anatomy of *Limulus*, were more strongly and lastingly given by the nervous system. The tail-spine belongs to the series of body-segments, and is no mere appendage to the dorsal arc of such. After formifaction and the attractive and repellent forces have produced in the germ-masses the phenomena of segmentation and vegetative repetition, as manifested in the similar and parallel heaps of granules, like bricks for the building, the inherited influences overrule the polaric ones, and operate in differentiating and adaptive lines, speedily showing the embryo *Limulus*, which, like that of *Astacus fluviatilis*, *Palæmon adspersus*, *Crangon maculosus*, *Eriphia spinifrons*, and one may add, all Cephalopods, takes its own course to the full manifestation of its specific characters, agreeably with the nature originally impressed on the germ. There was no divergence to a larval form with a term of active life as such; there was no metamorphosis, either "nauplial" or "trilobitic." Some objected to the king-crabs being called Crustacea; there was more ground, the author thought, for objecting to call them Arachnida or Myriapoda. Characters common to *Limulus* with their allied extinct gill-bearing, well limbed Articulata, have not a class-value. The author could not, at least, raise the Merostomes to an equivalency with, and run them parallel to and alongside of, the rest of the branchiate Condylopods. A class, after all, was an artificial group, a help to the classifier. One may call *Limulus* a Crustacean and yet discern in its anatomy the evidence of its more "generalised structure" than in Malacostraca; its type preceded that of either macrourous or brachyurous Crustacea, and indicates characters subsequently appropriated by and intensified in the air-breathing members of the Apterous Insecta of Linnæus. As compared with its longer-bodied and many-jointed predecessors, *Limulus* itself shows a concentrative specialisation; but vegetative repetition still reigns in the limb-series. "Inner antennules," "outer antennæ," "mandibles," "maxillæ," "maxillipeds," "legs," all work together by their basal joints in subserviency to mastication, and all end in pincers. As compared with modern crabs no structure was more striking and significant than the resistance, so to speak, of the heart in *Limulus* to the concentrative tendencies; it is still the "dorsal vessel," though the body-part containing it has the breadth and shortness of the crab's carapace, in which the heart is shaped to match. In both the neural axis supplying the cephaletral limbs is annular, but in modern crabs the subœsophageal part is defined by distance and concomitantly long and slender from the super-œsophageal or cerebral part. This differentiation had not taken place in *Bellimurus*, *Neolimulus Prestwichia*, and other palæozoic predecessors of *Brachyura*, whose organisation we have to thank their long-lived, lingering representative genus for enabling us to peer into. That such glimpses, with concomitant tracing of the development of the individual *Limulus*, afford us some ground, and that the like work, with persevering quest of its palæozoic fossil allies, may afford more, for guessing at the ways in which a pre ordained plan of derivation by congenital departures from parental form has operated, in originating the various deviations from a common primitive articulate type, is an encouraging faith. That the old ocean should have given the chance conditions of origin of crustaceous sub-classes, orders, genera, species, by natural selection, was not conceivable by the author, who, nevertheless, held the conviction that all forms and grades of Articulata were due to "secondary cause or law," as strongly as when he expressed the same conclusion in regard to the Vertebrata, and termed it "the deep and pregnant principle" evolved in the researches on the general homologies and archetype of their skeletons.

Mathematical Society, January 11.—Mr. W. Spottiswoode, F.R.S., president, and subsequently Prof. Cayley, V.P., in the chair. Major E. Close, R.A., was admitted into the society. Prof. Cayley gave an account of his paper "On the Surfaces the loci of the Vertices of Cones which satisfy six conditions."—Mr. J. W. L. Glaisher stated and illustrated the principal points in his communication "On the Constants which occur in certain summations by Bernouilli's Series.—Mr. W. B. Davis read a paper describing the methods he had used in the construction of tables of divisors, and exhibited tables of factors of numbers consisting of nine and twelve figures. A brief discussion ensued on the subject of this communication.—Mr. Roberts explained some of the results which he submitted to the society in his paper "On the parallel surface of Conicoids and Conics," and illustrated the same by means of a model and drawings of sections of one of the surfaces.

Zoological Society, January 16.—Prof. Newton, F.R.S., vice-president, in the chair.—The Secretary read a report on the additions that had been made to the Society's collection during the month of December, 1871, amongst which was particularly mentioned a young Prince Alfred's Deer (*Cervus alfredi*), born in the Gardens —A letter was read from Prof. Owen, F.R.S., communicating some particulars received from Dr. Julius Haast, of Christchurch, New Zealand, respecting the finding of the remains of *Aptornis* in the Glenmark Swamp, New Zealand.—Mr. H. E. Dresser exhibited and made remarks on specimens of the eggs of *Reguloides superciliosus* and *Reguloides occipitatis*, collected by Mr. W. E. Brooks in Cashmere.—A communication was read from Dr. G. Hartlaub and Dr. O. Finsch, giving an account of a collection of birds from the Pelew and Mackenzie Islands in the Pacific, to which was added a complete synopsis of the ornithology of this portion of the Caroline group.—A communication was received from Mr. A. Sanders, containing a complete description of the Myology of *Liolepis belli*.—Mr. A. G. Butler communicated a synomic list of the species formerly included in the genus *Pieris*, with references to all others described since the subdivisions of that genus by recent authors.—A communication was read from Mr. John Brazier, of Sydney, N.S.W., giving a list of the *Cypræa* met with on the coast of New

South Wales.—A paper by Mr. A. Anderson was read containing the second portion of his notes on the Raptorial Birds of India.

Chemical Society, January 18.—Dr. Frankland, F.R.S., president, in the chair.—At this meeting Dr. Odling exhibited some very fine specimens of rare metals and their compounds, which had been lent to him by Dr. Richter and Dr. Theodor Schuchardt. Among these was a bar, weighing about seven ounces, of metallic indium; an element discovered a few years ago by Richter, in conjunction with Reich; also some metallic rubidium.—Dr. David Howard then read an interesting paper "On quinicine and cinchonicine and their salts." These alkaloids are prepared artificially, from quinine and cinchonine respectively, by the action of heat on their salts, and are isomeric with them. Quinicine occurs along with the two last-mentioned alkaloids in cinchona bark, being apparently the one which is first formed during the growth of the cinchona plant.

Paris

Academy of Sciences, January 15.—A note by M. M. Lévy on a property of the focals of surfaces, was presented by M. Bertrand, in which the author puts forward the proposition that any surface and its focal intersect each other at right angles.—A note from M. Catalan, on General Didion's communication concerning the relation of the circumference to the diameter, was read, in which the authorship of similar formulæ is ascribed to Euler.—M. H. Resal communicated a memoir containing equations of the vibratory movement of a circular plate, and M. Serret a note by M. E. Ciotti on the employment of vibrating elastic plates for the realisation of a propeller, in connection with a recent communication from M. de Tastes.—A memoir on the measurement of very high temperatures, and on the temperature of the sun, by M. H. Sainte-Claire Deville, was read. The author maintained that the temperatures which may be produced and measured in the laboratory are not greatly exceeded in nature, and that the temperature of the sun is not far from 2,500—2,800° C. (= 4,532—5,072° F).—M. Delaunay read a note on the secular variations of the mean movements of the perigee and node of the moon.—M. Faye presented a note upon the investigations of Dr. Heis on meteors, which are confirmatory of M. Faye's previous communication as to the different centres of radiation observed in November last.—A letter was read from M. Janssen on the principal consequences which may be drawn at present from his observations of the eclipse of December last. (A translation of this letter will be found in another column.)—M. P. Guyot forwarded a note on a meteor observed at Nancy on the 20th of December last at 10h. 28m. A.M. This meteor passed from Cassiopeia through Perseus towards the Pleiades, near which it exploded, with a bright green light.—M. E. Becquerel presented a report on various memoirs by M. W. de Fonvielle regarding observations to be effected during balloon ascents. M. E. Becquerel also presented a note by M. T. Sidot on the electrisation by friction of metals in sulphide of carbon, and on the decomposition of that body by light. The author finds that certain metals, especially silver, aluminium, and iron, become electrised, and produce sparks when strongly agitated with pure sulphide of carbon, and that the latter, when exposed to the light of the sun, is decomposed, producing a gas and a solid flocculent matter. The same gentleman also communicated a joint note by MM. F. Lucas and A. Cazin containing an account of some experimental researches upon the duration of the electric spark.—Notes by M. Lion and M. Diamilla Müller on the action of ecliptical conjunctions upon the elements of terrestrial magnetism were read. According to the former considerable perturbations were observed at Alençon during the eclipse of the 11th December last.—M. Tarry presented a further note on the movement of recoil of cyclones in equatorial regions.—In a paper on the combustion of carbon by oxygen, M. Dumas showed, in opposition to M. Dubrunfaut, that carbon is combustible in perfectly dry oxygen.—M. Chevreul made some remarks on this paper.—A note by MM. L. Dusart and C. Bardy on the transformation of phenole into alkaloids was presented by M. Cahours. The authors have obtained phenylamine, chloride of phenyle, and diphenylamine by the action of hydrochlorate of ammonia and fuming hydrochloric acid upon phenole.—M. P. Barbier announced his having produced cymene by treating hydrate of essence of turpentine with bromine.—A letter was read from M. V. Meyers on the reaction between sulphur and aqueous vapour in the synthesis of sulphuric acid, and on the preparation of pure zinc by electrolysis.—An important discussion on the vexed question of spontaneous generation was raised by the reading of some reflections concerning heterogenesis by M. A. Trécul. In the discussion MM. Balard, Fremy, and Blanchard took part.—A somewhat cognate matter was also treated by M. A. Béchamp in his paper on the cause of alcoholic fermentation by beer-yeast, and on the formation of leucine and tyrosine in this fermentation.—M. C. Robin presented a note by M. S. Chantran on the fecundation of the crayfish, in which the author describes the impregnation of the ova as taking place after their expulsion from the oviducts.—A note by MM. E. Mathieu and V. Urbain on the gases of the blood, was presented by M. Cahours.

DIARY

THURSDAY, January 25.

Royal Society, at 8.30.—On the Absolute Direction and Intensity of the Earth's Magnetic Force at Bombay: C. Chambers, F.R.S.—On the Elimination of Alcohol: Dr. Dupré.—On the Action of Low Temperatures on Supersaturated Solutions of Glauber's Salt: C. Tomlinson, F.R.S.
Society of Antiquaries, at 8.30.—Miscellaneous Communications on Objects of Mediæval Antiquity.

FRIDAY, January 26.

Royal Institution at 9.—On the Demon of Socrates: Archbishop of Westminster.
Quekett Microscopical Club, at 8.

SATURDAY, January 27.

Royal Institution, at 3.—On the Theatre in Shakespeare's Time: Wm. B. Donne.

SUNDAY, January 28.

Sunday Lecture Society, at 4.—On Ice, as a Geological Agent: A. H. Green.

MONDAY, January 29.

London Institution, at 4.—Elementary Chemistry: Prof. Odling, F.R.S.
Royal United Service Institution, at 8.30.—On Modern Ships of War, as illustrated by the Models in the Institution: Nathaniel Barnaby.

TUESDAY, January 30.

Royal Institution, at 3.—On the Circulatory and Nervous Systems: Dr. W. Rutherford, F.R.S.E.

WEDNESDAY, January 31.

Society of Arts, at 8.—On Individual Providence for Old Age as a National Question: G. C. T. Bartley.

THURSDAY, February 1.

Royal Institution, at 3.—On the Chemistry of Alkalies and Alkali Manufacture; Prof Odling, F.R.S.
Royal Society, at 8.30.
Society of Antiquaries, 8.30.
Linnean Society, at 8.—On the Classification and Geographical Distributions of Compositæ: The President.
Chemical Society, at 8.

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NOTICE

THURSDAY, FEBRUARY 1, 1872

THE INTERNAL FLUIDITY OF THE EARTH

WE have been favoured with permission to reprint the following extract from a letter addressed by Sir Wm. Thomson to Mr. G. Poulett Scrope:—

The University, Glasgow, *Jan.* 15, 1872

DEAR SIR,—I thank you very much for the copy of your beautiful book on Volcanoes, which you have been so kind as to send me through Professor Geikie. It is full of matter most interesting to me, and I promise myself great pleasure in reading it.

I see with much satisfaction, in your prefatory remarks, that you "earnestly protest against the assertion of some writers, that the theory of the internal fluidity of the globe is or ought to be generally accepted by geologists on the evidence of its high internal temperature." Your sentence upon the "attractive sensational idea that a molten interior to the globe underlies a thin superficial crust; its surface agitated by tidal waves and flowing freely towards any issue that may here and there be opened for its outward escape," in which you say that you "do not think it can be supported by reasoning, based on any ascertained facts or phenomena," is thoroughly in accordance with true dynamics. It will, I trust, have a great effect in showing that volcanic phenomena, far from being decisive, as many geologists imagine them to be, in favour of a thin crust enclosing a wholly liquid interior, tend rather, the more thoroughly they are investigated, to an opposite conclusion.

I must, however, take exception to your next sentence, that in which you say that "M. Delaunay has disposed of the well-known astronomical argument of Mr. Hopkins and Sir W. Thomson, as to the entire or nearly entire solidity of the earth, derived from the nutation of its axis." Delaunay's deservedly high reputation as one of the first physical astronomers of the day, has naturally led many in this country to believe that his objection to the astronomical argument in favour of the earth's rigidity cannot but be valid. It has even been hastily assumed that the objection is founded on mathematical calculation, an error which the most cursory reading of Delaunay's paper corrects. His hypothesis of a viscous fluid breaks down utterly when tested by a simple calculation of the amount of tangential force required to give to any globular portion of the interior mass the precessional and nutational motions, which, with other physical astronomers, he attributes to the earth as a whole. Thus: taking the ratio of polar diameter to equatorial diameter as 299 to 300, and the density of the upper crust as half the mean density of the earth, I find (from the ordinary elementary formulæ) that when the moon's declination is $28°\frac{1}{2}$, the couple with which she tends to turn the plane of the earth's equator towards the plane of her own centre and the equinoctial line has for its moment a force of $\cdot 285 \times 10^{18}$ times the gravity of one gramme at the earth's surface, or rather more than a quarter of a million million tons weight, on an arm equal to the earth's radius. A quadrant of the earth being ten thousand kilometres, the area is five hundred and nine million square kilometres, or 5·09 million million million square centimetres. Hence a force of $\cdot 285 \times 10^{18}$ grammes weight distributed equally over two-thirds of the earth's area would give ·084 of a gramme weight per square centimetre. This supposition is allowable (for reasons with which I need not trouble you) in estimating roughly the greatest amount of tangential force acting between the upper crust and a spherical interior mass in contact with it, from the preceding accurate calculation of the whole couple exerted by the moon on the upper crust. It is thus demonstrable that the earth's crust must, as a whole, down to depths of hundreds of kilometres, be capable of transmitting tangential stress amounting to nearly $\frac{1}{10}$ of a gramme weight per square centimetre. Under any of such stress as this any plastic substance which could commonly be called a viscous fluid would be drawn out of shape with great rapidity. Stokes, who discovered the theory of fluid viscosity, and first made accurate investigations of its amount in absolute measure, found that a cubic centimetre of water, if exposed to tangential force of the millionth part of $\frac{1}{10}$ of a gramme weight on each of four sides, would even under so small a distorting stress as this, become distorted so rapidly that at the end of a second of time its four corresponding right angles would become one pair of them acute and the other obtuse, by as much as a two-hundredth part of the angle whose arc is radius, that is to say by ·29 of a degree. Not as much as a ten-million-millionth part of this distortion could be produced every second of time by the lunar influence in the material underlying the earth's crust without very sensibly affecting precession and nutation; for the effect of the maximum couple exerted on the upper crust by the moon is to turn the whole earth in a second of time through an angle of a one-hundred-million-millionth of ·57 of a degree, so as to give to it at the end of a second the position obtained by geometrically compounding this angular displacement with the angular displacement due simply to rotation. Hence the viscosity assumed by Delaunay, to produce the effect he attributes to it, must be more than ten million million million times the viscosity of water. How much more may be easily estimated with some degree of precision from Helmholtz's mathematical solution of the problem of finding the motion of a viscous fluid contained in a rigid spherical envelope urged by periodically varying couples.* The most interesting part of the application of this solution to the hypothetical problem regarding the earth, is to find how rapidly the obliquity of the ecliptic would be done away with by any assumed degree of viscosity in the interior; such an amount of viscosity, for example, as would render the excesses of precession and nutation above their values for a perfectly rigid interior, not greater than observation could admit.

The hypothesis of a continuous internal viscous fluid being disposed of, the question occurs, what rigidity must the interior mass have, even if enclosed in a perfectly rigid crust, to produce the actual phenomena of precession and nutation? The solutions given by Lamé and myself of the problem of the vibrations of an elastic solid globe, may be readily applied to determine the influences on precession and on the several nutations, which would be produced by elastic yielding with any assumed rigidity

* Helmholtz and Piotrowski, "Ueber Reibung tropfbarer Flüssigkeiten," Imp. Acad. Vienna, 1860.

short of infinite rigidity. This application I have no time at present to make; but without attempting a rigorous investigation, it is easy roughly to estimate an inferior limit to the admissable rigidity. In the first place, suppose, with perfect elasticity, the rigidity be so slight that distorting stress of $\frac{1}{10}$ of a gramme weight would produce an angular distortion of a half degree or a degree. The whole would not rotate as a rigid body round one "instantaneous axis" at each instant, but the rotation would take place internally, round axes deviating from the axis of external figure, by angles to be measured in the plane through it and the line perpendicular to the ecliptic in the direction towards the latter line. These angular deviations would be greater and greater the more near we come to the earth's centre, and the greatest angular deviation would be comparable with 1°. Hence the moment of momentum round the solsticial line would be sensibly less than if the whole mass rotated round the axis of figure. Now suppose for a moment our measurement of force to be founded on a year as the unit of time. We find the amount of precession in a year by dividing the mean amount of the whole couple due to the influence of moon and sun by the moment of momentum of the earth's motion round the solsticial line. Hence the amount of precession would be sensibly augmented by the elastic yielding; for the motive couple is uninfluenced by the elastic yielding, if we suppose the earth to be of uniform internal density. An ordinary elastic jelly presents a specimen of the degree of elasticity here supposed, as is easily seen when we consider that the mass of a cubic centimetre of such material is a gramme, and therefore that the weight of a cubic centimetre of the substance is the "gramme weight" understood in the specification $\frac{1}{10}$ of a gramme weight per square centimetre. If then, the interior mass of the earth were no more rigid than an ordinary elastic jelly, and if the upper crust were rigid enough to resist any change of figure that could sensibly influence the result, the precession would be considerably more rapid than if the rigidity were infinite throughout. The lunar nineteen-yearly nutation proves a higher degree of elasticity than this; the solar semi-annual nutation still a higher degree; and still higher yet the imperceptibility of the lunar fortnightly nutation; provided always we suppose the interior mass to be of uniform density, and the upper crust rigid enough to permit no influential change of figure.

The motive of the nineteen-yearly precession may be mechanically represented by two circles of matter pivoted on diameters fixed in the plane of the earth's equator, bisecting one another perpendicularly at the earth's centre. These two circles must oscillate round their pivot-diameters, each through an angle of about 5° on one side and the other of the plane of the equator, in a period of about nineteen years, to produce the lunar nineteen-yearly nutation (more nearly eighteen years seven months). If the radius of each of the supposed material circles is equal to the moon's mean distance from the earth, the mass of each must be a little less than the moon's mass, and one of them a little less than the other.* The diameter on which the latter is pivoted is to be the equinoctial line. The latter alone causes the nutation in right ascension; the former the nutation in declination. The phases of maximum and of zero deflection, in the oscillations of the two circles, follow alternately at equal intervals of time, so that when either is in the plane of the earth's equator, the other is at its greatest inclination of 5° on either side. Taking one of the constituents of the nutational motive alone, we find, on the principles indicated above, $\frac{1}{100}$ of a gramme weight per square centimetre as a very rough estimate for the greatest tangential stress produced by it in the material underlying the earth's crust. Now it is clear that the central parts of the earth and the upper crust cannot, in the course of the nutatory oscillations, experience relative angular motions to any extent considerable in comparison with the nutation of the upper crust, without considerably affecting the whole amount of the nutation. The nutation in declination amounts to 9″·25 on each side of the mean position of the earth's poles, and therefore the tangential stress of $\frac{1}{100}$ of a gramme weight per square centimetre cannot produce an angular distortion considerable in comparison with 9″.

An angular distortion of 8″ is produced in a cube of glass by a distorting stress of about ten grammes weight per square centimetre. We may, therefore, safely conclude that the rigidity of the earth's interior substance could not be less than a millionth of the rigidity of glass without very sensibly augmenting the lunar nineteen-yearly nutation. The lunar fortnightly nutation in declination amounts theoretically to about ·1″, and it is so small as to have hitherto escaped observation. It probably would have been so large as to have been observed were the interior rigidity of the earth anything less than $\frac{1}{200000}$ of that of glass, always provided that the upper crust is rigid enough to prevent any change of form sensibly influencing the nutational motive couple. To understand the degree of rigidity meant by "$\frac{1}{200000}$ of the rigidity of glass," imagine a sheet of some such substance as gutta-percha or vulcanised india-rubber of a square metre area and a centimetre thickness. Let one side of the sheet be cemented to a perfectly hard plane vertical wall, and let a slab of lead 8·8 centimetres thick (weighing therefore a metrical ton)* be cemented to the other side of it. If the rigidity of the substance be $\frac{1}{200000}$ of the rigidity of glass,† and the range of its elasticity sufficient, the side of the sheet to which the lead is attached will be dragged down relatively to the other through a space of $\frac{1}{12}$ of a centimetre.

In the argument from tidal deformations of the solid part of the earth's material, which I communicated to the Royal Society ten years ago, and mentioned incidentally at the recent meeting of the British Association, I showed that though precession and nutation would be augmented by want of rigidity in the interior, they would be diminished by want of rigidity in the upper crust, and that on no probable hypothesis can we escape the conclusion that the earth as a whole is less yielding than a globe of glass of the same dimensions and exposed to the same forces. That argument, therefore, proves about 200,000 times greater rigidity for the earth as a whole than what I

* The greater is equal to the moon's mass multiplied by the cosine of the obliquity of the ecliptic; the less is equal to the moon's mass multiplied by the cosine of twice the obliquity of the ecliptic.

* The metrical ton, or the mass of a cubic metre of water at temperature of maximum density, is ·9842 of the British ton. The thickness of a slab of lead of a square metre area, weighing a metrical ton, is, of course, equal to a metre divided by the specific gravity of lead.

† Everett's measurements give 244×10^6 centimetres weight per square centimetre for the rigidity of the glass on which he experimented. Instead of this I take 240×10^6, for simplicity.

have now written to you proves for the interior of the earth on the supposition of a thin preternaturally rigid crust.

I must apologise to you for having troubled you with so long a letter. I did not intend to make it so long when I commenced, but I have been led on by considerations of details, inevitable when such a subject is once entered upon.—I remain, yours very truly,

WILLIAM THOMSON

G. Poulett Scrope, Esq., F.R.S.

THE SOLAR ECLIPSE

IN the communication to NATURE, written from Ootacamund, I promised another when I was in possession of more information as to the work done, not only by the British Association parties, but by those representing the Indian and French Governments. Let me now endeavour to redeem my promise, seeing that since that communication was penned I have had the happiness of hearing from M. Janssen's own lips an account of what he did; have met Captain Waterhouse, the last representative at Ootacamund of Colonel Tennant's party; have visited Mr. Pogson at Madras, who obligingly gave me an account of the results obtained at Avenashi; and last, but not least, have learnt since my return home that the Jaffna party were successful, not only with the polariscope, but also with the camera and spectroscope.

Within a few minutes of the despatch of my last article I found that Captain Waterhouse, who assisted Mr. Hennessy in exposing the photographic plates taken by Colonel Tennant's party, was still at Ootacamund, and this welcome intelligence was soon followed by Captain Waterhouse himself, who was so good as to bring with him a drawing of one of the photographs; the plates themselves having been taken down the ghaut by Colonel Tennant, with the intention of comparing them at Pothonore with those taken by Mr. Davis. Unfortunately, as has been already stated, we missed each other, and so an absolute comparison of photographs did not take place; but from the drawing it was evident that in the two series the main form of the corona was the same. The photographs I learned were very sharp and good, and one appreciates their value the more when it is known that only a very little time before they were taken, any success, even a partial one, seemed out of the question, so persistently did cloud and mist hang over Dodabet on the eventful morning. I gathered that the spectroscopic observations had also been successful, and that a continuous spectrum with 1474 had been observed. If more lines than this were not seen, it is easily to be accounted for by the relatively long focal length of the object-glass employed to throw an image of the eclipsed sun on the slit.

Not until the morning after my interview with Captain Waterhouse did I learn the whereabouts of Dr. Janssen, who, from a study of the habits of the clouds and their prevailing drift, had concluded that the neighbourhood of Ootacamund was not the best that could be chosen. He had consequently taken his departure, and it seemed at first as if his whereabouts was known to no one. At last, however, Prof. Respighi and myself came upon his spoor; he was at Sholoor, on the N.E. flank of the range, at the solitary house of a tea-planter, to which there was no road, but which might be reached on ponies if a guide to it could be found. This guide Captain Sargeant, of the Revenue Department, obligingly provided, and in no very long time we reached the beautiful spot which Dr. Janssen had chosen.

It will be better that I should state his results in his own words. In a letter* to Prof. De La Rive, dated December 26, he thus writes:—

"J'ai été favorisé par un ciel d'une pureté presque absolue. Cette circonstance, et surtout les dispositions optiques toutes nouvelles que j'avais prises, m'ont permis de faire sur la couronne des constatations qui démontrent son origine solaire (pour la meilleure partie).

"Dans mon télescope,† le spectre de la couronne s'est montré non pas continu, mais remarquablement complexe. J'y ai constaté:

"Les raies brillantes du gaz hydrogène qui forme le principal élément des protubérances et de la chromosphère.

"La raie brillante verte déjà signalée aux éclipses de 1869 et 1870, et quelques autres plus faibles.

"Des raies obscures du spectre solaire ordinaire, notamment D. Ces raies sont beaucoup plus difficiles à apercevoir.

"Mes observations prouvent que, indépendamment des matières cosmiques qui doivent exister dans le voisinage du Soleil, il existe autour de cet astre une atmosphère très étendue, excessivement rare, à base d'hydrogène.

"Cette atmosphère, qui forme sans doute la dernière enveloppe gazeuse du Soleil, s'alimente de la matière des protubérances, lancée avec une si grande violence, des entrailles de la photosphère. Mais elle se distingue de la chromosphére et des protubérances, par une densité énormément plus faible, une température moins élevée, et peut-être par la présence de certain gaz différents.

"Il y a donc lieu de distinguer cette nouvelle atmosphère solaire. Je propose de la nommer *atmosphère coronale*, désignation qui rappelle que c'est elle qui produit la meilleure partie des phénomènes lumineux qui ont été désignés jusqu'ici sous le nom de couronne solaire.

"En annonçant ce résultat, je n'oublie pas, quant à moi, tout ce que nous devons aux travaux qui l'ont préparé, notamment ceux des astronomes américains aux éclipses de 1869 et 1870."

It will be seen that the importance of the brilliancy of the image, so strongly insisted upon by the Eclipse Committee in their Instructions, had been fully recognised by Dr. Janssen, whose instrument had more light even than those used by the British parties, who used "Browning With" reflectors of 9¼ inches aperture, and some 6 feet focus.

Although my account, in this place and at this time can only be of the most general character, the coincidence obtained by Janssen, Respighi, and myself on one point may be briefly referred to, namely, the distinct proof obtained by each of us that above the most vivid chromospheric layer, and even the prominences, we have hydrogen with its most familiar bright lines, and with much of the "structure" of its spectrum; these proofs being derived not only from the old method of inquiry, but from the new one employed by Professor Respighi and myself.

We spent the remainder of the day at Sholoor in mounting the hill at the back of the house to see the observatory, and to admire the wonderful view of the plains of Mysore, which was visible between a break in the hills; while the immediate neighbourhood, with its water-

* Bibliothèque Universelle, January 15, 1872, p. 103.

† Ce télescope a une ouverture de 0^m 37, et 1^m 42 seulement de distance focale. Les images y sont de 12 à 16 fois plus lumineuses que dans une lunette astronomique ordinaire. Le spectroscope avait été construit pour utiliser toute cette lumière.

falls, massive peaks, rocks here, and patches of wood there, steep ravines and tea-clad valleys, presented us with a scene of perfect beauty.

Next morning we were away before sunrise on our way to Mr. Pogson, whom we found at the Madras Observatory, preparing to exchange time signals with the Jaffna party. Three photographs were taken by Mr. Pogson at Avenashi, but the instrument used was so different from those used at Bekul and Dodabet (not to mention Jaffna) that it is difficult to institute a comparison in the time at my disposal; but it is not to be doubted that they will be of the highest importance when the general results come to be discussed. Mr. Pogson was assisted in the observations by his son and Mr. Chisholm, the Government architect, who was highly successful in sketching the corona and the eclipse effects upon the landscape.

Come we last to Jaffna. In my former article I referred only to the polariscope and spectroscope work done there. I have since learned that six photographs were taken with the sister instrument to the one used at Bekul.

The observations, in fact, were a perfect success. The morning was clear and bright, and could not have been finer had any one so wished.

At about six o'clock the party and those who were to assist them began to assemble on the Belfry Bastion in the Fort. Capt. Tupman observed with a polariscope and drew during the eclipse, and was assisted by Capt Varian of the *Serendib* as his time-keeper; Mr. Lewis with his telescope and polariscope was stationed inside the hut, with the photographic party, and Mr. Thwaites, Deputy Queen's Advocate, who was assisted by the carpenter of the *Serendib.* Capt. Fyers, R.E., with the spectroscope, had for his assistant Mr. W. S. Murray, Deputy Fiscal; and Capt. Hogg, R.E., who presided over the photographic department, was assisted by Mr. Twynam, Government agent, and Mr. J. W. Simpson. By these observers the polariscope results were arrived at, a telegraphic summary of which I quoted in my last communication. Six photographs were taken, being one more than we obtained at Bekul; and in the clockwork-driven integrating spectroscope the reversal of the dark lines was seen at the beginning of totality, and the hydrogen bright lines and 1474 during totality. No information yet about intensities.

Sketches were made by Mr. Foenander, of the Surveyor-General's Department, Colombo; Mr. Pargiter, Assistant Government Agent; Mr. Vine, M C.E., of the Public Works Department; Mr. Carmichael and Mr. Layard of the O.B.C.

The crowd of natives round the Belfry Bastion was very great; they set up a most hideous howl directly totality commenced, fancying that the end of the world was at hand. They were under the impression that the whole of the Expedition with assistants and all here during the eclipse were going to get into a balloon and off to the sun and not return.

It will thus be seen that the hopes of those interested in the various expeditions of this year have not been disappointed. The composition and structure of a part of the corona have been for ever set at rest, while we have seventeen photographs, taken by instruments of the same power and pattern, to compare with each other—eleven taken at the ends of a base line some 400 miles long, and six at an intermediate elevated point, whereby it was hoped to test the influence of the atmosphere on the observed phenomena. Whether the slight mist will have prevented this or not remains to be proved; but anyhow here is a wealth of records unequalled before, and we may hope to learn much of the outer coronal regions from their comparison, not only *inter se*, but with Mr. Holiday's admirable drawings, showing considerable changes, which have also come to hand.

J. NORMAN LOCKYER

THE ADMIRALTY MANUAL OF SCIENTIFIC INQUIRY

A Manual of Scientific Inquiry; Prepared for the Use of Officers in Her Majesty's Navy and Travellers in General. 4th Edition. Superintended by the Rev. Robert Main, M.A., F.R.S., Radcliffe Observer at Oxford. Pp. 392. (John Murray, 1871.)

IN one of the earlier numbers of the *Philosophical Transactions* may be found a long list of observations proposed to be made by travellers who were about to visit the Peak of Teneriffe. Athanasius Kircher, in his *China Illustrata*, had given an account of such great marvels, that the less credulous, even of those days, wondered and almost doubted; and it was thought to be of advantage to know whether unicorns and dragons really did exist in foreign parts, whether diamonds grew, and what was the precise nature of that "poyson which turneth a man's bloud to gelly." Long afterwards the Royal Society issued instructions for the Antarctic Expedition, hints for collecting information in China, and a book entitled "What to Observe," but there was no general manual for the use of observant travellers, directing them specially not only what to observe, but how to observe. In 1849 the Lords Commissioners of the Admiralty, conceiving that "it would be to the honour and advantage of the Navy, and conduce to the general interests of Science, if new facilities and encouragement were given to the collection of information upon scientific subjects by the officers, and more particularly by the medical officers, of Her Majesty's Navy when upon foreign service," gave orders for the compilation of "The Admiralty Manual." The work was originally edited by Sir John Herschel, and was divided into various sections, each the work of some competent authority.

The work is divided into four parts. The first includes astronomy, hydrography, and tides; the second terrestrial magnetism, meteorology, atmospheric tides; the third geography, statistics, medical statistics, ethnology; and the fourth geology, mineralogy, seismology, zoology, botany. In this last edition all the articles are brought *en rapport* with the progress of science since 1849; the article on tides by Dr. Whewell is revised by the present editor of the book; the articles on statistics, medical statistics, ethnology, geology, mineralogy, botany, have also been revised by other than the original authors. There are two capital maps, the one to illustrate hydrographic delineation; the other to show the approximate limits of the great currents and drifts of the ocean.

The Astronomy (by the Astronomer Royal) is the shortest article in the book, extending over no more than 12 pages. Hydrography, on the other hand, occupies 49 pages, and contains much useful information regarding soundings, the discovery of land, sailing directions, and artificial harbours. The directions are essentially practical and eminently suggestive; thus, take the following from *Approaching a coast*:—"Always bear in mind that no description can equal a tolerably faithful sketch, accompanied by bearings. In all four sketches take angles roughly with a sextant between objects at the extremities of four drawings, and two or more intermediate ones, and affix them to the objects of the moment, and have at least one angular height in the picture; let that be of the highest and most conspicuous or best defined object."

The article on Tides (26 pages) gives minute directions for tide observations and the construction of curve tables. The next section, on Terrestrial Magnetism, by Sir Edward Sabine, is of great importance, and describes the methods of observation most in vogue; the observations of local attraction, of vibration, of deflection, and so on. We miss, however, any account of the magnetism of iron ships, and the elimination of the compass error caused thereby. Also we feel assured that simple instructions for travellers as to the use of compasses on land, in the midst of forests, &c., would prove of much service. Under the heading Meteorology we find directions for observing systematically a large number of aerial phenomena, water-spouts, bull's-eye signals, showers of dust and ashes, cyclones, various electrical manifestations, &c. Passing over the articles on atmospheric waves and barometric curves, we come to that on Statistics, which is of very general interest, and relates to the state of education and crime of a people, the manufactures, commerce, currency, revenue, municipal regulations, &c. This is followed by "Medicine and Medical Statistics," regarding the various fevers and other diseases to which travellers are specially exposed, with hints for determining the geographical distribution of diseases.

The chapter on Ethnology by the late J. C. Prichard, revised by Mr. E. B. Tylor, is to be specially commended to the notice of travellers; under the term he includes "all that relates to human beings, whether regarded as individuals, or as members of families or communities;" the physical and social history of man. This chapter is divided into three parts:—(1) of the Physical Character of Nations; (2) Characteristics of the state of Society, &c.; (3) Language, Poetry, Literature. We are lamentably deficient in our knowledge regarding the earlier history of the physical sciences, and are glad to find that Mr. Tylor alludes to the acquirement of knowledge of this nature in the following paragraph:—"The crude notions entertained by uncivilised races on subjects within the scope of physical science are matters worthy of inquiry. Science they can hardly be said to possess, though this was scarcely true with the ancient Mexicans. All nations observe the changes of the moon, and measure the lapse of time with a greater or less degree of accuracy by the movements of some of the heavenly bodies. The special names given to the months, if any, should be recorded. Inquiry should be made whether the motions of the planets are observed, and whether these bodies are distinguished from fixed stars; what ideas are current as to the conformation of earth and sky and the cause of eclipses; whether attempts are made to ascertain the duration of the solar year, whether there are names for the constellations, and what they are if they exist."

Of the remaining portions of this work we need only allude to that devoted to "Seismology, or Earthquake Phenomena," by Mr. Robert Mallet, which contains many details as to the observation of effects of rare occurrence in these latitudes, but to the traveller in South America the suggestions would be invaluable. Thus we have an account of instruments for observing the velocity and direction of the shock of an earthquake, observations to be made in a city affected by an earthquake, and the preparation of coseismal and meizoseismal curves. To conclude: the whole work is wonderfully suggestive, not alone to the traveller, but to the home observer; it teaches us to arrange in order and systematise our observations, and in so doing conveys a great deal of collateral information.

G. F. RODWELL

OUR BOOK SHELF

Gmelin-Kraut's Handbuch der Chemie, Anorganische Chemie. In Drei Banden, Sechste umgearbeitete Auflage. Herausgegeben von Dr Karl Kraut, Heidelberg. Erster Band zweite Abtheilung, pp. 176. (London: Williams and Norgate.)

IT is now eighteen years since the appearance of the fifth edition of this work; this, of course has necessitated the change from the old atomic weights to the new, but the arrangement of the elements and sections of the book has been retained as in former editions. The present volume has been thoroughly revised, the information having been brought up to a very recent date; should the remaining volumes be equally reliable, it will probably be the most complete work on inorganic chemistry in any langauge. Dr. Kraut has obtained the assistance of Drs. Naumann, Ritter, and Jorgensen, in order to expedite the conclusion of the work. There is no book to our knowledge which contains so large an amount of information in a small space as Gmelin's Handbook. It is, as expressed in the preface, a complete, concise, and systematic handbook of chemistry up to the latest time. The merits of this book for the purposes of reference are so well known that it would be quite superfluous to enter into any lengthened description of it. In the volume now under consideration oxygen, hydrogen, carbon, boron, phosphorus, and sulphur, with some of their more important compounds, are treated of; the article on ozone and its properties is perhaps typical of the book, it occupies fourteen pages, and forms a very valuable and complete history of this body. The completion of the book may be looked for with interest, although necessarily it will be some time before this can be accomplished.

Astronomical Phenomena in 1872. By W. F. Denning, Hon. Sec. of the Observing Astronomical Society. (London: Wyman and Son.)

THIS brochure consists of some general remarks on astronomical observing, and some forty pages of data almost entirely taken from the "Nautical Almanack" for 1872. The former are addressed to the simplest tyro, and are so meagre as to give the impression of a want of accurate knowledge. In the section touching upon instruments we are told that "with regard to the spectroscope, micrometer, and other astronomical appliances, it will be better to say but very "little." Accordingly very little is said, and that little is unimportant. Speaking of objects, Mr. Denning startles us with the announcement that "Comets are not interesting objects in a telescope" (we should like to hear upon what experience he grounds this assertion); and he deals with the hypothetical plant Vulcan by naïvely telling his disciples that when a total eclipse of the sun "is in progress, the region of the heavens in the immediate vicinity of the solar orb should be subjected to very careful scrutiny." For such untutored gazers as are addressed in the earlier pages the data in the later sections are insufficient. There are no times of rising and setting of the moon and planets, no positions of Jupiter's satellites at times of eclipse, no information upon the points on the moon's limb at which occulted stars will disappear and reappear, no warning of the effects which change of geographical position will produce in some phenomena which are computed for Greenwich only. Altogether the book is a very weak production. J. C.

Die Arachniden Australiens nach der Natur beschrieben und abgebildet, von Dr. L. Koch. Erste Lieferung. Pp. 56. Plates iv. (Nurnberg, 1871.)

Dr. L. Koch intends in this work to describe the spiders of Australia, not confining himself apparently to the large insular tract that generally passes under this name, but taking in also the Viti Islands, the Friendly, Pelew, and other groups. In his Preface to this, the first portion of his work, he says that though he has with much care and industry for twenty years observed the Arachnida of a little circuit of not more than from four to five hours walk, yet every year there comes to light within this small compass some new species that had up to then remained concealed; indeed it often happened that each little journey increased the number of forms known in the district. How true this observation is every investigator will feel; but knowing and feeling it, what courage does it not require to set to work to write the history of the spiders of a district which itself is not even yet half explored; and when the spiders are done, we are promised another work on the Myriapods. Such courage deserves to succeed, and we wish the enterprise every prosperity. The work will be published at intervals of two months, and be completed in two years; each bi-monthly part will contain four plates and some five sheets of text.

Following the families and genera as laid down by Thorell in his "European Spiders," L. Koch commences with the Epeiridæ, and describes six new species of the interesting genus *Gasteracantha*. Here, as in the other genera, the new species are well figured by the author in quarto plates. It is to be observed that some of the species described are not to be met with, at least have not at present been met with, in any part of Australia, but are introduced into this work by the head and shoulders as it were thus:—*G. violenta* comes from New Guinea, and *G. hepatica* from Java. Two new genera, *Tholia*, with three species, and *Anepsia* for *Epeira rhomboides*, L.K., are given. Ten new species of the genus *Argiope* are described, and three new species of *Cyrtarachne*. The diagnoses of the new genera are very properly given in Latin, and the work may be regarded as quite indispensable to all those engaged in the study of the spiders. W.

LETTERS TO THE EDITOR

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

Change of Habits in Animals and Plants

Some weeks since I sent a few notes on *Nestor notabilis*,* showing a curious change in the history of this mountaineer. I now beg to add an extract from the *Otago Daily Times*, in confirmation of this strange story of the progressive development of change in the habits of the Kea, from the simple tastes of a honey-eater to the savageness of a tearer of flesh:—

"Some time ago we mentioned that Mr. Henry Campbell, of Wanaka Station, had noticed that sheep on his run were frequently attacked by birds. We are indebted to Mr. Campbell for some further information on the subject. The birds in question are of the kind called by shepherds "the mountain parrot," and the scientific name of which is *Nestor notabilis*. The Maories call it the Kea. The birds come in flocks, single out a sheep at random, and each alighting on its back in turn, tears out the wool and makes the sheep bleed, till the animal runs away from the rest of the sheep. The birds then pursue it, continue attacking it, and force it to run about till it becomes stupid and exhausted. If in that state it throws itself down, and lies as much as possible on its back to keep the birds from picking the part attacked, they then pick a fresh hole in its side, and the sheep, when so set upon, in some instances dies. When the sheep stops bleeding the birds appear to cease to attack it, though Mr. Campbell is not very clear upon this point, and thinks they attack it more for sport than hunger. For three winters back his sheep have been attacked in this way, and it was not till this winter (though he previously suspected it) that he found the birds were the offenders. Where the birds so attack the sheep, the elevation of the country is from 4,000 to 5,000 feet above the sea level, and they only do so there in winter time. On a station owned by Mr. Campbell about thirty miles distant from the other, and at the same altitude, in the same district, and where the birds are plentiful, they do not attack the sheep in that way. For those on whose stations they are an annoyance, it may be mentioned that their numbers can be kept well thinned by shooting them. If one is wounded the rest gather round, and can be shot in fives and sixes at a time."

This note is interesting in the face of the destructive influence commonly exerted by *introduced* upon *native* life. Here we have an indigenous species making use of a recently imported aid for subsistence, at the cost of a vast change in its natural habits.

In the vegetable world we meet with a change in the habit of a native species* which is somewhat analogous.

Our *Loranthus micranthus* sometimes neglects its customary supports, found often on such trees as *Melicytus* or *Melicope* (representatives of *Violarieæ* and *Rutaceæ*), for the more attractive exotics, *Cytisus*, *Cratægus*, the plum, and the peach. Such change in its habits this fragrant parasite acquires at the cost of deserting the interlaced boughs of tangled gully for a more conspicuous position in the trim shrubbery or cultivated garden. At this time I can see a most vigorous specimen of *L. micranthus* growing on *Cytisus laburnum*, covered with countless panicles of perfume-laden blossoms, on which our introduced bee is luxuriously regaling. Here we have the foreign bee gathering sweets from native flowers growing on an exotic tree.

In this neighbourhood the laburnum was first planted, I believe, by myself, in 1859, and the bee introduced about the same time.

Ohinitahi, Oct. 7, 1871 Thomas H. Potts

* See Nature, vol. iv., pp. 489, 506.

* See Trans. New Zealand Institute, vol. iii., p. 190.

A Case of Stationary Wave on a Moving Cord

It is well known to mathematicians that a stretched cord, moving lengthwise with a velocity bearing a certain relation to its tension and weight, will retain any curvature which may be impressed upon it; and consequently would pass through a crooked tube without pressure against its sides. That this may be the case, the velocity, V, must equal $\sqrt{\frac{T}{M}}$; T being the tension, and M the weight of the cord per unit of length.

Passing from a stationary curve on a moving cord to one moving along a fixed cord, it is easy to see that this velocity, V, must be that of the transmission of a transverse vibration; and from this immediately follows the formula for the times of vibration of stretched strings.

The case of the stationary wave, however, though simple in theory, is rarely practically realised; and I think a short notice of a case in which it is constantly produced may not be without interest.

In Captain Dennet's admirable invention for saving life from shipwrecks, a rocket is employed having a light line attached to it. This line is previously "faked down" on two rows of pins in a box; and, the pins being withdrawn, it remains in a series of zigzags which yield without entanglement to the very rapid motion of the rocket—the strain on the cord being only due to its inertia. As then the force required to set it in motion is proportional to the weight of cord moved multiplied by its velocity, and this weight is also proportional to its weight per unit of length multiplied by the velocity, the strain or tension, $T = MV^2$ or $V = \sqrt{\frac{T}{M}}$; the relation which we have already seen is necessary to the production of a stationary wave. Accordingly, we find that the rope, instead of at once following the flight of the rocket, rises almost perpendicularly from the box, and only passes into its low trajectory at a distance of six or eight feet, with a sharp irregular curve, which remains comparatively steady during the whole flight of the rocket. This curve is no doubt first produced in the first portion of the rope, which is "faked down" on the ground outside the box; but it would be impossible to see its formation, because of the smoke of the discharge, even if the motion were not too rapid.

One rather curious result of the above-mentioned conditions is, that however erratic the flight of the rocket may be, the rope will continue to follow through the whole track, as if the air were a solid which the rocket had pierced.

Another result is, that no lateral vibrations can be propagated along a rocket line—a fortunate condition with regard to steadiness of flight. HENRY R. PROCTER

Clementhorpe, North Shields, Jan. 26

Ocean Currents

PROF. EVERETT has evidently misapprehended what I said in my letter to NATURE, January 11. Nine foot-pounds would, of course, generate in a pound of matter a velocity equal to that acquired by the pound falling through a space of nine feet. And in reference to the deflecting power of rotation, what I meant was not the amount of deflection in a given space passed over, but the positive amount, say in feet, in a given time.

Edinburgh, Jan. 27 JAMES CROLL

ON TEACHING GEOLOGY AND BOTANY AS PARTS OF A LIBERAL EDUCATION

ON Monday, Jan. 22, one of a series of lectures on Educational questions was given at the rooms of the Society of Arts by Mr. J. M. Wilson, of Rugby. The following may be taken as an abstract of the lecture :—

Two points have to be considered : (1) When, if at all, these Natural History Sciences ought to be introduced into schools ; (2) What they should include, and how they should be arranged for teaching purposes.

The problem before schoolmasters is to adjust the rival claims of the subjects which press for admission into the school course, all of which may urge something in their favour. These subjects have increased in number and extent so that the question of re-arrangement is pressing. For the solution at present is to admit a little of all, or nearly all ; and the effect of this is to distract. A wide education levels up, but also levels down, and weakens, by eliminating the close study of detail, and the drudgery that is essential in valuable work. It is that conflict between the old theory of *promise* and the new theory of *performance* ; and schools are in great danger of giving less faculty than they did formerly, though they give increased knowledge.

To meet the requirements some *stratification* of studies must be effected, so that not so many shall be followed at once. Greek and Chemistry and Physics (except Mechanics), should be excluded from the elementary course, which should include Latin, French, Arithmetic, and Natural History. Then bifurcation should begin ; the one branch leading to Greek and a mainly literary education, the other to Science; both continuing Latin and English, and French and History. The recognition of the bifurcation, both by the Universities and by the great schools, is urgently needed. Without it Science must be dwarfed or excluded, and literature also suffer from the distraction which is already felt at schools. The programme of the reformers in education ought to include the abolition of Greek as a compulsory subject at the Universities.

By Natural History is meant what Huxley has introduced to us under the word "Erdkunde." The earth, its relation to sun and moon, the phenomena of day and night, and seasons ; the changes going on, the activities of the earth, rain, and rivers, and sea, and earthquakes, and slow changes of level, and their geological effects, and something also of geology proper. The teaching should be based on the familiar knowledge of the boys, and should extend and systematise it, and without being too dogmatical, should be practical where possible. A little botany, enough to teach the objects and the interests of the science, and the principles of structure and classification, and something of geographical distribution, may well be included in the natural history of this elementary stage in education. The object of the master should be to discover and train scientific ability, as well as to give scientific information, and for this purpose these studies have great advantages. The bearing of the experience gained at Rugby on these questions was also given.

THE SURVIVAL OF THE FITTEST

LAST summer a discussion took place in your pages on the expression, "Survival of the Fittest," and on the principle it formulates. Though, as being responsible for this expression, there seemed occasion for me to say something to dissipate the errors respecting it, I refrained from doing so, for the reason that the rectification of misstatements and misinterpretations is an endless work, which it is almost useless to commence.

In your last number, however, the question has cropped up afresh in a manner which demands from me some notice. A Professor is tacitly assumed to be an authority in his own department; and a statement made by him respecting the views of a writer on a matter coming within this department, will naturally be accepted as trustworthy. Hence it becomes needful to correct serious mistakes thus originating.

In your abstract of Prof. E. D. Cope's paper, read before the American Association for the Advancement of Science, I find the following sentences :—

"This law has been epitomised by Spencer as the 'Preservation of the Fittest.' This neat expression, no doubt, covers the case, but it leaves the origin of the fittest entirely untouched."

There are here two misstatements, the one direct and the other indirect, which I must deal with separately.

So far as I can remember, I have nowhere used the phrase, "Preservation of the Fittest." It is one which I have studiously avoided ; and it belongs to a class of phrases for the avoidance of which I have deliberately given reasons in "First Principles," sec. 58. It is there pointed out that such expressions as "Conservation of Force," or "Conservation of Energy," are objectionable, because "conservation" implies a conserver, and an act of conserving—implies, therefore, that Energy would disappear unless it was taken care of ; and this is an implication wholly at variance with the doctrine enunciated. Here I have similarly to point out that the expression "Preservation of the Fittest" is objectionable, because in like manner it supposes an act of preserving—a process beyond, and external to, the physical processes we commonly distinguish as natural ; and this is a supposition quite alien to the idea to be conveyed. One of the chief reasons I had for venturing to substitute another formula for the formula of Mr. Darwin, was that "Natural Selection" carries a decidedly teleological suggestion, which the hypothesis to be formulated does not in reality contain ; and a good deal of the adverse criticism which the hypothesis has met with, especially in France, has, I think, arisen from the misapprehension thus caused. The expression, "Survival of the Fittest," seemed to me to have the advantage of suggesting no thought beyond the bare fact to be expressed; and this was in great part, though not wholly, the reason for using it.

Prof. Cope's indirect statement, that I have said nothing to explain "the origin" of the fittest, is equally erroneous with his direct statement which I have just corrected. In the "Principles of Biology," sec. 147, I have contended that no "interpretation of biologic evolution which rests simply on the basis of biologic induction, is an ultimate interpretation. The biologic induction must be itself interpreted. Only when the process of evolution of organisms is affiliated on the process of evolution in general, can it be truly said to be explained. . . . We have to reconcile the facts with the universal laws of re-distribution of matter and motion." After two chapters treating of the "External Factors" and "Internal Factors," which are dealt with as so many acting and reacting forces, there come two chapters on "Direct Equilibration" and "Indirect Equilibration"—titles which of themselves imply an endeavour to interpret the facts in terms of Matter, Motion, and Force. It is in the second of these chapters that the phrase "Survival of the Fittest" is first used; and it is there used as the most convenient physiological equivalent for the purely physical statement which precedes it.

Respecting the adequacy of the explanation, I, of course, say nothing. But when Prof. Cope implies that no explanation is given, he makes still more manifest that which is already made manifest by his mis-quotation—either that he is speaking at second hand, or that he has read with extreme inattention. HERBERT SPENCER

Athenæum Club, Jan. 29

THE CHANCE OF SURVIVAL OF NEW VARIETIES

AN argument first urged by the writer of an article on the "Origin of Species" in the *North British Review* for June 1867, regarding the probability of the preservation of a new modification or variety among the descendants of a plant or animal, has of late attracted much attention. It has been discussed at length by Mr. Mivart, one of the ablest critics of the Darwinian theory, and Mr. Darwin himself has, with characteristic candour, ascribed great, and as I believe undue, importance to the inferences drawn from it.

To some extent I agree with the remarks of Mr. Davis, published in your journal of the 28th December last, but I venture to think that the soundness of the argument in question has not been thoroughly tested, and that it will not bear close examination. The calculus of probabilities is a very subtle instrument, and, even in what appear to be its simpler applications, a very fallacious one, if every step in the process is not carefully considered

The reviewer started with a seemingly simple statement of the case—"A million creatures are born; 10,000 survive to produce offspring. One of the million has twice as good a chance of surviving; but the chances are 50 to 1 against the gifted individual being one of the ten thousand (at first erroneously printed 'hundred') survivors." The fallacy here lies in the assumption that under the conditions which, according to the Darwinian theory, enable natural selection to become an efficient modifying agent, the chance of survival of a favourable modification can be correctly represented by the ratio of 2 to 1.

To avoid complication let us confine the argument to non-diœcious plants or self-fertilising lower animals. The preservation of a new variety or modification of structure depends upon two separate elements related respectively to growth and reproduction. The individual must reach maturity, and must reproduce offspring, and for each of these processes it must be able to overcome the obstacles offered by the action of other organic beings, and by external physical conditions. As a general rule we may assume that the same modification does not affect both growth and reproduction, and as the main stress of the struggle for existence turns on the dangers that affect the early period of growth, and the difficulties attendant on the production of healthy offspring, we shall sufficiently illustrate the subject in hand by considering these separately.

The chance of a modified individual growing to maturity depends upon its power of resistance to, or escape from, the various hostile agencies that surround the young animal or plant, whose combined influence is (by hypothesis) such that but one out of every hundred reaches maturity. Let us assume, for the sake of illustration, that the most important dangers to which the creature is exposed arise from physical conditions—such as excessive drought or damp—and from other organisms, as when it is the favourite food of some common animal. Now let the supposed modification affect the former relation. Let the modified organism be better fitted to resist drought; the result will be an enormous probability in favour of its escape from a danger that may destroy nine-tenths of the unmodified creatures around him, and a similar argument will apply to such a modification as would make the individual modified distasteful, or less than usually attractive, as an article of food. In point of fact, the dangers arising from external physical conditions are usually far less constant in their action than those arising from organic foes, and it is quite conceivable that even in the extreme case of a modification originating in one single individual of a species, if it were such as to give a decided advantage in that direction, the balance of probability would be in favour of survival, and in case of reappearance among numerous individuals in the next generation, have a preponderating chance of ultimate preservation.

The application of figures to measure the advantage given by a modification relating to the capacity of a species for reproduction involves no less difficulty, and may lead to the most various estimates of the probability of survival. A variation in a plant which should double the number of seeds produced without lessening their vitality, would give an advantage of 2 to 1 in the chance of producing offspring, but this, as the reviewer has shown, would not much increase the probability of the ultimate prevalence of that variety. But if the numbers of a plant were chiefly kept down by such a cause as the fruit being a favourite article of food, a modification of its flavour that would lead to some other fruit being preferred would almost certainly lead to the perpetuation of the variety with modified fruit, and not only to the rapid destruction of the unmodified form, but also to a reduction in the prevalence of some other plant.

For it must be recollected that the struggle for existence is not limited to the offspring of a single species. The rivals of each organism are all around, and the chance of survival of a new variety may be enormously increased if it be not only better able to resist hostile

* By way of correcting a further misapprehension of Prof. Cope, I may here point out that this conception, in its less developed form, goes back to a much earlier date than the "Principles of Biology" to which he refers. In the *Westminster Review* for April 1852 (pp 498-500), I have contended that "this inevitable redundancy of numbers—this constant increase of people beyond the means of subsistence," necessitates the continual carrying-off of "those in whom the power of self-preservation is the least:" that all being subject to the "increasing difficulty of getting a living which excess of fertility entails," there is an average advance under the pressure since 'only those who *do* advance under it eventually survive:" and these "must be the select of their generation." There is however, in the essay from which I here quote, no recognition of what Mr. Darwin calls "spontaneous variation," nor of that *divergence of type* which this natural selective process is shown by him to produce.

agencies that the unmodified form of the same species, but better than other rival organisms that may be its competitors in the struggle for existence.

I make these remarks without any desire to press the conclusion to an extreme length. I am not one of those more Darwinian than Mr. Darwin himself, who believe that the theory of Natural Selection explains everything, and has left no mysteries unsolved. I feel no doubt but that very many modifications arise that do not perpetuate themselves by the survival of a sufficient number of similarly modified individuals, even in cases where the variation may be slightly favourable; but I cannot admit the validity of an argument that goes to the very root of the principle of Natural Selection, and leads, by the appearance of exact reasoning, to a result that every naturalist feels to be absurd.

In truth, it is impossible to assign any limit to the amount of probability in favour of the preservation of a new variety. In the absence of disturbing causes affecting the equilibrium which the conditions hitherto existing in a given region tend to establish between the numbers of each species, it may be safe to assume that the probability of any new variety establishing itself is but small. But let that equilibrium be disturbed—let some hitherto unknown plants spread widely, as so many European weeds have done in Australia. This must lead to a corresponding diminution in the number of individuals of the previous vegetable inhabitants of the country, and a corresponding reduction among the animals that fed upon them. Let one of these animals be modified so as to be able to derive nourishment from the intrusive species. Is it not evident that the chance of its survival, and that of its similarly modified descendants, would be so great as to approach to certainty, unless the modification happened to bring with it other counterbalancing disadvantages?

JOHN BALL

THE USE AND ABUSE OF COMPLIMENTARY NAMES

THOSE whose fortune it is to work in some particular branch of science which has not been by any means exhausted, and to encounter daily some new form from an unexplored region which seems to warrant recognition as a new species, are often in difficulty to obtain a suitable name, one which shall distinguish the new species from its congeners, or give indication of one of its most prominent characteristics. It would seem that some (I fear many) are not so fully impressed as they should be with the importance of giving appropriate specific names to new species. "Trivial" names is in many cases an accurate designation. When a new name has to be given, it seems to me that the first effort should be directed towards applying a name which has at least some connection with the object to which it is applied, and if possible indicate one of the features by which its specific distinction is established. In very large genera this will often be difficult, but seldom impossible, if sufficient reflection be permitted. This presupposes, of course, clear notions of what are the distinctive features of the new species, and something more than a mere superficial knowledge of its congeners. The custom of giving complimentary names has considerably increased of late years, and seems almost to have culminated in absurdity. It is never a thankful office to impute blame, or point out the failings of others, and I should never have ventured to draw attention to this subject did I not conceive that the application of complimentary specific names has become an abuse which needs to be protested against. I am willing to concede that the occasional dedication of a new species to some acknowledged authority, one who has published a monograph of the genus, or who has identified himself more or less with the subject, may be a graceful compliment; but even this should hardly supersede a name indicative of some special feature in the new species. My own feelings are in favour of wholly restricting such compliments to generic names. But wherefore should a mere collector, one who has stumbled over a new species by mere accident, by collecting everything that came in his way of a particular kind, unable perhaps even to recognise generic distinctions, be flattered by having his name attached to the new form by some one who has had all the scientific labour in examining, describing, and naming it for him? Has science no higher aim than that of scattering compliments? It must cause many a smile to pass across the countenances of the unscientific if they open a new cryptogamic flora, a monograph, or even glance through a volume of some scientific journal, to see on one page how Mr. Brown ventures to name something new in honour of his friend Mr. Robinson, and a few pages further on Mr. Robinson returns the compliment in favour of Mr. Brown; or in another case how in five or six genera, extending over as many pages, the same "indefatigable collector" is honoured by having his name as many times repeated, as if new species were only so many pegs on which compliments are to be suspended. My own experience is very much restricted to cryptogamic botany, and my remarks may be much less pertinent to other branches of natural science. Zoologists may not be addicted to such forms of flattery. Continental mycologists are certainly very great sinners in this respect. My object in drawing the attention of readers of NATURE to this subject is to protest against this "abuse of complimentary names," and to ascertain if some definite restriction cannot be placed upon this tendency to encumber our lists with an array of names which convey only one meaning, and which I would designate "flattery names." I hardly think it necessary to cite particular instances, as a question of this kind should be decided upon its merits, and without the introduction of personalities. The sceptical should make the experiment with some recent volume containing descriptions of new species. In one contingency, I think that it is not only admissible but advisable to use a complimentary name. If an author describes a species under a name which has already been adopted in the same genus, it would be very inconvenient to have the one specific name applied by two authors to different things. In such a case it is the custom for any one who may be working up and publishing a synopsis of the genus to suppress the most recent of the two specific names, and apply to it the name of the author who unconsciously fell into the error. Provided always that he recognises the species having priority of name as a valid member of the genus, there cannot be much abuse of this recognised practice, against which I have nothing to urge. It would be simple folly to make laws which there is no power but "common sense" to enforce; and no decision which I may determine upon will be binding upon any one save myself; yet I cannot but regret that any who have laboured year after year in love for their own special branch of science, often following it for its own sake alone, through many sacrifices, should be tempted to employ the knowledge they have so acquired as a means whereby to compliment their friends or flatter their inferiors, forgetful of the practical sarcasms that they are hurling at their own pursuits.

M. C. C.

THE ECLIPSE OBSERVATIONS AT BEKUL

THE illustrations which accompany this, for the loan of which we are indebted to the courtesy of the Editor of the *Illustrated London News*, are from photographs of the Eclipse party stationed at Bekul, taken by Mr. McC. Webster, the Collector of South Canara. The first represents the fort in which Mr. Lockyer and Captain

WAITING FOR THE ECLIPSE

THE BUNGALOW AT BEKUL, CANARA

Maclear had erected their instruments. Mr. Davis's photographic and Dr. Thomson's polariscopic observations being carried on at a little distance below. The instruments represented are the 9¼ reflector constructed by Mr. Browning, with a mounting by Cooke, and the double refractor, consisting of two telescopes of six inches aperture, mounted on one of the universal stands prepared for the Transit of Venus observations in 1874, and lent by the Astronomer Royal.

The second is a representation of the bungalow which formed the residence of the same party during their stay in India, erected under the friendly shelter of a grove of spreading banyan-trees. The temperature in the middle of the day at Canara reaching commonly to 90° Fahr. within doors, it will be seen how necessary was not only the shelter of the trees for their residence, but the umbrella which a native attendant is holding over the head of one of the observers during the actual time of observation.

ON THE INFLUENCE OF VIOLET LIGHT ON THE GROWTH OF VINES, AND ON THE DEVELOPMENT OF PIGS AND BULLS

GENERAL A. J. PLEASONTON, from Philadelphia, U.S., has been engaged since 1861 with some very interesting experiments on the influence of light, transmitted through violet glass, in developing animal and vegetable life. In April 1861, cuttings of vines of some twenty varieties of grapes, each one year old, of the thickness of a pipe-stem, and cut close to the spots containing them, were planted in the borders inside and outside of the grapery, on the roof of which every eighth row of glass was violet-coloured, alternating the rows on the opposite sides. Very soon the vines began to attract great notice from the rapid growth they were making. Every day the gardener was kept busy in tying up the new wood which the day before had not been observed. In a few weeks after the vines had been planted, the walls and inside of the roof were closely covered with the most luxurious and healthy development of foliage and wood.

In September of the same year Mr. Robert Buist, a noted seedsman and horticulturist, from whom the General had procured the vines, visited the grapery. After examining it very carefully, he said :—" I have been cultivating plants and vines of various kinds for the last forty years ; I have seen some of the best vineries and conservatories in England and Scotland ; but I have never seen anything like this growth." He then measured some of the vines, and found them forty-five feet in length, and an inch in diameter at the distance of one foot above the ground. And these dimensions were the growth of only five months !

In March 1862 they were started to grow, having been pruned and cleaned in January of that year. The growth in this second season was, if anything, more remarkable than it had been in the previous year. Besides the formation of the new wood, and the display of the most luxuriant foliage, there was a wonderful number of bunches of grapes, which soon assumed the most remarkable proportions—the bunches being of extraordinary magnitude, and the grapes of unusual size and development.

In September, when the grapes were beginning to colour and to ripen rapidly, Mr. Buist visited the grapery again, and estimated that there were 1,200 pounds of grapes. General Pleasonton remarks that in grape-growing countries, where grapes have been grown for centuries, a period of time of from five to six years will elapse before a single bunch of grapes can be produced from a young vine ; while here, only seventeen months after, his grapery had yielded the finest and choicest varieties of grapes.

During the next season (1863) the vines again fruited, and matured a crop of grapes, estimated, by comparison with the yield of the previous year, to weigh about two tons ; the vines were perfectly healthy, and free from the usual maladies which affect the grape. Many cultivators said that such excessive crops would exhaust the vines, and that the following year there would be no fruit ; as it was well known that all plants required rest after yielding large crops. Notwithstanding, new wood was formed this year for the next year's crop, which turned out to be quite as large as it had been in the season of 1863 ; and so on, year by year, the vines have continued to bear large crops of fine fruit without intermission for the last nine years. They are now healthy and strong, and as yet show no signs of decrepitude or exhaustion.

The success of the grapery induced General Pleasonton to make an experiment with animal life. In the autumn of 1869 he built a piggery, and introduced into the roof and three sides of it violet-coloured and white glass in equal proportions—half of each kind. Separating a recent litter of Chester country pigs into two parties, he placed three sows and one barrow pig in the white pen, and three other sows and one other barrow pig in the pen under the violet glass. The pigs were all about two months old. It will be observed that each of the pigs under the violet glass was lighter in weight than the lightest pig of those under the sun-light alone in the white pen. The two sets were treated exactly alike; fed with the same kinds of food, at equal intervals of time, and with equal quantities by measure at each meal, and were attended by the same man. On the 4th of May, 1870, the six sows, being weighed, the following conclusion was obtained : –

	Under the violet pens.	Under the white pens.
November 3, 1869	122 lbs.	144 lbs.
March 4, 1870	520 lbs.	530 lbs.
Increase	398 lbs.	386 lbs.

Consequently, although the pigs placed under the violet pens actually weighed 10 lbs. less than those under the white pens ; yet, taking into consideration the 22 lbs. less which the first pigs had previously weighed, there is an actual gain of 12 lbs. The two other barrow pigs offered nearly the same result.

The next experiment of General Pleasonton was with an Alderney bull calf, born on Jan. 26, 1870. At its birth it was so puny and feeble that the man who attends upon his stock—a very experienced hand—told him that it would not live. He directed him to put it in one of the pens under the violet glass. In 24 hours a very sensible change had occurred in the animal. It had arisen on its feet, walked about the pen, took its food freely by the finger, and manifested great vivacity. In a few days his feeble condition had entirely disappeared. It began to grow, and its development was marvellous. On March 31, 1870, two months and five days after its birth, its rapid growth was so apparent that, as its hind quarter was then growing, he had it measured. Fifty days afterwards it had gained six inches in height, carrying its lateral development with it. The calf was turned into the barn yard, and manifested every symptom of full masculine vigour, though at the time he was only four months old. He is now one of the best developed animals that can be found anywhere.

This is only a very short *résumé* of the third edition of a pamphlet published by General Pleasonton, entitled, " On the Influence of the Blue Colour of the Sky in Developing Animal and Vegetable Life : as Illustrated in the Experiments of the Author between the years 1861 and 1871 " (Philadelphia, 1871). 8vo. 24 pp.

The account of it which I had addressed to the French Academy was followed by two different notes from Cailletet and Bert. In my next article I will examine them, with some references to the explanation of General Pleasonton's experiments.

Paris, Jan. 10 ANDRÉ POËY

MAGNETIC DISTURBANCES DURING THE LATE TOTAL ECLIPSE

IN the list of papers read before the Paris Academy of Sciences, which was given in last week's NATURE, I noticed one on the magnetic perturbations observed at Alençon during the late total eclipse. Now it would at first sight appear reasonable to expect that any effect produced on the magnetic needle at Alençon by a phenomenon whose maximum phase was as far removed as India or Australia, should have nearly equal effect on the needle in England, and in all countries adjoining France. It has moreover been established by frequent comparisons of carefully measured photographic records, taken at different magnetic observatories, that any disturbance of the earth's magnetic force is felt almost simultaneously at stations differing several hundred miles in both latitude and longitude. I was, therefore, ju tified in supposing that I should find some indications on our photo-magnetic records of a disturbance corresponding to the perturbations of the needle at Alençon, alluded to by M. Lion in his note to the Academy. The result of my examination of the records is, that there is not the slightest trace of a disturbance on either the vertical or horizontal curves, and that the declination magnet has been more than usually quiet, although on the two previous days it happened to have been somewhat disturbed about the hour at which the totality of December 11 occurred.

Accidental causes influence too largely the readings of a declination magnet for much reliance to be placed on them, however careful the observer, when they are in open contradiction to the photo-records of instruments whose diurnal corrections are sensibly constant.

Stonyhurst Observatory, Jan. 28 S. J. PERRY

SCHOLARSHIPS AND EXHIBITIONS FOR NATURAL SCIENCE IN CAMBRIDGE, 1872

THE following is a list of the Scholarships and Exhibitions for proficiency in Natural Science to be offered in Cambridge during the present year :—

TRINITY COLLEGE.—One or two of the value of about 80*l.* per annum. The examination will be on April 5, and will be open to all undergraduates of Cambridge and Oxford, and to persons under twenty who are not members of the Universities. Further information may be obtained from the Rev. E. Blore, Tutor of Trinity College.

ST. JOHN'S COLLEGE.—One of the value of 50*l.* per annum. The examination (in Chemistry, Physics, and Physiology, with Geology, Anatomy, and Botany) will be on the 12th of April, and will be open to all persons who are not entered at the University, as well to all who have entered and have not completed one term of residence. Natural Science is made one of the subjects of the annual College Examination of its students at the end of its academical year, in May ; and Exhibitions and Foundation Scholarships will be awarded to students who show an amount of knowledge equivalent to that which in Classics or Mathematics usually gains an Exhibition or Scholarship in the College. In short, Natural Science is on the same footing with Classics and Mathematics, both as regards teaching and rewards.

CHRIST'S COLLEGE.—One or more, in value from 30*l.* to 70*l.*, according to the number and merits of the candidates, tenable for three-and-a-half years, and for three years longer by those who reside during that period at the College. The examination will be on March 19, and will be open to the undergraduates of this College ; to non-collegiate undergraduates of Cambridge ; to all undergraduates of Oxford ; and to any students who are not members of either University. The candidates may select their own subjects for examination. There are other Exhibitions which are distributed annually among the most deserving students of the College.

CAIUS COLLEGE.—One of the value of 60*l.* per annum. The examination will be on March 19 in Chemistry and Experimental Physics, Zoology with Comparative Anatomy and Physiology, and Botany with Vegetable Anatomy and Physiology ; it will be open to students who have not commenced residence in the University. There is no limitation as to age.—Scholarships of the value of 20*l.* each, or more if the candidates are unusually good, are offered for Anatomy and Physiology to members of the College. —Gentlemen elected to the Tancred Medical Studentships are required to enter at this College ; these Studentships are four in number, and the annual value of each is 113*l.* Information respecting these may be obtained from Mr. B. J. L. Frere, 28, Lincoln's Inn Fields, London.

CLARE COLLEGE.—One or more of the value of 50*l.* per annum. The examination (in Chemistry, Chemical Physics, Comparative Anatomy and Physiology, and Geology) will be on March 19, and will be open to students intending to begin residence in October.

DOWNING COLLEGE.—One or more of the value of 40*l.* per annum. The examination (in Chemistry, Comparative Anatomy, and Physiology) will be early in April, and will be open to all students not members of the University, as well as to all undergraduates in their first term.

SIDNEY COLLEGE.—Two of the value of 40*l.* per annum. The examination (in Heat, Electricity, Chemistry, Geology, Physiology, Botany) will be in October, and will be open to all students who may enter on the College boards before October 1.

EMMANUEL COLLEGE.—One or more of the value of 40*l.* to 60*l.* per annum. The examination on March 19 will be open to students who have not commenced residence.

PEMBROKE COLLEGE.—One or more of the value of 20*l.* to 60*l.*, according to merit. The examination in June (in Chemistry, Physics, and other subjects), will be open to students under twenty years of age.

ST. PETER'S COLLEGE.—One from 50*l.* to 80*l.* per annum, according to merit. The examination, on April 4 (in Chemistry, Comparative Anatomy and Physiology, and Botany), will be open to students who will be under twenty-one years of age on October 1, 1872, and who have not commenced residence.

Although several subjects for examination are in each instance given, this is rather to afford the option of one or more to the candidates than to induce them to present a superficial knowledge of several. Indeed, it is expressly stated by some of the Colleges that good clear knowledge of one or two subjects will be more esteemed than a general knowledge of several.

Candidates, especially those who are not members of the University, will, in most instances, be required to show a fair knowledge of Classics and Mathematics, such, for example, as would enable them to pass the Previous Examination.

There is no restriction on the ground of religious denomination in the case of these or of any of the Scholarships or Exhibitions in the Colleges or in the University. Further information may be obtained from the tutors of the respective Colleges.

It may be added that Trinity College will give a Fellowship for Natural Science once, at least, in three years ; and that most of the Colleges are understood to be willing to award Fellowships for merit in Natural Science equivalent to that for which they are in the habit of giving them for Classics and Mathematics.

The following lectures in Natural Sciences will be delivered at Trinity, St. John's, and Sidney Sussex Colleges during Lent Term, 1872 :—

On Sound and Light. (For the Natural Sciences Tripos.) By Mr. Trotter, Trinity College, on Mondays, Wednesdays, and Fridays, at 10, commencing Monday, February 5.

On Electricity and Magnetism. (For the Natural Sciences Tripos, a short course in continuation of that of last term.) By Mr. Trotter, Trinity College, on Tuesdays and Thursdays, at 9, commencing Thursday, February 1.

On Electricity and Magnetism, for the special examination for the ordinary degree. By Mr. Trotter, Trinity College, on Tuesdays, Thursdays, and Saturdays, at 11, commencing Thursday, February 1.

On Chemistry. By Mr. Main, St. John's College, on Mondays, Wednesdays, and Fridays, at 12, in St. John's College laboratory, commencing Wednesday, January 31. Instruction in Practical Chemistry will also be given.

On Palæontology. (The Annuloida, &c.) By Mr. Bonney, St. John's College, on Mondays, Wednesdays, and Fridays, at 9, commencing Wednesday, January 31.

On Geology. (For the Natural Sciences Tripos. Physical Geology.) By Mr. Bonney, St. John's College, on Tuesdays and Thursdays, at 10, commencing Thursday, February 1.

A course on Stratigraphical Geology will be given in the Easter Term. Papers will be given every Saturday at 11.

Elementary Geology (for the special examination), on Tuesdays and Thursdays, at 11, commencing Thursday, February 6.

On Botany. (For the Natural Sciences Tripos.) By Mr. Hicks, Sidney College, on Tuesdays, Thursdays, and Saturdays, at 12, beginning on Thursday, February 1. The lectures during this term will be on Structural and Physiological Botany.

On the Physiology of the Nervous System. By the Trinity Prælector in Physiology (Dr. M. Foster), at the New Museums, on Mondays, Tuesdays, and Wednesdays, at 11, commencing Monday, February 5.

The Physiological Laboratory is also open for practical instruction in Physiology to all those who have gone through the elementary course.

NATURAL SCIENCE AT OXFORD

THE following regulations have been issued for the Final Honour Examination in the Natural Science School:—

BIOLOGY.—1. Candidates who offer themselves in the Final Honour Examination for examination in Biology will be expected to show an acquaintance, firstly, with General and Comparative Anatomy; secondly, with Human and Comparative Physiology, inclusive of Physiological Chemistry; and thirdly, with the General Philosophy of the subject.

2. In these subjects the candidates will be examined both by paper work and practically; and will be required to give evidence of being competent not merely to verify and describe specimens already prepared for naked-eye or microscopic demonstration as the case may be, but also to prepare such or similar specimens themselves.

3. The following works are provisionally recommended by the Board of Studies for use in the study of the above-mentioned Departments of Biology. When the letter F or G is prefixed to the title of a work, it will be understood to indicate that the work is written in French or German, and is not as yet translated into English:—

General Anatomy.—Sharpey in Quain's Anatomy, ed. 7, 1867; The Micrographic Dictionary, by Griffiths and Henfrey, now in course of re-publication; The Histological Catalogue of the College of Surgeons, by Prof. Quekett; (G) Kölliker's Handbuch der Gewebe-lehre, ed. 1867; Stricker's Handbook of Human and Comparative Histology, now in course of translation for the New Sydenham Society.

Comparative Anatomy.—Huxley's Introduction to the Classification of Animals; Huxley's Anatomy of Vertebrated Animals, 1871; (F) and (G) Gegenbaur's Grundzüge der Vergl. Anatomie, 1869; (F) Milne-Edwards, Leçons sur la Physiologie, 1857-1870; The Osteological and Physiological Catalogues of the College of Surgeons, by Prof. Owen; The Anatomical and Physiological Catalogues of the Oxford Museum; Flower's Osteology of Mammalia, 1871; (F) Cuvier's Ossemens Fossiles, ed. 2, 1821-1824; Rolleston's Forms of Animal Life, 1870; Bronn's Klassen und Ordnungen des Thierreichs, 1860-1871.

Human Physiology.—Carpenter's Human Physiology, ed. 7, 1869; (G) Funke's Lehrbuch der Physiologie, now in course of re-publication; (G) Hermann's Handbuch der Biologie, 1870; Dalton's Human Physiology; Draper's Human Physiology, 1856; (G) Ranke, Grundzüge der Physiologie, 1868; (G) Wundt's Lehrbuch der Physiologie, 1865; (G) Ludwig's Lehrbuch der Physiologie, 1858-1861; (G) Budge's Lehrbuch der speciellen Physiologie des Menschen, 1862.

Comparative Physiology.—Carpenter's Comparative Physiology, 1854; Marshall's Outlines of Physiology, 1867; (F) Milne-Edwards' Leçons sur la Physiologie; (G) Bergmann and Leuckart, Anatomisch-physiologische Uebersicht des Thierreichs, 1855.

General Philosophy of Biology.—*a.* Darwin's Origin of Species; Van der Hoeven's Philosophia Zoologica, 1864, Lyell's Principles of Geology, ed. 1870, chaps. xxxiv—xxxvii.; Mivart's Genesis of Species; Spencer's Principles of Biology, 1864-1867; Principles of Psychology, ed. 1868-1871; *b.* Agassiz's Essay on Classification, chap. iii.; Whewell's History of the Inductive Sciences (For a Historical Survey of the Progress of Biology); *c.* Van der Hoeven's Handbook of Zoology, 1857; Nicholson's Manual of Zoology, ed. 2, 1871 (For Zoology); Van der Hoeven's Philosophia Zoologica, lib. iv.; Lyell's Principles of Geology, chap. xxxviii—xli. (For Geographical Distribution).

Ethnology and Anthropology.—Waitz's Anthropology; Brace's Races of the Old World, ed. 2, 1870.

4. Candidates may, in addition to the amount of work indicated in the preceding paragraphs, bring up any of the "Special Subjects" contained in the list appended below. A candidate who offers himself for examination in a special subject will be expected to show, firstly, a detailed practical acquaintance with specimens illustrating that subject, for which purpose the catalogues in the University Museum can be made available; and, secondly, exact knowledge of some one or more monographs treating of it. Excellence, however, in a special subject will not compensate for failure in any essential part of the general examination. Every candidate must state, at the time of entering his name for examination, what special subject, if any, he takes in. List of special subjects and of books recommended in connection with them:—

Comparative Osteology.—Cuvier's Ossemens Fossiles, any one of the five volumes; Flower's Osteology of Mammalia; Prof. Huxley's Anatomy of Vertebrated Animals.

The Comparative Anatomy and Physiology of the Organs of Digestion.—The Physiological Catalogue of the Royal College of Surgeons, vol. i.; (F) Milne-Edwards's Leçons, vol. vi.; Articles "Stomach and Intestine" and "Pancreas" in Todd's "Cyclopædia of Anatomy and Physiology;" (F) Schiff, Leçons sur la Physiologie de la Digestion, 1868.

The Comparative Anatomy and Physiology of the Organs of Circulation and Respiration.—(F) Milne-Edwards's Leçons sur la Physiologie, vol. iii.; (F) Marey's Physiologie Médicale de la Circulation du Sang, 1863; (F) Bert, Leçons sur la Physiologie Comparée de la Respiration, 1870.

The Comparative Anatomy and Physiology of the Nervous System.—(F) Leuret's and Gratiolet's Anatomie Comparée du Système Nerveux, Tom. ii., par M. Pierre Gratiolet, 1857; (F) Vulpian's Leçons sur le Système Nerveux; Brown-Séquard's Lectures, 1865.

The Comparative Anatomy and Physiology of the Reproductive Systems. — Physiological Catalogue of the Royal College of Surgeons, vols. iv. and v.; (G) Kölliker's Entwickelungsgeschichte, 1861; (F) Milne-Edwards Leçons, vol. ix.

Ethnology.—Brace's Races of the Old World, ed. 2, 1870.

5. Candidates who offer themselves for examination in Geology, Zoology, or Botany will be required to exhibit practical acquaintance with those subjects to at least the same extent as candidates who offer themselves for examination in any one of the special subjects above mentioned are required to do with reference to those subjects. But they will not be required to go through the same amount of practical work in the departments of Biology not specially connected with Geology, Zoology, or Botany as candidates who do not bring up any one of these three subjects.

NOTES

The Senior Wrangler for the present year is Mr. Robert Rumsey Webb, son of the late Mr. Thomas Webb, of Monmouth. He was educated under the Rev. C. M. Roberts, M.A. (St. John's College, Cambridge), at the Monmouth Grammar School, and entered at St. John's College in October 1868, having previously obtained a Somerset Exhibition by open competition. Mr. Webb's college tutor was Mr. J. E. Sandys; his private tutor Mr. Routh, of St. Peter's. Mr. Horace Lamb, the Second Wrangler, was born at Stockport, in November 1849, was educated at the Stockport Grammar School, and for a short time studied at Owens College, Manchester. In the year 1868 he gained a minor scholarship at Trinity College, and in 1870 was elected to a Foundation Scholarship. He was placed in the first class in the First B.A. Mathematical Honour Examination in the University of London in 1870; and in the succeeding year gained the Sheepshanks Astronomical Exhibition at Trinity College. His college tutor was Mr. Prior; private tutor, Mr. Routh, of St. Peter's. Mr. John Bascombe Lock, the Third Wrangler, son of Mr. Joseph Lock, of Dorchester, was educated at the Bristol Grammar School. In the Easter Term of 1868 he obtained an open Mathematical Scholarship at Caius College, where he obtained a Foundation Scholarship in May. Mr. Routh was his private tutor, and Mr. N. M. Ferrers his college tutor.

The following are the lectures on Science at the University of Oxford this term:—The Rev. Bartholomew Price, the Sedleian Professor of Natural Philosophy, on Light; the Savilian Professor of Astronomy, Rev. C. Pritchard, on Newton's "Principia" and the Lunar Theory; Prof. Clifton, Professor of Experimental Philosophy, on Experimental Optics; Prof. Westwood, Professor of Zoology, on the Classes and Orders of Articulated Animals; Prof. Phillips, Professor of Geology, on the Geology of the country round Oxford; Prof. Rolleston, Professor of Anatomy, on Digestion. In addition to these lectures Prof. Clifton announces that the physical laboratory of the University will be open daily for instruction in Practical Physics from 10 to 4 o'clock each day. Prof. Rolleston proposes to form classes for practical instruction as in former Terms. The Chemical Laboratory is open as usual for Quantitative and Qualitative analysis. Dr. Acland, the Regius Professor of Medicine, also announces that, in addition to his course of clinical instruction at the infirmary, he "will also on days and places to be hereafter mentioned demonstrate on the spot sanitary defects in a town and in a village, illustrating thereon principles of general and special sanitary administration." In the Laboratory of the Medical Department at the University Museum various methods of examining water and other subjects connected with sanitary science will be taught, commencing on February 1, by Mr. C. C. Pode, M.B., Exeter College, with the assistance of Mr. S. J. Sharkey, B.A., of Jesus College. Those lectures and demonstrations on sanitary matters are a novel and peculiarly-interesting feature introduced this Term for the first time.

Dr. Paget has been appointed Regius Professor of Medicine at the University of Cambridge.

The Professorship of Botany in the Royal College of Science for Ireland is vacant by the resignation of Prof. W. T. Thiselton-Dyer.

The King of Italy has conferred upon Mr. Edward Whymper, Vice-President of the Alpine Club, the decoration of Chevalier of the Order of St. Maurice et Lazare, "in recognition of the value of his recently published magnificent work upon the Alps."

We have to record the death, on Saturday last, of Dr. W. Baird, F.R.S., of the Zoological Department of the British Museum, at the age of 69.

The American Academy of Arts and Sciences on the 9th of January presented the American Rumford Medals to Mr. J. Harrison, jun., of Philadelphia, for his invention of safety boilers. The medals are provided for by an endowment fund or gift of 5,000 dols. in the United States Funds, to the Academy, made by Count Rumford in 1796. By the conditions of this endowment the interest of the fund is to be applied "every second year" to the procuring of two medals, one of gold and one of silver, in value equal to the amount of two years' interest of the fund (600 dols.), and these medals (or their equivalent in money) are to be awarded to the author of the most important discovery or useful improvement in the application of heat or light, which shall, in the opinion of the Academy, "tend most to promote the good of mankind." Although the fund was provided at that early day no discovery or improvement of sufficient importance, in the opinion of the Academy, appeared until 1859, when the first award was made to Dr. Robert Hare, of Philadelphia, for his compound oxy-hydrogen blowpipe and improvements in galvanic apparatus. Since then the awards of the medal have been as follows:—1862, John B. Ericsson, for his caloric engine; 1865, Prof. Daniel Treadwell (Harvard College), for improvements in the management of heat; 1867, Alvan Clark, for improvement in lens of refracting telescope; 1870, George H. Corliss, Providence, for improvements in the steam-engine; 1871, Joseph Harrison, jun., Philadelphia, for "the mode of constructing steam boilers invented and perfected by him," which "secures great safety in the use of high-pressure steam, and is, therefore an important improvement in the application of heat."

A meeting in aid of the Livingstone Exploration Fund was held in the City of London on Tuesday last, the Lord Mayor in the chair; the subscriptions received in the room amounting to over 250*l.* Sir H. Rawlinson announced at the meeting that he had that day received from the Foreign Office a despatch which was to be presented by Lieutenant Llewellyn Dawson to the Government agent at Zanzibar, in which Dr. Kirk was instructed to give to Lieutenant Dawson all the advice and assistance in his power, and was authorised to advance any sum which might be required for the purposes of the expedition within the limit of the balance of the Government grant of 1,000*l*, which remained in his hands, and which, according to the last account, amounted to 650*l.* He also stated that the subscriptions already received reached 2,700*l.* or 2,800*l.*

The Second Course of Cantor Lectures for the session will be delivered by the Rev. Arthur Rigg, M.A., on "Mechanism." The first lecture will be given on Monday evening, Feb. 5, at eight o'clock, and the remainder on following Mondays till March 11.

The *Athenæum* states that Mr. E. J. Reed, C.B., late Chief Constructor of the Navy, is about to establish a new quarterly magazine of a scientific character, the first number of which will appear early in March, to be devoted to the improvement of naval architecture, marine engineering, steam navigation, and seamanship generally. It will be called *Naval Science*, and will be under the joint editorship of the Rev. Dr. Woolley, Director of Education to the Admiralty, and Mr. Reed.

The following are the number of entries for the classes at the Newcastle College of Physical Science for the present term:—Evening classes—chemistry, 36 (including one lady); physics, 40 (including four ladies); geology, 19; advanced mathematics, 15; elementary mathematics, 24; political economy, 12 (this class will not be held). Day classes. New entries at Epiphany term—chemistry, 8; physics, 7; mathematics, 8; geology, 3.

The following are appointed trustees to the Alder Memorial Fund, of which we spoke last week:—Sir W. G. Armstrong, Mr I. L. Bell, Mr. J. Blacklock, Mr. H. B. Brady, Mr. A. Hancock, Mr. D. P. Morison, Mr. R. S. Newall, and the Rev. A. M. Norman; who have received the following suggestion from the subscribers:—"That it should be suggested to the trustees that the establishment of a College of Physical Science in Newcastle appears to offer opportunities for the employment of the fund in furtherance of zoological science, more likely to be generally appreciated as a memorial of our late distinguished naturalist than the scheme originally proposed, and that in the event of the establishment of a chair of biology in the College, the application of the interest of the fund might properly take the form of a scholarship or other reward for proficiency in zoology, to be associated with Mr. Alder's name."

The Natural History Society of Newcastle-on-Tyne has received a gift of 20*l.* from the Misses Bewick; which sum is to be applied in defraying the cost of new cabinets as they might be required. The Society has also been presented with a most valuable collection of fossils from Mr. M. R. Pryor, Fellow of Trinity College, Cambridge, consisting of about 140 species of Upper Greensand fossils from Cambridgeshire; 130 species from the Red Crag; a fine series from the Lower Greensand Coprolite Bed of the Eastern Counties; a fair representative collection from the Oxford Clay at St. Ives, Huntingdon; and a number of Chalk fossils; all admirably mounted and named.

A resolution has been presented to the Congress of the United States providing for the printing of a number of copies of the report of the investigation by Prof. Hayden upon the geology of Nebraska and Wyoming territory.

In a letter from Government House, Barbadoes, January 6, 1872, to one of our contributors, the Hon. Rawson Rawson writes:—"Agassiz, Count Pourtales, and a party of *savans* have just left this. The United States surveying vessel in which they go to the Pacific had to put in for some slight repairs. They were here for two days and I went on board and spent one day with them. Agassiz pronounced my collection of shells quite unique in series of specimens, from the youngest stage to adult. He was in ecstacies with the *Holopus*,* which he spent hours in examining, and I had to let him take it away to describe it in all detail. He had seen and studied D'Orbigny's *H. rangii*, and thinks mine the same species, but that it is of the normal form, while the one D'Orbigny described was both incomplete and abnormal. I had Dr. Gray's sketch with me, and certainly the resemblance to it was very great. I think I may fairly regard it as the gem of my collection; but in writing of it I must not forget to tell you of our day's dredging. It was successful beyond our expectations—four live specimens of a fine new crinoid, like *Apiocrinus*, which Agassiz was able to watch alive for hours; a *Pleurotomaria Quoyana*, of which the artist was able to draw the animal; a new and wonderfully beautiful species of *Latiaxis*, Brachiopods in any number, vitreous sponges in mass, some new Echini. You can fancy the state Agassiz was in, and time would quite fail me to tell you of all the interesting things he said about the various forms as he recognised them. Need I say that all this has determined me to make an effort to get our shores dredged, beginning in shallow and going out to the depth they dredged at, *i.e.*, about forty or fifty fathoms. We shall, doubtless, get lots of treasures, upon the duplicates of which you shall have first claim."

* Vide Notes on *Holopus*. By Dr. J. E. Gray. Annals and Mag. Nat. Hist, vol. viii., 4th series, p. 394.

Prof. B. A. Gould writes from the Argentine National Observatory at Cordoba, under date December 8, 1871, that the new observatory had then been formally inaugurated about six weeks, after a series of most unexpected and vexatious obstacles and delays. The climate had, however, proved far less propitious than had been expected, the cloudy nights being nearly as numerous as the clear ones, although no rain falls during one half the year. When, however, the sky is clear, it is of a wondrous transparency, stars of the seventh magnitude being distinctly visible on favourable nights to the naked eye, and the planets magnificent in their brilliancy. The large equatorial was already in adjustment, and Prof. Gould had had some beautiful views of Saturn. Owing to the breaking out of the epidemic in Buenos Aires at the beginning of 1871, all communication with Europe, by post or otherwise, had been almost entirely suspended during the year; faint rumours of the success of the eclipse observations in Spain in December 1870 had but just reached Cordoba.

A black marble slab, bearing the following inscription in brass characters, has just been placed over the grave of the late Sir John Herschel, in the north aisle of the nave of Westminster Abbey:—

JOHANNES HERSCHEL
GULIELMI HERSCHEL
NATU OPERE FAMA
FILIUS UNICUS
"CŒLIS EXPLORATIS"
HIC PROPE NEWTONUM
REQUIESCIT
GENERATIO ET GENERATIO
MIRACILIA DEI NARRABUNT
PSALM. CXLV. 4, 5.
VIXIT LXXIX. ANNOS
OBIIT UNDECIMO DIE MAII
A D. MDCCCLXXI.

The following account of the fall of a meteorite is taken from Gruithuisen's "Naturgeschichte des Gestirnten Himmels:"—On July 24, 1790, at 10.30 P.M. a fiery globe larger and brighter than the full moon, as seen from Mormes, passed from S. to N. in 2 s., and burst leaving a white cloud. 3 m. after explosion the two observers heard a heavy thunder-clap that shook the windows and opened some of them. The 15 leagues distant chain of the Pyrenees gave a continuous echo lasting 4s. The fragments fell in extraordinary quantity between Juliac and Barbotan, 4 hours N. and 5 hours NE. from Mormes; they fell fused so as to bake the impression of straw, and make no sound on the roof of houses, weighing 4 "loth" to 20 "pfund." The ball of fire was seen from Bayonne, Auch, Pau, Tanbes, Bordeaux, and Toulouse, from the latter place only as something larger than a fixed star.

We give a fuller account of the volcanic eruption at Ternate alluded to by our correspondent Mr. A. B. Meyer in NATURE for January 18. The Batavia *Handelsblad* of Sept. 25 states that on the afternoon of Aug. 7 a violent earthquake was felt, of which the exact direction was unknown. The Ternate mountain had from 9 A.M. caused a dull, rumbling sound to be heard, varied at intervals by loud reports, and began in the course of the day to cast out streams of lava. The sky looked dark, and the whole country round about was darkened by the down-coming clouds of smoke. Luckily a southerly wind sprung up, which gave another direction to the glowing lava-streams flowing landwards, and led the fire in seven currents to the ravines. This frightful natural phenomenon held on during the night between the 7th and the 8th. The inhabitants, thinking their island to be doomed, could not sleep, and passed the night outside their houses looking up anxiously at the furious volcano which seemed to threaten them all with certain destruction. At day-break the outburst became still worse, and the population began to fly to the islands of Tidore and Halmaheira. The eruption of fire and stones held on for about twelve days, after which it became less. The damage done to houses and plantations is enormous, but has not as yet been accurately ascertained. This outburst was the most violent known at Ternate within the memory of man. The whole island shook from the underground motion. A moment of rest was followed by another explosion, which shook the houses to their foundations. There were, luckily, only some slight earthquake-shocks felt. On Aug. 28 the volcano was again at rest, at least, only a small cloud was seen coming out of the crater.

WE take the following from the *Times of India* :—"The *Western Star*, which is *par excellence*, the journal for marvels, tells the following story of a murder :—The manner in which the murderers were detected would, our contemporary adds, if true, go far to prove the Darwinian theory. The story briefly told is this : A Madrassee had a monkey which he was very fond of. The man had occasion to go on a journey, and took with him money and jewels, and his chum the monkey. Some rogues determined to rob him of everything he had ; accordingly they lay in wait for him and murdered him. Having secured the money and jewels they threw the murdered man into a dry well, and having covered it up with twigs and dry leaves, they went home. The monkey, who was on the top of a tree, saw the whole of the proceedings, and when the murderers departed he came down and made tracks for the Tahsildar's house, and by his cries and moans attracted the attention of that functionary. Inviting the Tahsildar by dumb signs to follow him, the monkey went to the well and pointed downwards. The Tahsildar thereupon got men to go down, and of course the body was discovered. The monkey then led the men to the place where the jewels and money were buried. He then took them to the bazaars, and as soon as he caught sight of one of the murderers he ran after him, bit him in the leg, and would not let him go till he was secured. In this way all the murderers were caught. The men, it is said, have confessed their crime, and they now stand committed for trial before the Tellicherry Court at the ensuing session. That monkey, we think, ought to be made an inspector of the police."

THE Panama papers report an increasing demand for the Colombian gaucho, and urge the Government to the enactment of regulations to prevent the entire destruction of the forests of these trees in Darien, where they are most abundant. Instead of simply treating the trees for the juice, as the maples are managed in the United States, the tree is cut down, and, of course, no further benefit can be derived from it. In illustration of the extent to which this vegetable product is now being collected, the *Panama Star and Herald* informs us that 160 tons had just been brought to that city as the cargo of a single vessel, mostly from the vicinity of Guayaquil.

SCIENTIFIC SERIALS

THE *Scottish Naturalist* for January.—This number is mainly occupied by a number of short papers illustrative of various subjects of interest or novelty in the natural history of Scotland, among which we may notice especially the British species of *Crambus*, a genus of moths, by the Editor ; on the Cachalot or Sperm-whale (*Physeter macrocephalus*) of the north-east of Scotland, by Robt. Walker, with plate ; and the commencement of the Editor's "Insecta Scotica," an essay to catalogue the insects inhabiting Scotland, with a map to show the natural divisions of the country into the 12 districts adopted in the list. The introductory remarks to the Editor's catalogue of Lepidoptera are valuable, and the article, when completed, promises to be an important contribution to British zoological literature.

THE *American Journal of Science and Art* for November 1871 opens with a continuation of Prof. Le Conte's elaborate paper on "Some Phenomena of Binocular Vision." Prof. Dana, in an article on the position and height of the elevated plateau in which the glacier of New England in the glacial era, had its origin, considers that the idea of one central glacier source for the whole continent is without foundation. The icy plateau he locates at the watershed between the St. Lawrence valley and Hudson's Bay at an altitude at least 4,500 feet above the present level. With the exception of a preliminary catalogue of the bright lines in the spectrum of the chromosphere, by Prof. C. A. Young, which we propose to reprint, the remaining papers in this number are chiefly chemical, and of varied interest, but of which it would be impossible to give the substance in the form of a brief abstract.

The first article in the December number treats of the geological history of the Gulf of Mexico, and is accompanied by a map, which is, unfortunately, not coloured, and is hence somewhat obscure. The article is divided into three portions, treating respectively of the cretaceous period, the tertiary period, and the quaternary beds. This is followed by an article by Asaph Hall, on the Astronomical Proof of a Resisting Medium in Space. It will be remembered that one of the main proofs of the existence of the interstellar æther is the retardation of Encke's Comet. So long ago as the year 1819 Encke calculated that the periodic times of the comet had diminished to the extent of more than half a day during thirty-three years. Thus the periodic time between 1786 and 1795 was 1,208·112 days, while between 1805 and 1819 it was 1,207·424 ; and in order to account for the diminution, Encke adopted the hypothesis of a resisting medium in space. From later observations of this and other comets, Mr. Hall is led to the conclusion that comets furnish no proof of the existence of the æther, and that the retardation of Encke's comet is due to some unknown cause, possibly to the fact of its passing through streams of meteoric matter, which may influence its motion. —Mr. Southworth gives an account of a new Micrometric Goniometer eye-piece, formed by means of a micrometer capable of measuring to the $\frac{1}{25000}$ of an inch.—Dr. Dawson contributes an article on the bearing of Devonian Botany on questions as to the Origin and Extinction of Species, in which he expresses a hope that the further study of fossil plants may enable us thus to approach to a comprehension of the laws of the creation, as distinguished from those of the continual existence of species. The other articles relate to the American Spongilla, a Craspelote, Flagellate Infusorian, by Professor H. James Clark ; description of a Printing Chronograph, by the use of which it has been proved that "for three observers, twice as many observations can be reduced in the same time as when a recording chronograph is employed." The next paper was read before the American Association at Indianopolis, and discusses the longitude determination across the Continent. This embodies results obtained by the Coast Survey, in their endeavours to determine the longitude of San Francisco and various intermediate points by telegraphic exchange of clock signals with the Harvard Observatory.—The remaining papers treat of the Invertebrata dredged in Lake Superior in 1871 ; and of Kilauea and Mauna Loa.

In the number for January 1872, the commencement of Vol. iii. of the new series, we find a valuable article on Alpine geology by Prof. Sterry Hunt, in the form of a review of Favre's *Recherches Géologiques*. Mr. John De Laski notices the evidence of glacial action on Mount Katahdin, the highest land in Maine, and of the Devonian formation, now 5,000 feet above the sea, the top of which he believes to have been overridden by the glacier. The total thickness of the glacier he estimates at not less than 8,000 feet, and believes that by the slow grinding motion of this ice-sheet all the surface of New England became broken up to great depths. We have again a number of chemical articles, and an interesting contribution to geology by Mr. C. H. Hitchcock, on the Norian or Upper Laurentian Group of New Hampshire. In this number there is also, as usual, a variety of miscellaneous information on the various branches of physical and natural science.

SOCIETIES AND ACADEMIES

London

Royal Society, January 25—"On the Action of Low Temperatures on Supersaturated Solutions of Glauber's Salt." By Charles Tomlinson, F.R.S.

"On the Elimination of Alcohol." By A. Dupré, Lecturer on Chemistry at Westminster Hospital. Communicated by W. Odling, F.R.S.—Obviously three results may follow the ingestion of alcohol. All the alcohol may be oxidised and none be eliminated, or a portion only may be oxidised and the rest be eliminated unaltered; or, lastly, all may be eliminated again unaltered. Assuming the last to be the case, it would follow that, if a certain quantity of alcohol be taken daily, the amount eliminated would increase from day to day until, at last, the amount eliminated daily would equal the daily consumption, be this time five, ten, or more days. If, on the other hand, all the alcohol consumed is either oxidised or eliminated within twenty-four hours, no increase in the daily elimination will take place in consequence of the continuance of the alcohol diet. Guided by these considerations the author undertook two series of experiments, in which the amount of alcohol eliminated by both kidneys and lungs was carefully estimated. The analytical processes employed are described in detail. First series:—After a total abstinence from alcohol for eleven days, the urine and breath were examined, after which, from the 12th to the 24th day, both inclusive, the author took 112 cub. centims. of brandy daily (equal to 48·68 grms. absolute alcohol). The urine and breath were examined on the 12th, the 18th, and the 24th day. The urine was also examined during the five days following the cessation of the alcohol diet. The analytical results obtained are given in a table. Second series:—After having again abstained from the use of alcohol in any shape during ten days, the author took 56 cub. centims. of brandy (same as above) at 10 A.M. on March the 29th. The urine was collected from three to three hours up to the 12th, from the 12th to the 24th, and during the next succeeding two days. The alcohol eliminated in the breath was also estimated during the same intervals. The analytical results are also arranged in a tabular form. The results of both series may be summed up as follows:—The amount of alcohol eliminated per day does not increase with the continuance of the alcohol diet; therefore all the alcohol consumed daily must, of necessity, be disposed of daily; and as it certainly is not eliminated within that time, it must be destroyed in the system. The elimination of alcohol following the ingestion of a dose or doses of alcohol ceases in from nine to twenty-four hours after the last dose has been taken. The amount of alcohol eliminated, in both breath and urine, is a minute fraction only of the amount of alcohol taken. In the course of these experiments, the author found that, after six weeks of total abstinence, and even in the case of a teetotaller, a substance is eliminated in the urine, and perhaps also in the breath, which, though apparently not alcohol, gives all the reactions ordinarily used for the detection of traces of alcohol, viz., it passes over with the first portions of the distillate, it yields acetic acid on oxidation, gives the emerald-green reaction with bichromate of potassium and strong sulphuric acid, yields iodoform, and its aqueous solution has a lower specific gravity and a higher vapour tension than pure water. The presence of a substance in human urine and the urine of various animals, which yields iodoform, but is not alcohol, had already been discovered by M. Lieben. The quantity present in urine is, however, so small that the precise nature of this substance has not as yet been determined. Finally, the author points out an apparent connection between this substance and alcohol. It was found that, after the elimination due to the ingestion of alcohol had ceased, the amount of this substance eliminated in a given time at first remained below the quantity normally excreted, and only gradually rose again to the normal standard. A careful study of this connection may perhaps serve to throw some light upon the physiological action of alcohol.

"The Absolute Direction and Intensity of the Earth's Magnetic Force at Bombay, and its Secular and Annual Variations." By Mr. Charles Chambers, F.R.S., Superintendent of the Colaba Observatory.—The observations discussed in this paper were taken at the Colaba Observatory during the years 1867 to 1870, and consist of observations of Dip, Declination, and Horizontal Intensity. The principal results deduced by the author from these observations are shown in the following statement:—

Magnetic Element	Epoch.	Value at epoch.	Value at common epoch, January 1st, 1869.	Secular change. Per annum.	Semiannual inequality. Excess of April to September over mean of year	Calculated probable error of a single weekly determination.
		° ′ ″	° ′ ″	′ ″	″	″
Declination . .	April 1, 1868	0 46 47 E.	0 48 36 E.	+2 5	+1	±20
Dip	Oct. 1, 1868	19° 4′·2	19° 4′·7	+1·9	+0′·3	±0′·25*
Horizontal Force .	April 1, 1869	8·0591	8·0581	+·0040	·0000	±·0043*
Total Force . .	Jan, 1. 1869	8·5264	8·5264	+·0059	+·0003	—

In column 2 is entered the mean epoch to which the mean value of each element, entered in column 3, corresponds.

The absolute observations were taken at a height of 38 feet above the ground, and by comparing them with observations taken with differential instruments at a height of 6 feet above the ground, they are shown to indicate distinctly a diminution of terrestrial magnetic action with increase of height, with respect both to secular variation of Declination and Horizontal Force, and to diurnal inequality of Horizontal Force.

Royal Geographical Society, January 22.—Sir H. C. Rawlinson, president, in the chair.—Mr. C. R. Markham, secretary, read, at the request of the president, the following statement regarding the proposed Exhibition for the Search and Relief of Dr. Livingstone:—"Letters were received from Livingstone, dated at Lake Bangweolo, on July 8, 1868, and the last that have come to hand were dated Ujiji, May 30, 1869. He announced that the work still before him was to connect the lakes he had discovered; and he intended to explore a lake to the westward of Tanganyika, in the Manyema country, and thence to complete his labours, but he was sorely in need of men and supplies. The Arab traders interested in the slave-trade were anxious to thwart him, and no one would take charge of his letters. He mentioned having written thirty-four letters which had been lost. This is the last positive news from Dr. Livingstone. There was one Arab report in November 1870, that he was at the town of Manakoso, with few followers, waiting

* In English units.

for supplies, and unable to move; but the last certain intelligence will be three years old on the 30th of next May. The question now is, shall this great and noble-hearted man be left to his fate? In January 1870 the Treasury sanctioned a grant of 1,000*l.* to send stores by natives from Zanzibar through the political agent; but this method of affording relief failed, and neither letters from Livingstone nor proof that he ever received the stores have reached the coast. Mr. Stanley, an American traveller, has also attempted to penetrate into the interior, but he was stopped by disturbances at Unyanyembe. It has thus become clear that, if Livingstone is to be relieved, a properly equipped expedition, ably commanded, must be despatched from this country to do the work. The Lords of the Treasury have declined to grant any pecuniary aid to the expedition which is destined to bring succour to Dr. Livingstone, who, it must always be remembered, is Her Majesty's Consul for the interior of Africa. No adverse decision from the Treasury will, however, be allowed to check the necessary preparations, nor to retard them for a single day. The known facts upon which the Council of the Society have had to base their decision are few, but they all pointed to one obvious course. According to the latest rumours, which were to some extent corroborated by the great traveller's expressed intention, Dr. Livingstone is in the Manyema country, to the westward of Lake Tanganyika, where he may be prostrated by sickness, and where, at all events, according to his last letters, he was urgently in want of supplies. As experience has proved that it would not be safe to entrust the charge of supplies to the Arab traders, the only alternative is to despatch a relief expedition led by Europeans, and the Council of the Society had determined on that course. The fortunate accident that an excellent opportunity offered itself of reaching Zanzibar in the first steamer that has ever made the direct voyage by the Suez Canal was a sufficient reason for the rapidity with which it was necessary to prepare and despatch the expedition. Nearly 200 persons had volunteered to take part in the expedition, and the choice of a leader had fallen upon Lieut. Llewellyn Dawson, R.N., a scientific seaman, who possessed most of the qualifications which were needed to fill so difficult and trying a post, and in whose ability and judgment the Council had perfect confidence. It was intended that he should be accompanied by a second in command, and the Foreign Office had applied to the Admiralty that any naval officer who served on this expedition should be rated on one of Her Majesty's ships, so as to be allowed time and full pay. Mr. W. Oswell Livingstone, Livingstone's son, who was born twenty years ago in the neighbourhood of Lake N'gami, would also accompany the expedition. It was hoped that Mr. New, a gentleman connected with the Mombas Mission, would act as interpreter, and the party would in all consist of an escort of about fifty picked men, besides porters. It would leave England early in February in the *Abydos* steamer, chartered by Messrs. J. Wiseman and Co., who had generously undertaken to convey all stores free of charge, and, if possible, to secure reduced charges for passages for the members of the expedition." A discussion ensued on the reading of this statement, in which Mr. J. R. Andrews, Dr. Purcell, Mr. Lee, Mr. J. Ball, Admiral Collinson, Mr. Thorpe, the Rev. Horace Waller, and others joined. The letter from the Treasury declining to aid was called for and read, and comments made on the possible meaning of the chief sentence in the letter—"A new expedition is not the only means left through which Dr. Livingstone's safety may with reason be hoped for." The following communications were read:—1. "Letter to Dr. Kirk on an Ascent of Kilimanjaro." By the Rev. Charles New, of Mombasa. This letter describes the recent visit of the author to Chagga and his ascent of Mount Kilimanjaro to the snow-line. Mr. New had made a collection of plants growing in the upper zones of vegetation on the mountain which he had forwarded to Dr. Hooker at Kew. He described the varied zones, from the tropical country at the base of the mountain up to the magnificent snow-coloured dome which forms the summit. The lower slopes were covered with dense forests of gigantic trees clothed with mosses, the upper of heaths and green pastures. 2. "Ascent of the Padass River and Visit to the *Muruts* Country in Northern Borneo." By Lieut. C. de Crespigny, R.N. This journey was undertaken in the search of ourang-otangs or *Mias*, which abound in that part of Borneo. The Padass rises on the slopes of the lofty mountain Kini-balu, and flows through a plain furrowed by the courses of many other rivers. Much information was given concerning the *Paluans* and *Muruts*, and other little-known tribes, and cases of the employment of ourang-otangs as domestic servants, employed to collect fire-wood, &c., were given.

Entomological Society, January 22.—Mr. A. R. Wallace, president, in the chair.—The Rev. T. A. Marshall, and Messrs. H. W. Bates, A. Müller, and F. Smith, were elected into the council, to replace members retiring therefrom. Prof. Westwood was elected president; Mr. S. Stevens treasurer; Messrs. McLachlan and Grut, secretaries; and Mr. Janson, librarian. The retiring president read an address, and the meeting ended with the usual votes of thanks to the officers.

Victoria Institute, January 22.—Mr. Charles Brooke, F.R.S., in the chair.—Dr. W. M. Ord, "On the influence of colloid matters upon crystalline forms," illustrated by numerous diagrams and specimens. Having briefly defined the use of the terms, he proceeded to show that when crystalline substance was deposited in a colloid, such as gum or albumen, it assumed not a crystalline, but a globular form. Diagrams showing the various changes that took place illustrated this part of the lecture. The action of salt water on the carbonate of soda in the case of the shell of the lobster, and the changes in the organisms were explained; the formation of bone in hawk-man tortoise and the codfish were alluded to, and Dr. Ord concluded by drawing attention to the importance of the investigation of the chemistry of colloids.

GLASGOW

Geological Society, January 11.—Mr. John Young, vice-president, in the chair. Mr. James Thomson, F.G.S., read a paper on *Palæocoryne scoticum* and *P. radiatum* from the carboniferous shales of the West of Scotland. He stated that at the first excursion of the society to Corrieburn, in 1858, he had observed in some portions of shale a small, delicate, stellate body which he could not refer to any genus or species he had seen described. Since then, at various Saturday aftertnoon excursions of the society, he had discovered other forms of a similar kind. He had consulted the collections of the Geological Society, the Government Museum of Practical Geology, and the British Museum in London, without finding any similar organisms; and lacking the necessary facilities for prosecuting the work himself, he had at length placed them in the hands of Prof. Duncan, in order that they might be identified and named. On investigation Prof. Duncan found them to be new and undescribed forms which could only be referred to the *Hydrozoa*. The calcareous investments of these *Palæocorynida* made their recognition as true Hydrozoa a matter of some difficulty; but this had been overcome by the examination of the anomalous genus *Bimeria* (Wright), which, as pointed out by Prof. Duncan, shows a very decided resemblance to the fossil under consideration, the semi-solid investment being continued over the greater part of the tentacles and upper part of the body. These minute but interesting forms are found both in our highest and lowest beds of limestone—at Roughwood, Broadstone, Auchinskeigh, and Gare—and their discovery may be said to add another link to the chain that unites the present with the remote past.—Mr. Thomson also gave notes on a new species of *Palæchinus*, from the limestone shale of Auchinskeigh. It was most nearly allied to Dr. Scoular's species *sphæricus*, but differed in the form and ornamentation of both the ambulacral and interambulacral plates. He proposed to name it provisionally *Palæchinus scoticus*. Mr. Thomson exhibited the fossils and some beautiful microscopic sections in illustration of his paper.

PARIS

Academy of Sciences, January 22.—A paper by M. J. Boussinesq on the geometrical laws of the distribution of pressures in a homogenuous and ductile solid, subjected to plane deformations, was communicated by M. de Saint-Venant.—M. Faye read a note on Encke's comet and the phenomena which it presented at its last appearance.—A sixth letter from Father Secchi on the solar protuberances was read, containing a tabulated summary, with explanations of all the observations made upon the protuberances during the year 1871.—M. Trémaux forwarded a note on phenomena indicating the condition of the sidereal medium.—M. Delaunay communicated a note by MM. Prosper and Paul Henry, on the construction of very detailed celestial maps, and exhibited a map prepared by them on the principle indicated.—A note on the Meteorological Annual of the Paris Observatory for 1872, by M. E. Renon, was read; the author criticises some of the numerical results given in that volume.—M. E. Dubois presented a note on a marine gyroscope.—M. H. de Jacobi communicated his researches on the induction currents produced in the coils of an electro-magnet between the poles of which a metallic disc is set in motion; this paper contains results of great value.—A note by M. E. Liais on the spectrum analysis of the zodiacal light and on the corona of eclipses was read. The author states that he has found that the spectrum of the zodiacal light is continuous, and calls attention to his previous observations on the solar corona, the nature of which he claims to have established in 1858.—MM. Becquerel presented a note on the temperature of soil observed at the Jardin des Plantes, at the Observatory, and at Montsouris during December 1871 at 10 centimetres below the surface.—M. I. Pierre read a note on the simultaneous distillation of water and iodide of butyle, in which he stated that iodide of butyle boils under water at 204·8° F., rising through the water in drops with a bubble of vapour attached to each, and that during this ebullition the two liquids pass over in the proportion of 21 water to 79 iodide. Iodide of ethyle behaves similarly.—M. H. Sainte-Claire Deville presented a report on a memoir by M. Grüner on the action of oxide of carbon upon iron and its oxides.—A note by M. A. Rosenstichl, on a method of separating the two isomeric toluidines, was read.—M. P. Thenard presented a note by M. A. Houzeau on the preparation of ozone in a concentrated state.—The discussion on the subject of heterogenesis, commenced at the last meeting, was continued in two notes by MM. Balard and Fremy, and in a paper by M. Pasteur on the nature and origin of ferments.—M. J. de Seynes also presented a note in reply to a passage in M. Trécul's memoir.—M. Monnier read a paper on the functions of the respiratory organs in aquatic larvæ.—M. C. Bernard presented a memoir by MM. A. Estor and C. Saint-Pierre on the analysis of the gases of the blood; and M. Brongniart communicated a note by M. de Saporta on the fossil plants of the Jurassic epoch.

VIENNA

I. R. Geological Institution, January 16.—M. von Hauer presented the third number of the "Memoirs of the Geological Institution," containing a monograph of the Echinoderms of the more recent tertiary deposits of the Austro-Hungarian empire, by Dr. G. Laube.—M. G. Tschermak explained the contents of a memoir sent by Dr. C. W. C. Fuchs, from Heidelberg, for the "Mineralogische Mittheilungen." The author details the chemical processes which take place in lavas at the moment of the eruption, and by the observation of broken crystals in the lava, concludes that the melted masses, some time before the eruption, must have had a higher temperature than in the moment of eruption.—M. Th. Fuchs demonstrated some detailed sections of the upper tertiary strata in the neighbourhood of Vienna. They seem to prove that the marine sands appear in some localities below, in others above, the Leitha limestone.—M. Ch. Paul, on the upper tertiary strata of Sclavonia. They are divided into three different members, corresponding to the three great divisions of the strata of the Vienna basin. The lowest division, the marine beds, consists chiefly of calcareous strata, the Leithakalk. The middle division, the sarmatic beds, is formed of a large mass of sandstones which are overlain by white sands of fresh water origin. The congerian beds, finally, are separated into two members—the lower containing large layers of lignite, and characterised by *Unio maximus*, *Paludina Sadleri*, and other species of this genus with smooth shells; and the upper, without lignites and containing an entirely different fauna, also with many species of *Paludina* with ribbed and ornamented shells.—Fr. von Hauer, on new geological discoveries in Eastern Transylvania, made by F. Herbich. Between Barsyek, on the Moldavian frontier, and the region south of Kronstadt, a large range of mountains consisting chiefly of calcareous strata is developed, which had formerly been regarded as belonging almost entirely to the Jurassic formation. The recent investigations of Mr. Herbich, on the contrary, show that here are developed almost all the particular types of Alpine formations of mesozoic ages. The Trias is represented by the Wurfenslater and Guttenstein limestone, which are overlain by red Hallstatt marbles, with *Ammonites Metternichii*, &c.; the Lias by the Grosten and Adneth strata, &c. It is very remarkable that some of these strata—for instance, the Hallstatt marbles—are entirely wanting in the whole range of the Northern Carpathians, which connect the Transylvanian mountains with the Eastern Alps.

BOOKS RECEIVED

ENGLISH.—Zanzibar; City, Island, and Coast: Capt. R. F. Burton. 2 vols. (Tinsley Brothers).—Queen Charlotte Islands: F. Poole, edited by J. Lyndon (Hurst and Blackett).—Chemical Notes for the Lecture Room, 3rd edition: Thos. Wood (Longmans).—The Differential Calculus: F. Wilson (Longmans).—The Pipits: by the Author of Caw-Caw (Glasgow, J. Maclehose).

FOREIGN.—(Through Williams and Norgate.)—Die Krankheiten des Linsensystems: Dr. Max Salomon.—Lehrbuch der anorganischen Chemie, 2te Abtheilung: Dr. Ph. Th. Büchner.—Jahresbericht über die Fortschritte der Chemie für 1869, Heft 2: Ad. Stricker.—Zoologische Mittheilungen, Band 1: Dr. L. W. Schaufuss.—Thesaurus Ornithologiæ, Band 1: Dr. C. G. Giebel.—Botanische Untersuchungen, 1: Dr. N. J. C. Müller.—Geschichte der Himmelskunde: Dr. J. H. von Mädler.—Thesaurus Literaturæ Botanicæ. Fas. 1: G. A. Pritzel.—Die Foraminiferen des schweiz. Jura: Dr. J. Kübler.

DIARY

THURSDAY, FEBRUARY 1.

ROYAL SOCIETY, at 8.30.—On the Lunar Variations of Magnetic Declination at Bombay: C. Chambers, F.R.S.—On a Possible Ultra-Solar Spectroscopic Phenomenon: Prof. Piazzi Smyth, F.R.S.—On the Normal Paraffins: C. Schorlemmer, F.R.S.

SOCIETY OF ANTIQUARIES, 8.30.—On a Camp opposite Clifton on Leigh Down, with Remarks on Vitrified Forts: Rev. H. M. Scarth.

CHEMICAL SOCIETY, at 8.—On the Relation between the Atomic Theory and the Condensed Symbolic Expressions of Facts and Changes (Dissected Formulæ): Dr. C. R. A. Wright.

LINNEAN SOCIETY, at 8.—On the Classification and Geographical Distributions of Compositæ: The President.

FRIDAY, FEBRUARY 2.

GEOLOGISTS' ASSOCIATION, at 7.—Special General Meeting.—On the Chloritic Marl Deposits of Cambridge: Rev. T. G. Bonney, F.G.S.

ARCHÆOLOGICAL INSTITUTE, at 8.

ROYAL INSTITUTION, at 9.—On the Identity of Light and Radiant Heat: Prof. Tyndall, F.R.S.

SATURDAY, FEBRUARY 3.

ROYAL INSTITUTION, at 3.—On the Theatre in Shakespeare's Time: Wm. B. Donoe.

MONDAY, FEBRUARY 5.

ROYAL INSTITUTION, at 2.—General Monthly Meeting.

ENTOMOLOGICAL SOCIETY, at 7.

LONDON INSTITUTION, at 4.—Elementary Chemistry: Prof. Odling, F.R.S.

ANTHROPOLOGICAL INSTITUTE, at 8.—Anniversary Meeting.—On Hereditary Transmission: Geo. Harris.—Strictures on Darwinism: H. H. Howorth.—The Wallons: Dr. Charnock and Dr. Carter Blake.

TUESDAY, FEBRUARY 6.

ROYAL INSTITUTION, at 3.—On the Circulatory and Nervous Systems: Dr. W. Rutherford, F.R.S.E.

ZOOLOGICAL SOCIETY, at 9.—Contributions to a General History of the Spongiadæ, Part I: Dr. Bowerbank.—Notes on *Rhinoceros sumatrensis*, with a photograph from life: Dr. John Anderson.

SOCIETY OF BIBLICAL ARCHÆOLOGY, at 8.30.—On an Inscription in Hebrew or Ancient Phœnician Characters, discovered at Siloam, of the Age of the Kings of Juda: Ch. Clermont Ganneau.

WEDNESDAY, FEBRUARY 7.

GEOLOGICAL SOCIETY, at 8.—On the Geology of the Neighbourhood of Malaga: M. D. M. d'Orueta.—On the River-Courses of England and Wales: Prof. A. C. Ramsay, F.R.S.—Migrations of the Graptolites: Dr. H. Alleyne Nicholson, F.R.S.E.

SOCIETY OF ARTS, at 8.—On the Forests of England, their Restoration, and Scientific Management: T. W. Webber.

MICROSCOPICAL SOCIETY, at 8.—Anniversary Meeting.

PHARMACEUTICAL SOCIETY, at 8.

THURSDAY, FEBRUARY 8.

ROYAL INSTITUTION, at 3.—On the Chemistry of Alkalies and Alkali Manufacture: Prof. Odling, F.R.S.

ROYAL SOCIETY, at 8.30.

MATHEMATICAL SOCIETY, at 8.—On the Factors of the Differences of Powers, with especial reference to a theorem of Fermat's: Mr. W. Barrett Davis.—On an Algebraical Form and the Geometry of its dual connection connection with a polygon, plane, or spherical: Mr. T. Cotterill.

SOCIETY OF ANTIQUARIES, at 8.30.

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ERRATA.—P. 243, col. 2, line 6 from top, prefix "vertical" to "band;" line 10, for "table" read "tall."

THURSDAY, FEBRUARY 8, 1872

THE FOUNDATION OF ZOOLOGICAL STATIONS

WHOEVER contemplates a little closely the state of Science at the present time, must be struck with the fact that, whilst in almost every other branch of public and private life co-operation has been established, and has worked out great results, its influence on the life of Science is but small and insignificant.

This may sound strange to all those who know the number of Scientific Societies, Academies, and Unions to be found in England, Germany, America, Italy, France, in short, everywhere where Science is cultivated at all. But if one looks into the life of these societies, there is not much co-operation to be found in them. They publish periodicals ; but there are publishers who do quite as well as societies, and sometimes even better. They meet and talk science ; but this does does not add much to the real progress of science. Sometimes they found museums or cabinets, and this is a better service ; they establish a library for the use of their members, and this is perhaps the best they do altogether. A man may be fellow of twenty different societies, but that will not affect much the progress of the scientific work he does ; if he is member of certain academies his reputation may be raised in the eyes of the outside public, but no essential help is afforded by that either to him or to his work, except in the case where such academy has some influence on the Government, as, for instance, the Royal Society. The Menagerie in the Regent's Park, established by the Zoological Society, is one of the solitary instances in which, the initiative being taken by a scientific body, an institution has been evolved, drawing immense revenue from the public pocket, which is for the most part spent upon scientific objects. It is the application of this method of securing support which will be strongly advocated in the present paper, as a practicable path for the future progress of biological research.

There is also another great society in Britain which does, perhaps, better work for science than any other. This society is the British Association for the Advancement of Science. Not only does its great and well-deserved reputation make it powerful and influential, but also the large sum of money it distributes annually for the direct progress of science. This influence is due principally to the fact that the best men in British Science participate with great eagerness in the meetings of the Association and lend to it all their personal authority and reputation. The considerable sum of money to be distributed is due to the great number of scientific and lay people that take part in its meetings.

The combination of these two elements ought to be imitated in every special branch of science. The times are past when great scientific men did not condescend to speak to a general public, and happily nobody believes any longer that science must be lowered and lost, because the general public looks at and hears a little of its inner life. Great scientific men have an immense influence upon the public, and that is an immense benefit to the public ; on the other hand, the general public takes interest in, and pays money for the progress of science, and that is a great benefit for science.

The meetings of the British Association therefore are an essential step in the right direction for lending science the great help of co-operation. But a great deal more of it is needed if that element is to supersede by-and-by the old lines and ways of mere individual and disorganised action. Especially is co-operation wanted in the single sciences. Every one knows how great is the progress in meteorology and astronomy brought about by the possession of special laboratories and observatories. Even if all the universities were extinct at once, these sciences would go on perfectly well by the help of the observatories. Chemistry is aided by innumerable laboratories, erected for practical purposes. Mechanics governs the world and finds itself at home everywhere, involving by its special character many elements of co-operation.

Other sciences do not enjoy these privileges, though they want them perhaps even more than some of those that are in possession of them. Amongst the number of these sciences, perhaps the most neglected in the way of co-operation is Biology, that science which occupies at present such an eminent place in the public interest, and yet the most neglected, in so far as no other science feels at present the necessity of co-operation and organisation so much as biology. The reason is a very obvious one. Biology has undergone a complete revolution by Mr. Darwin's great work. This revolution has augmented the number of special problems in such enormous proportions that biology is now completely at a loss to solve all these problems by the aid of the means placed hitherto at its disposal, and looks pretty much like a boy who has suddenly grown in one year out of all his clothes, presenting the ridiculous aspect of a man in a child's dress. The thing which a father would do for his boy would be to go and buy another dress. This obviously was also the idea of Prof. Carl Vogt, who long since began an agitation for the establishment of a zoological laboratory at the sea-coast, of which agitation he wrote me in a letter the following account:—

"During the years 1844—1847 the plan for the establishment of an expedition was worked out at Paris by Milne-Edwards, and I participated in it. The object was the investigation of a coral-island, and the establishment of a station upon it for at least several years. The ship and the station should be furnished with all possible things, especially for dredging-work. The scheme fell to pieces owing to a question of etiquette. The commander of a man-of-war of the Royal Navy would not submit to the direction of a naturalist.

"As you know, I lived from 1850 to 1852 at Nice. The instruments for observation, which I bought by the money earned by literary work, consisted of a microscope, a surface net, and some large sugar-bottles. I tried at the time by the help of two deputies, my friends Valerio and Dunico, to bring about the foundation of a zoological station at Villafranca, asking only for some rooms in the empty buildings of the Darsena, and the establishment of some tanks in them. Nevertheless I had not the least success.

"In the year 1863 my friend Matteucci became Minister of Public Instruction in the kingdom of Italy. With him, as a physicist who especially dealt with physiological subjects, and who, understanding the necessities and wants of physical science, intended to make important reforms, I easily arrived at a mutual understanding. It was his idea to elevate the studies in Italy by introducing foreign, especially German, scientific men into the chairs at the universities, who should teach the new generation of Italian students. I worked out for him a project for the erection of a zoological station at Naples, the most suitable place in Italy. The Casino Reale at Chiatamone was to be transformed and fitted up for such a purpose, and a little steam yacht for dredging was to be placed at the disposal of the station. The latter was in the meantime intended for a sort of school, connected with the whole system of public instruction, to form teachers of natural history for the whole kingdom. The plan was completely worked out and adopted by Matteucci and several others among the first scientific men of Italy. They applauded it heartily; Filippo de Filippi especially did everything he could to bring it into play, and talked about it, as he told me, to King Victor Emmanuel during a hunting-party. Matteucci afterwards left the ministry—Filippi and he are dead—the fate of the project is easily to be understood.

"Thus I had got round the Mediterranean. In January 1871 I was at Trieste delivering public lectures. On January 8 I published in the *New Free Press* two letters on 'Some Necessities and Wants of Scientific Investigation,' the subject of which presented itself to my mind when viewing some of the Austrian arrangements for public instruction. I may be allowed to say that my article met with universal approval; and some Triestian friends, amongst whom I may mention especially Field-Marshal Lieutenant v. Möring, at the time Governor of the Coast District, talked with me on it, and agreed that Trieste would be a very good place for the execution of my project. Möring himself directed my attention to some small buildings at Miramare, lying outside the park; we visited them together and talked about the necessary arrangements to be made. I worked out a fresh project, made rather special calculations on the money necessary for executing it, and sent all this to the Austrian Minister of Public Instruction, Herr v. Stremayr, with whom I spoke on the subject afterwards, when I passed through Vienna. As you know, I addressed at the same time Gegenbaur, Haeckel, and you, to approve my views and assist me. You sent me besides a letter from Darwin, who applauded much your own plan for erecting a station, and had even offered a subscription for it. I added all these letters to my memorial, which unfortunately had the same fate as the Italian: Stremayer left the ministry before he could do the least thing for the realisation of a plan which he thought exceedingly valuable."

Though Prof. Vogt did not succeed in carrying out his plan, there can be no doubt that his idea is the very one wanted for the present state of biology. A great number of other zoologists entertained it, but nobody knew how to execute it.

In the winter of 1868-69 I found myself at Messina, occupied with the investigation of the embryology of Crustacea. Together with my friend Miclucho Maclay I often spoke of the necessity of establishing a zoological station on the coast of the Mediterranean, and we agreed to leave a considerable quantity of instruments, amongst which was a small aquarium furnishing a constant stream of water, to our successors in Messina. An Austrian squadron, just sailing round the globe with a considerable number of naturalists, amongst whom were Herr v. Scherger and others, stopped several days in the harbour of Messina, and caused me many thoughts about the great advantage such and other expeditions would derive from a net of scientific stations stretched over the whole globe.

But how to get anything like such stations built and kept up for years? I did not know at that time that Prof. Vogt had already tried to get assistance from several great governments, and had failed to succeed. But I did not even try to do anything like this, knowing beforehand that it would be useless. Zoology is at present in a rising condition, it has still to conquer the place it ought to occupy in the attention of the public by making itself indispensable to intellectual progress. As it is, governments will not easily be induced to sacrifice much money for the progress of this science.

I took another line. After some unsuccessful attempts to get money by collecting small sums, I combined the idea of founding a scientific station with the plan of building a great public aquarium at Naples. My calculation was, that by the entrance-fee of that aquarium the sums necessary for keeping up the station could easily be obtained, and that perhaps more than that would come out of it. I saw at a certain distance even the possibility of erecting other stations with the surplus of the Naples income, and of giving in such a way quite a new development to biological science, just that development which biology wanted after the great event of the Darwinian theory.

As soon as I had got a hold at Naples, I began to spread my ideas in letters and conversations. I [illegible] the pleasure of finding almost everybody in England and Germany quite ready to assist as much as possible. I brought the subject before the meeting of the British Association in Liverpool, and succeeded so far that a committee was appointed by Section D, composed of Prof. Rolleston, Dr. Sclater, and myself as secretary, under the name of "The Committee for the Foundation of Zoological Stations in Different Parts of the Globe."

This was during the war between Germany and France. While it lasted it was almost impossible to do anything in favour of the scheme I had got into my head, except thinking and meditating upon it as much as possible. But as soon as peace was made I proceeded again, as well with the negotiations at Naples as with agitation in other countries.

As secretary of the above-named committee, I gave a report to the meeting of the British Association at Edinburgh. I stated in that report that the establishment at Naples was now quite safe, so far as the permission of the Town Council was concerned; and that, in all probability, the station would be seen there in working order in January 1873. I added that I had got the assistance of my own Government, and I may add here that the Italian Government also assists me greatly. I proposed further in my report that the British Association might consider the opportunity given by the cessation of the annual grant to the Kew Obervatory, of building a zoological station at one of the most favourable places on the British coast. My idea in proposing this was based on the same considerations which had made me go to Naples. I thought it very convenient and very practicable to build a small station, for example, at Torquay or Plymouth, and to combine in such a station, in the same way as at

Naples, a laboratory with a larger aquarium for the public. The income of the latter in a place like Torquay, where there are so many residents and visitors at all times of the year, would completely suffice to keep up the laboratory, and pay a modest sum to a naturalist, who would be charged with the management of the station. Being unable to attend personally the meeting at Edinburgh, I could not give all the reasons which induced me to make this proposition. All the more I shall avail myself of the present opportunity to do so.

The present state of zoology requires, as stated above, new means of investigation. Systematism and simple faunistic researches fall very far short of the problems now ripe for solution. Two great departments of biological science go much ahead of all others, and these two are embryology and the study of the life of animals in relation to all those conditions which regard the struggle for existence and the action of natural selection.

If we speak first of the latter chapter, it is clear that past times have done much more in promoting knowledge about it than the present generation. It is rather out of fashion to study the habits and conditions of life of an animal. Systematism, the making of genera and species, have so much exceeded their legitimate grounds, that they have almost completely suppressed that other branch of natural history. We owe it to Mr. Darwin that he completely upset this one-sidedness, in proving, by his admirable treatises on the Domestication of Animals and Plants, on Sexual Selection, on the Fertilisation of Orchideæ by the Interference of Insects, of what fundamental importance these studies of the habits and conditions of animal life can be. He added not only an enormous number of hitherto unknown facts to the storehouse of science, but he showed what immense importance these facts gained by deriving from them the great principle of natural selection—a principle as grand as any in modern science. Very few zoologists (in naming Mr. Wallace and Mr. Bates, I do justice to these eminent men as two of those who promoted these studies independently of Mr. Darwin) have followed Mr. Darwin's lines in these departments. Nevertheless this must happen: it constitutes one of the most urgent necessities of biological study in our time, and it must not only be done for our domestic animals, and those that live most closely around us, but wherever animals are to be found, and so above all in that enormous field of animal life which occurs in the sea.

Every one will agree with me that we know scarcely any of the secrets of the life of the sea bottom. We have short notices on the habits of some fishes; but this is altogether insignificant compared with the immense bulk of things unknown to us in the same department. And of echinoderms, cuttle fish, jelly fish, polyps, &c., &c., our knowledge simply amounts to nothing.

Here an aquarium, under scientific guidance and superintendence, can work immense good and progress. And such an aquarium will do double service; first, it will attract the public and yield money; and then it will serve immediately and directly the progress of science, by giving the only possibility of knowing something about the habits and the life of marine animals.

But a zoological station with an aquarium will serve equally as much for the progress of embryology. Whoever looks at the development of biological science must see that, during the last ten years, embryology has made very important progress, not only in accumulating facts, but in rendering them serviceable to the progress of ideas and principles.

An offspring of the theory of descent is the maxim that the ontogenetical development is an abbreviated recapitulation of the phylogenetical development. This maxim, or law, if we choose to call it a law, gives enormous importance to embryology. By the help and application of it we may succeed in getting a deep insight into the history of animal life long before the geological record. The Cambrian and Silurian systems yield us already a fauna of so high perfection, and so complete a series of representatives of almost every great class of animals, that we could easily be led to believe in a waving up and down of animal creation, not in a constant progress, so comparatively small are the differences between the present fauna of the earth, and those which the geological record of all the strata makes known to us. Embryology, on the contrary, starts at the very beginning of organic life, tells us how out of simple organic matter cells became formed, how these cells took different functions, thus differentiating and organising the being that possessed them. Embryology further tells us how out of one form, one single form, whole classes came forth, and renders it possible for us to trace the lines of origin of every member of these classes, down to the common ancestor of all of them.

Systematists, looking out anxiously for the "natural system" of the animal kingdom, and turning to mere anatomical differences, may be compared to Sisyphus rolling his stone. They cannot succeed without taking to embryology. But embryological studies are among the most difficult in the whole range of biological science. Not only the interpretation of the facts, and the conclusions to be drawn from observation, require an immense amount of circumspection, caution, and critical ability; but even the simple statement of a fact, the mere act of observation, is often exceedingly difficult. How many monographs on the embryology of the chicken have been written since Caspar Friedrich Wolff published his immortal book against the doctrines of Haller. Pander, Baer, Remak, His, and many others, have treated the same subject, and still to-day there is uncertainty on the most fundamental questions. This is above all to be attributed to the mechanical difficulties of observation. And these difficulties do not exist only in the case of birds' eggs; they are the same for the eggs of almost all animals, especially for those of marine animals. These require a constant stream of salt water to keep them alive, a stream which is only to be had by the help of an aquarium. It is principally due to the absence of such aquariums that our knowledge of the development of fishes is still so rudimentary; for, though the works of Baer, Rathke, Vogt, Lereboullet, Kupffer, and others have taught us a good deal, nevertheless the essential parts of fish-embryology are still wanting. And this is the more to be regretted as it cannot be doubted that the eggs of fishes are, in many regards, preferable as objects for the investigation of general embryological facts to those of the birds. Considering only the fact that all other vertebrata have proceeded from fishes, most likely from shark-like animals, it will be of the greatest importance to acquire convenient methods for investigating the embryology of these animals.

Besides, the enormous mass of other marine animals waits equally for the establishment of laboratories provided with aquariums, before the study of their embryology can safely, and with due prospect of success, be taken in hand. And that the common ancestors of all the higher animals have lived in the sea, and must have left the traces of their nature still in the embryos of marine animals, is more than likely. Every attempt, therefore, to get back to these ancestors, and to build up scientific genealogy, must lead to the investigation of the embryology of marine animals, must cause, in consequence, the desire of having laboratories near the coast, provided with tanks and continual streams of sea water, to overcome the mere mechanical difficulties of the study.

These are reasons of the most imperious nature to move all those who can do something, to combine their exertions for the foundation of zoological stations near the sea-coast.

When I therefore proposed, in the name of the Committee for the Foundation of Zoological Stations, the erection of such a station at Torquay, my principal object was to create a greater facility for English zoologists to execute scientific works of the above-mentioned nature. Without denying one moment the immense benefit zoology has always derived from English naturalists, one may justly lament that embryology has not found so many students in a country which has such great opportunities of following the study as, for example, has been the case in Germany. England abounds in splendid localities for the study of marine animals; the innumerable harbours, firths, and bays yield an immense material for the scientific observer. Students at the universities would have the easiest access to these localities, and would gain a great mass of information from them; but circumstances have directed almost the whole scientific spirit in another direction—almost all the biologists are occupied with the completion of the faunistic records of the English seas. The existence of a zoological station at Torquay must lead to a greater cultivation of the other branches of marine zoology by Englishmen, and must open also for foreign zoologists the opportunities yielded by the fauna of the south coast of England for carrying in studies in comparative anatomy and embryology.

It will be essential, not only for the progress of zoology in general, but also for the development of the whole scheme for the foundation of zoological stations, that those countries which contribute by their natural position most to the progress of marine zoology should be provided first with zoological stations. If zoological stations in other parts of the world outside Europe are to be founded, they will require above all zoologists to conduct them. Where are these at present to be found? Nowhere, I believe. If, therefore, the great object of my plan is to be attained, it will only be by gradually and consistently developing its base—the foundation of stations in Italy, Britain, France, Norway, and perhaps Spain or Portugal. With the help of these stations, zoologists may be educated who would be inclined to go to remoter places, such as, for instance, Capetown, Ceylon, Japan, or Australia, and conduct or work only for a couple or more years in the stations built in those countries. There can be no doubt that the benefit for science would be enormous if there existed efficient working stations in these countries; but to make them efficient the principal means is to give them well-instructed naturalists at their head, and this is at present not possible.

Therefore I take the opportunity of repeating once more that it seems to be essential to proceed with the foundation of a zoological station at Torquay, and to head that station by a young, laborious zoologist, who is already experienced in histological and embryological work. It cannot but be that science, and especially British science, will derive considerable benefit from such a proceeding.

Naples, Jan. 2 ANTON DOHRN

THE NATURAL HISTORY OF EGYPT AND MALTA

Notes of a Naturalist in the Nile Valley and Malta. By Andrew Leith Adams, M.B. (Edinburgh: Edmonston and Douglas, 1871.)

FEW men have better opportunities for furnishing valuable contributions to the Natural History of foreign parts than surgeons attached to the Army and Navy; an education in at least the rudiments of natural science, combined with abundant leisure, presenting means which are not at the disposal of all travellers. As a rule, we fear that this class of men have done but little for Science compared with what might have been expected of them. There are, however, some honourable exceptions, among them our present author, whose "Wanderings of a Naturalist in India" has been already given to the public, and who now publishes the results of the labours of his leisure hours and vacation rambles in the investigation of the archæology and natural history of the Lower Nile and Malta.

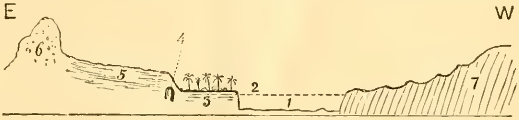
The most interesting portion of Dr. Adams's researches in Egypt and Nubia relates to the evidence as to the period when the northern portion of the African Continent became elevated above the sea. On this point he says:—

"The discovery of the common cockle and other marine shells far inland, and over vast tracts of Algeria and the desert of Sahara, even up to height of more than 900 ft. above the present level of the Mediterranean, and at a depth of 300 ft. below it, fully establishes the fact that a large portion of North Africa was, at no very distant period, covered by the ocean; moreover, that the highlands of Algeria, Tunis, Morocco, and Barbary, were at this period separated from Africa by sea, and that the submergence occurred during the modern or post-tertiary period. Further researches have also proved that the same description of phenomena are to be observed along the borders of the Red Sea. A question therefore suggested itself to me in 1863, whether or not Egypt and Nubia had participated in the same continental movements. Accordingly, no opportunities were omitted during our short sojourn in Lower Egypt in searching for similar evidences of upheaval and depression, but, owing to the flatness of the country, drifting of the desert sands, and great expanse of cultivation on the river's banks, and our rapid movements, I was unable to discover any traces. It was not until we approached the frontier of Nubia, and passed the first cataract, that favourable opportunities were presented. The Nile, now contracted by the porphyritic and sandstone rocks, flows between steep banks, and creating accumulations of alluvium and bendings and openings in its course, the desert may be said to come down to its margin.

"Wherever these Nile deposits exist, there may be seen clusters of date and doom palms, and fields, whilst further back stand the mud-built villages of the natives; and still more inland are observed plateaus and terraces at variable levels, covered with finely rounded angular stones and drifted sand. These terrace cliffs continue, with broken intervals, from below the first cataract up to the extreme point attained by us at the top of the second cataract. The observer may have some difficulty at first in tracing these river terraces, but, after a little experience, there will be no trouble in making them out. Let him proceed from the river (1) across the alluvial plain (3), on which stands Der, the capital of Nubia, to the ruined temple (4) of Rameses the Great, on the verge of the cultivated tract, then mount the plateau immediately above (5), and wander inland until he gains a height of 130 ft. above the highest mark of the inundation (2), and commence digging among the stones, when he will come to a reddish-brown soil, highly impregnated with natron, which the natives collect for top-dressing on their fields below. There he will find abundance of Nile shells distributed throughout the soil from the margin of the cliff above the temple inland for upwards of a mile, and until the drifted sand of the desert makes it difficult or impossible to trace them further; indeed, the same appearances are observable along the right bank of the river throughout the distance just indicated. These fossil fluviatile shells belong to species nearly all of which have been proved to exist in the Nile at the present day, and comprise the following species, determined from specimens sent to the Geological Society of London, and examined by the late eminent conchologist Mr. S. P. Woodward:—*Unio lithophagus* (?) *Bulimus pullus*, *Paludina bulimoides*, *Aetheria semilunata*, *Cyrena fluminalis* and variety *trigona*, *Iridina nilotica*."

From these data Dr. Adams concludes that Egypt and Nubia participated in like movements with other portions of the Continent to the east and west; but whether or not, in common with them, they were entirely submerged under the sea at the same epoch, is not so clear, as no marine shells have yet turned up in either Egypt or Nubia.

At Malta, the author's researches were chiefly devoted to an investigation of the fossil mammalian remains in which this group of islands is so rich, for which purpose the British Association, at the suggestion of Dr. Falconer, Mr. Busk, and Captain Spratt, voted 60*l.* in 1863 in aid of his explorations. These Maltese mammalian remains are of unusual interest, comprising the *Hippopotamus Pentlandi*, an animal about as large as the existing Nile species; the *Elephas melitensis* of Falconer, or Pigmy



Maltese Elephant, not more than $4\frac{1}{2}$ ft. in height; the still smaller *Elephas Falconeri* of Busk, the average height of which at the withers could not have exceeded $2\frac{1}{2}$ to 3 ft.; a new large species, named by Dr. Adams, from the place of its discovery, *Elephas Mnaidræ*, the Gigantic Fossil Dormouse, *Myoxus melitensis*, described by Dr. Falconer to be "as big in comparison to the living dormouse as the bandicoot rat to a mouse," and the Hollow-jawed Dormouse, *Myoxus Cartei*, another new species detected by the author. Conspicuous among other vertebrate remains are those of the Gigantic Swan, *Cygnus Falconeri*, another large swan, several other species of land and water birds, at least two species of fresh-water turtles, and a lizard.

With regard to the recent Molluscan fauna, a small land-snail belonging to the genus *Helix* has been found near St. Paul's Bay, and on the bare limestone cliffs of the west highlands of Gozo two recent shells of the genus *Clausilia*, not apparently found in the adjoining continent or Sicily; and at present these represent the only living animals that can be said to be peculiar to the Maltese Islands. Lists of the fishes and birds of Malta are given at the end, the majority of the latter being birds of passage, with respect to the habits of which some interesting particulars are given.

We have no information with regard to the vegetable productions of the island, and this is to be regretted, as observations made during so long a residence would doubtless have elicited some new and interesting facts. The author, however, has probably acted wisely in not trusting to second-hand information which his own botanical knowledge would not have enabled him to verify. We find the well-known *Cynomorium* of Gozo, the *Fungus melitensis* of the Knights of St. John, a flowering plant, spoken of as a lichen.

The volume is illustrated with some well-executed woodcuts and lithographic plates, and we recommend it to all interested in the subject.

OUR BOOK SHELF

A Synonymic Catalogue of Diurnal Lepidoptera. By W. F. Kirby. (London: Van Voorst, 1871.)

THE great work on the "Genera of Diurnal Lepidoptera," by Doubleday and Hewitson, completed after the lamented death of the former by the assistance of Prof. Westwood, included under each genus a synonymic list of all the described species which the authors were able to determine. But more than twenty years have elapsed since the completion of this most valuable work, which still remains without a competitor either in this country or on the Continent, and thus our means of reference upon systematic matters connected with the beautiful and interesting group of butterflies generally, have remained at what must be regarded nowadays as a somewhat antiquated standpoint, whilst the business of describing has been carried on with the most astonishing energy. In Britain Doubleday's *collaborateur* Hewitson, and his successor Butler, have described an almost inconceivable multitude of new species, and a considerable number have also been added to the list by Bates and Wallace; whilst on the Continent the Brothers Felder and Dr. Herrich-Schäffer have been equally active. New views have also been put forward as to the natural sequence and limitation of the groups (families and subfamilies) into which the great Rhopalocerous tribe is divided, and the whole face of this department of entomological science has undergone a wonderful change in the last twenty years.

Under these circumstances many an entomologist has no doubt often wished that a new "Genera of Diurnal Lepidoptera" would make its appearance; but such works are not to be lightly undertaken, and it may be long before we can hope to see a good, general, systematic treatise upon this group of insects. In the meanwhile we welcome Mr. Kirby's catalogue as a most important aid to the study of the Diurnal Lepidoptera. It is a complete catalogue of the described species of the group, amounting, as an estimate, to about 9,600 in number, and gives the synonyms both of the genera and species in a clear and easily-intelligible form. With the assistance thus offered to him by Mr. Kirby, the entomologist may easily ascertain what has been done by former writers in this department of his science, and it will be his own fault if he does not keep himself *au courant* with its future progress.

Criticism upon a work of this nature would be out of place here, and we can only cordially recommend the results of Mr. Kirby's most conscientious labours to the attention of all entomologists.

A Class-book of Inorganic Chemistry, with Tables of Chemical Analysis, and Directions for their Use. By D. Morris, B.A. (London: G. Phillip and Son, pp. 157.)

THIS work has been compiled for the use of students preparing for the Oxford and Cambridge Middle Class Examinations, and the Matriculation Examination of the University of London; it lays claim to no originality of treatment, and professes to be simply a collection of "enlarged notes," . . . "originally culled from the best modern books." Under these circumstances we are somewhat surprised that the author should have ventured to publish it; we are quite unable to detect any special merit in the book, and it is disfigured by many passages which show great want of exactness. Thus, we find "nitric acid, or nitric anhydride, N_2O_5;" "sulphate of potassium or dipotassic sulphate;" the formula of phosphate of calcium is written $3Ca2PO_4$, of chloride of lime $CaOCl_2O$. We are told that "ammonium and sodium are distinguished by the smell of ammonia on the addition of caustic potash." "Pure water has no action upon the metal (lead), but water charged with air corrodes it, and the oxide of lead thus formed dissolves in the water." Among the redeeming qualities of the book may be mentioned the questions which are selected from various University examination papers, and the examples given worked out in the text; but with errors of the nature of those given above it is impossible to recommend the book to the student, or to regard it as a reliable source of information.

The Elements of Plane Geometry for the Use of Schools and Colleges. By Richard P. Wright, Teacher of Mathematics in University College School, London, formerly of Queenwood College, Hampshire. With a Preface by T. Archer Hirst, F.R.S., &c., late Professor of Mathematics in University College, London. Second Edition. (Longmans, 1871.)

THIS work would have been more correctly described as being "by Eugène Rouché and Ch. de Comberousse, translated and edited by Richard P. Wright," &c. But although Mr Wright can lay small claim to originality, he has shown judgment in the selection of an eminently logical and masterly treatise on geometry, and he has rendered it into clear and forcible English. The arrangement is excellent, and many of the conclusions for which Euclid found it necessary to reason geometrically on each particular case are treated generally by purely logical considerations. Many of the demonstrations, notably that of the *pons asinorum*, are far more simple and convincing than those in Euclid. The difficulty of the twelfth axiom is met by the easy axiom that *through a point without a line only one parallel can be drawn to that line.* In some points there seems to be an unnecessary alteration of the language of Euclid, as in the definition of a figure, "Surfaces and Lines or combinations of them." This definition seems to have been introduced to enable the authors to describe a *locus* as a figure; but it having been pointed out that a locus is not a figure, Mr. Wright has described it as a *line*, but has not restored the word figure to its ordinary acceptation. At the same time it is not quite correct to define a locus as *a line*, excluding such loci as a pair of parallel lines, the circumference of a circle with its centre, &c. Again, the word *circumference* is substituted for the word *circle* whenever the circumference only is intended. It is true that the word *circle* in Euclid is used in two different senses, but this leads to no ambiguity of ideas; while the use of the word *circumference* for *the circumference of a circle* only excludes its application to an ellipse or other closed curve. The word *angle* is not defined when first introduced, but we are told afterwards that it "may be regarded as the quantity of turning of a definite character around the vertex, which a movable line must receive in passing from the direction of one side to that of the other.' We fail to see the force of the words "of a definite character," and would suggest the following definition: "When a straight line moves about a fixed point in itself so as to occupy a new position, the quantity of turning it has undergone is called the angle between the two positions." The exercises are ingenious and instructive, but those of the earlier chapters are much too difficult for mere beginners. The treatment of proportion is good, and the work as a whole is an admirable introduction to the higher mathematics, and a great help to independent investigation. We especially recommend it to students who have found themselves discouraged by the cumbrous form and initial difficulties of Euclid. The second edition contains the alterations suggested by a late eminent mathematician in the *Athenæum* on the appearance of the first edition, with the addition of the substance of the second book of Euclid, and in a few cases the demonstrations of Euclid have been restored. H. A. N.

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 Aurora Borealis of Feb. 4th

AFTER a rather long absence of auroral displays, a brilliant and many coloured example was seen here last night, February 4, not quite so vivid as that of October 1870, but coming next to it so far as my own experience goes.

At about 8 P.M., when the maximum development was reached, all the heavens were more or less covered with pink ascending streamers, except towards the north, which was characteristically dark and grey, first by means of a long low arch of blackness transparent to large stars, and then by the streamers which shot up from that and along its whole length, for they were green or grey only for several degrees of their height, and only became pink as they neared the zenith, the region where the more precise phenomena occurred, as thus:—

1. The focus of the vertical streamers coming up from all azimuths was very constant among the stars, but was not in the zenith itself, being nearly 18° south and 5° east thereof.

2. The red streamers varied from orange to rose-pink, red-rose, and damask-rose, or from strontium α, through calcium α, lithium α, and on to and beyond potassium α, that is, they did so to the naked eye, but the spectroscope knew no variety of reds amongst them; and I, having a very good referring spectrum in the lower part of the field of view, giving potassium α, lithium α, sodium α, citron acetylene and green acetylene, be-

sides the blue and violet, saw Angström's green aurora' line perpetually over citron acetylene at W.L. 5579, and the red aurora line between sodium α and lithium α, but nearer to the latter, say at W.L. 6350.

3. Now, W.L. 6350 in the solar spectrum is a pretty bright scarlet red, so that orange could easily be made of it by the green aurora mixing therewith, and the spectroscope separates each of the two kinds of light with perfect ease But how came potassium red or W.L. 7700, *i.e.*, the blood red, lurid red, and tragedy red of painters to appear so markedly to the naked eye, and yet not be seen at all in the spectroscope, either as a new ingredient or an altered place of the red line? It would apparently be by the mixing up of rays and streamers of the *blackness* out of that long, low dark arch on the northern horizon. But when a spectroscope fails (as fail it must) to show a characteristic line for a region of blackness, what other instrument can we take to prove the case?

Excessively faint greenish and bluish lines appeared at wave lengths 5300, 5100 and 4000 nearly; but the main light in the spectroscope was to the extent of 8-tenths of the whole, that of the green line 5579, and of 1·7-tenths the red line 6350; while to the naked eye the splendour of the display and its variety consisted in triple mixtures of 5579, 6350, and the unknown dark medium. Could something be ascertained about that, if those who have good telescopic star spectroscopes were to observe a star when shining through one of these inky black arches?

At 9.30 P.M. when all the aurora had faded or passed away towards the south, where a few straggling pink patches still appeared, the northern horizon and its sky being now free from the black arch, as well as the green streamers, perfectly astonished me by the clear pellucid blue of a true starlight night sky in a bright climate and clear atmosphere. Evidently the dark arch and streamers are as much a part of the aurora as the green and red lights, but how to investigate them—that is the question.

C. PIAZZI SMYTH

15, Royal Terrace, Edinburgh, Feb. 5

LAST evening an aurora of rather unusual brilliancy was seen here. I happened to be out with a friend in the country about sunset, when the sky was completely overcast and fine rain was falling. We noticed that darkness did not come on so quick as usual, and at 7 o'clock it was so light as to lead my friend to believe that the moon was shining above the clouds. Later in the evening slight breaks began to appear in the clouds, through which the first magnitude stars were just visible, and through these openings an intense red illumination appeared. The spectroscope gave from every part of the heavens a very bright line in the green, and another fainter one nearer the blue, together with a diffused light over the green and blue parts of the spectrum. The brightest part of the aurora was towards the S.W. From the large amount of light, although it was raining at the time, it must have been one of the brightest auroras that have been witnessed for years.

G. M. SEABROKE

Rugby, Feb. 5

COMING up the Channel on Sunday night last in the P. and O. screw-steamer *Delta*, about 9.40 P.M., I saw a very fine aurora. The sky was cloudy, which somewhat dimmed its brightness, but it was rather brilliant towards the N.

Having a Hoffman's direct vision spectroscope with me, I turned it towards the brightest red portion which lay towards the N.E., and with a moderate slit got a very sharp and distinct line in the green at or near the position of F in the solar spectrum. No other lines were visible. But on removing the telescope, and observing the spectrum with the naked eye, a fine crimson line revealed itself near C; the colour of it was exactly that of hydrogen α, as seen in a vacuum tube.

I also thought that there were faint traces of structure visible in the blue and violet, but of this I cannot be sure.

There had been traces of auroral phenomena visible early in the same evening. The green line was so distinct that unpractised observers saw it easily. The red line, however, was much fainter, and appeared to flicker.

I much regret that I had no means of recording the position of the lines.

R. J. FRISWELL

ABOUT six o'clock on Sunday evening the ruddy appearance of the upper clouds gave warning of an aurora in prospect, but I was not prepared for the magnificent sight which appeared on looking out an hour later. The higher part of the sky seemed covered with bright rose-coloured clouds, which, from the dark masses of clouds passing underneath, seemed continually to be shifting in position. Intervals of deep green appeared amongst the red, and these, when looked at with a spectroscope, gave a stronger light than their surroundings. Objects near were illuminated as if the moon had risen behind the clouds. I had a miniature spectroscope of Browning's, with which I examined the brightest parts, and obtained four lines—one very bright green, two very faint nebulous green bands, and one red line. Having a spirit lamp handy, in which were remnants of sodium, lithium, and sulphate of copper, I was able roughly to estimate the positions of the lines. The red was about a third from *D* towards the lithium line; the very bright green about a third from *D* to the copper line near *b*, the other faint green bands were more refrangible, and I should think their places were between *b* and *F*, and near *F*, but I could not get their positions so well as the other two; certainly the most refrangible was not so far as the violet-potassium line which I could see in the field.

The light green was present everywhere, the red only showed occasionally with very varying intensity, and the most refrangible green line was also continually varying, but it was brighter than the second green line.

The light around attained its maximum about a quarter to eight, and then very slowly diminished to about midnight, when it had nearly disappeared. A light drizzling rain was falling the whole time.

J. P. MACLEAR

Shanklin, Feb. 5

THERE has been a magnificent red aurora here this evening. I saw it first before twilight had quite disappeared, and at first thought it was the crimson of sunset unusually late. It was at its finest between six and seven; at that time there were columns of light shooting up from the horizon almost to the zenith, and occupying almost half the horizon from the E. of N. round by E. The crimson colour was variegated with bluish white in a way that I have not seen before The barometer was at about 29·45 inches, with a strong breeze from the south.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, Feb. 4

THERE was a fine display of aurora here yesterday evening. I first observed it about 5 30, just in the twilight, but it was then confused with the rays of the setting sun; as the darkness deepened the aurora came out alone, and was then extremely beautiful. It extended from the extreme N.E. to the extreme N.W., but from the reflection of the numerous clouds, appeared to have a much larger area. It was of a bright crimson colour, with the rays golden or orange, of which, however, only a very few were visible.

As the evening came on, about 8 0, the clouds gradually became thicker, and at last almost entirely covered the sky; the only effect then apparent was a deep red glow, which continued with unequal intensity until 11 45, and with all probability much later. At 9.35 there was a break in the clouds towards the E., when the aurora shone forth in all its splendour. The aurora was most certainly visible in daylight, just appearing as the twilight came on.

I have no doubt if the atmosphere had been clearer, we should have had a most magnificent display; as it was, the effect was really beautiful.

J. S. H.

Gloucester, Feb. 5

THERE has been a magnificent and extensive auroral display this evening, of which I beg to send you the following account.

After a very heavy fall of rain, which lasted in this part of the country from 1 o'clock P.M. until 5 30 o'clock, there were collected in the northern horizon numerous *cirro-stratus* clouds, which gradually at first, and afterwards rapidly, moved towards the E., with the strata to the S. As these were passing away, I saw, about midway between these clouds and the zenith a bright patch of pale red light, which became well defined by 6 o'clock. A few minutes after this appeared I saw in the N.W. another patch of red light, and by 6.15 there stretched from N., N.W., and N.E. three very broad streamers converging in the zenith, and forming a splendid crimson canopy, the streamers being quite separated, until meeting, by dark spaces. These slowly disappeared, and of a sudden there appeared a bluish-white streamer stretching N.E. to and passing the zenith by about 10°.

At this time I could see that the Pleiades were partly covered, although not hidden, by a part of this streamer. At 6.35 it faded away. At 6.40 light clouds began to rise in the W. and S.W., and as I recognised this phenomenon as auroral, having seen similar clouds on other occasions of auroral displays, I carefully watched them, and saw at 6.50, in the S.W., a crimson-coloured patch, undefined in shape, originating from the light clouds. At 6.55 there shot up from the S. beautiful red, crimson, and blue streamers, which converged in the zenith. At 6.58 other bands of crimson and blue arose due S., and joined the others in the zenith. At 7.0 I was quite astonished to see the aurora appear in the S.S.E., by which time the previous brilliant display in the S. had dimmed, and the whole of them formed a southern canopy. During this southern display, the northern parts were quite dark, with heavy looking clouds; but at 7.5 the clouds slightly broke up, and I saw a faint redness in the N.E., about 45° above the horizon. By this time the southern streamers and patches began to spread and assume a mottled appearance, which reached by 7.10 the N.W. At 7.15 the N.W. and E. were quite dark and cloudy, and there remained only slight traces of the aurora in the S.W. high up in the heavens, and by 8.35 it had entirely disappeared. JOHN JEREMIAH

Park, Tottenham, Feb. 4

DOUBTLESS many of your readers witnessed the magnificent aurora which occurred on Sunday, February 4. If any one else has noted the position of the radiant point, as seen from this station, the following observations, made somewhat roughly, from this place (lat. 53° 17′ 8″ N., long. 6° 10′ 22″ W., nearly) may be of use in determining approximately the height of that point above the earth.

At 7.15 (Greenwich time) its zenith distance was 23°; its bearing in azimuth 4° E. of S. At 7.30 its zenith distance was the same; its azimuth 15° E. of S. At 9.10 its zenith distance was 13°; its azimuth 1° W. of S. M. H. CLOSE

Newton Park, Blackrock, Dublin, Feb. 5

LAST evening (Sunday, Feb. 4) there was a brilliant display of aurora visible in North Devon with some unusual features. At 6 o'clock the sky was clear, except a cloud of deep rose aurora over Orion, and another detached portion toward the west. This soon developed into a cloudy arch of the same colour stretching from east to west; then, a little south of the zenith between the Pleiades and Aldebaran, this arch culminated to an obtuse point of white cloud, something like a broad gothic arch. The northern half of the heavens was quite clear, but a series of radiations towards the south, and spreading east and west, issued from this point. For some time it seemed doubtful whether it was aurora, or a peculiar appearance of the clouds caused by high air currents, and a refraction of light from the sun's rays in the higher regions of the atmosphere. At one time there was some appearance of spiral radiations, or drift of cloud from this point near the zenith, with a distinct but irregular gap of clear sky, somewhat similar to the Coalhole in the galaxy near the Southern Cross; but this did not last long, although the general appearance was continued for more than half an hour, with varying play of light, over a space of about 140° of the southern heavens, with pretty well-defined eastern and western boundaries of deep rose colour, culminating in the white focus near the Pleiades, which appeared the centre of action. The rose colour was chiefly confined to the eastern and western boundaries, with intermitting starts of whitish radiation toward the south. Occasionally well-defined streaks of a lighter tint crossed the western portion of the rosy cloud, which appeared to originate from the light of the sun, now, of course, far below the horizon. At length the eastern portion became less brilliant, but still Orion was enveloped in a steady rosy haze, although it gradually became fainter, until, a little before 7 o'clock, the rosy colour below Orion toward the eastern horizon became as brilliant as ever, and soon a straight broad ray of rose colour started up from the horizon. This was not curved or arched, like the whiter radiations which seemed to originate from near the zenith; nor was it, like them, intermittent and wavy; but had the appearance of a broad beam of rosy light originating below the horizon, and darting straight upward in a diagonal direction, proceeding over Castor and Pollux and Jupiter. Then the north side of this became of a peculiar light bluish green; if I may be allowed to coin a word, it was of a moonshiny colour. If the moon had been a few days younger, I should have thought it originated from the moon. This very peculiar and distinct broad beam or bar of light almost developed prismatic colours from its southern rosy edge to its northern bluish-green well defined border. There was also a somewhat indistinct tendency to the same prismatic appearance, spreading some little distance over the heavens on the south side of this beam near the zenith. The northern segment of the sky from Castor and Pollux to about direct west was still perfectly clear, both from cloud and aurora, right down to the horizon; there was a bank of cloud along the southern horizon. About 7 o'clock there was an appearance of rosy tint to the north of the peculiar straight bank spoken of, and this reached as far as the pointers in the Great Bear. About the same time there was a peculiar development of white cloud from the zenith toward the north-west, streaked and fringed with well defined radiations, and this gradually increased until the northern portion of the heavens, which had hitherto been quite clear, was covered to within 30° of the horizon, the border of this cloud being very distinctly and deeply serrated with fan-like shapes radiating from near the zenith. The phenomena I have described occupied more than an hour, and my attention was now drawn from it until after 8 o'clock, when the whole heavens were cloudy, but behind and between the clouds the rosy tint was still visible as an irregular arch stretching from north to west. As the clouds broke off the whitish wavy radiation could be occasionally seen still issuing from near the zenith, and across the western part of the rosy arch were occasionally seen the straight diagonal bars of a brighter shade, apparently caused by the light of the sun, but the clouds obscured most of the phenomena. At a last look near 9 o'clock the clouds had somewhat cleared, and there were two brilliant arches, more like the regular aurora from the north-west horizon towards the zenith, at right angles to the more cloudy arch, which had been visible for some time stretching from the north to the west. W. SYMONS

Barnstaple, Feb. 5

LAST evening (Sunday, February 4) the sky presented a weird and unusual aspect which at once struck the eye. A lurid tinge upon the clouds which hung around suggested the reflection of a distant fire, while scattered among these torn and broken masses of vapour having a white and phosphorescent appearance, and quickly altering and changing their forms, reminded me of a similar appearance preceding the great aurora of October 1870. Shortly some of these shining white clouds or vapours partly arranged themselves in columns from east to west, and at the same time appeared the characteristic patches of rose-coloured light which are seen in an auroral display.

About 8 o'clock the clouds had to a certain extent broken away, and the aurora shone out from behind heavy banks of clouds which rested on the western horizon, the north-eastern horizon being free from cloud and shining brightly with red light. And now, at about 8.15, was presented a most beautiful phenomenon. While looking upwards I saw a stellar-shaped mass of white light form in the clear blue sky immediately above my head, not by small clouds collecting, but apparently forming itself in the same way as a cloud forms by condensation in a clear sky on a mountain top, or a crystal shoots out in a transparent liquid, leaving, as I fancied, an almost traceable nucleus or centre with spear-like rays projecting from it; and from this in a few seconds shot forth diverging streamers of golden light, which descending met and mingled with the rosy patches of the aurora hanging about the horizon. The spaces of sky between the streamers were of a deep purple (the effect of contrast), and the display, though lasting a few minutes only, was equal if not excelling in beauty, though not in brilliancy, the grand display in 1870, before alluded to, in which latter case, however, the converging rays met in a ring or disc of white light of considerable size.

What struck me particularly was the aurora developing itself as from a centre in the clear sky, and the diverging streamers apparently shooting downwards, whereas in the ordinary way the streamers are seen to shoot up from the horizon and converge overhead. The effect may have been an illusion, but if so it was a very remarkable one. Examined with one of Mr. Browning's direct seven-prism spectroscopes, I saw the principal bright line in the green everywhere (the other lines were not visible), and noticed the peculiar flickering in that line which I noticed in 1870, and which has also I think been remarked by Sir John Herschel. The general aurora lasted for some time till lost in a clouded sky, and in fact rain was descending at one time while the aurora was quite bright. Strong wind prevailed during the night. The aurora was probably extensive, as the evening, notwithstanding the clouds, was nearly as bright as moonlight.

Guildford, Feb. 5 T. RAND CAPRON

The Floods

TWO of the largest districts which are most constantly flooded are, perhaps, Oxford and "The Plain of York." The same cause floods both these districts, namely, what Mr. Mackintosh has called "Colonel Greenwood's hard gorge and soft valley theory." Both these districts have been worn down by rain and rivers in the soft oolitic strata; and the Humber and the Thames have ever had, and have now, to force outlets through comparatively hard chalk gorges. The rain-flood waters, checked at these gorges, overflows and deposits alluvium behind the gorges. The same takes place in the soft strata of the Weald, behind the nine comparatively hard chalk gorges of the North and South Downs. GEORGE GREENWOOD

Brookwood Park, Alresford, Feb. 3

Zodiacal Light

THE evening of Feb. 2 being clear, after a long persistence of rainy cloud for many days, about 6.5 P.M. I began to notice the existence of a zodiacal light. Some time later, probably about 6.40, it was considerably brighter than any portion of the galaxy in sight at the time, though this might not have been the impression of an inattentive spectator, as the gradual melting away of its edges produced much less contrast with the ground of the sky than the better defined outline of the Milky Way. Its light was, in fact, so imperceptibly diffused that it was impossible to fix its boundaries or extent with any accuracy. Its general position was, however, undoubtedly a little below the square of Pegasus (where its upper edge fell short of α and γ), and beneath the three stars of Aries; but its light was here so enfeebled that its termination was quite uncertain, and it could only be said that the direction of its axis was towards the Pleiades. Its breadth where most brilliant, near Pegasus, might probably be estimated at 8° or 9°, from comparison with the distance from α to β, and with the length of the belt of Orion; but this determination was liable to great uncertainty. It was thought to show a ruddy tinge, not unlike the commencement of a crimson Aurora Borealis; this may have been a deception, but it was certainly redder or yellower than the galaxy. At 7 I examined it with a little pocket spectroscope, which shows very distinctly the greenish band of the aurora; but nothing of the kind was visible, nor could anything be traced beyond a slight increase of general light, which, in closing the slit, was extinguished long before the auroral band would have become imperceptible. It was still visible at 8.30. The phenomenon had been previously noticed, but with less distinctness, on Dec. 30 and Jan. 11.

T. W. WEBB

Hardwick Vicarage, Herefordshire

Magnetic Disturbance during Solar Eclipse

WITH the known relation existing between the sun and terrestrial magnetic disturbance, it is not surprising that some indication of a change in the earth's magnetism might be expected during a solar eclipse; and the case cited by the Rev. S. J. Perry, of its supposed observation by M. Lion, is not the first instance of the kind.

Shortly after the eclipse of 1870, Signor Diamilla Müller, of Florence, published a paper in the *Gazzetta Ufficiale*, No. 17, describing some magnetic observations made in Italy during the 21st, 22nd, and 23rd December, and from which it appeared that there was a slight variation in the curve of the 22nd, at the time of the eclipse, which did not appear in the curves of the preceding and subsequent days. Signor Müller at once concluded that the variation was produced by the eclipse; but it was pointed out by Senhor Capello, of the Lisbon Observatory, that the same disturbance was recorded by his self-recording instruments, but it occurred there some time before the totality. It was also recorded by the instruments here, and proved to be insignificant when compared with other disturbances continually observed.

A careful examination of the curves for the time of the 1860 eclipse has also failed to show any trace of a similar movement then occurring. G. MATHUS WHIPPLE

Kew Observatory, Feb 5

Circumpolar Lands

MR. HAMILTON, in NATURE of January 25, refers to a paper in which "the rising of the land at the *poles* is inferred as a necessary result of the cooling and contracting of the earth." He then goes on to give the substance of part of the paper, beginning as follows:—

"If a spheroid of equilibrium, in motion about an axis, contract uniformly in the direction of lines perpendicular to its surface, a new spheroid is produced, having a greater degree of eccentricity, because if equal portions are taken off the two diameters, the ratio of the equatorial diameter is increased. This is equivalent to a heaping up of matter around the *equator*."

The reasoning of this latter passage appears sound, but it contradicts the former one. As I have shown in my letter to which Mr. Hamilton replies, the facts, so far as known, appear to point to a relative increase of the *polar* diameter; he admits this, and then gives reasons for expecting a relative increase of the *equatorial* one. He must have made some oversight.

Old Forge, Dunmurry, Jan. 27 JOSEPH JOHN MURPHY

THE HISTORY OF PHOTOGRAPHY

I TRUST you will kindly allow me space for a few lines on the subject of some rare specimens connected with the History of Photography, now in the possession of Madame Nièpce de St. Victor, whose husband it will be remembered was the first to employ glass, and a transparent medium (albumen) for the purposes of photography, thus discovering, to a great extent, the process of Photography as it exists at the present day. The first glass negative, or rather *cliché*, Madame Nièpce possesses, as likewise prints executed in 1848.

Nièpce de St. Victor was likewise one of those who have worked hard to secure *natural colours* in the camera, some very perfect specimens—photographs of coloured dolls—which prove distinctly that the solution of the problem is not impossible, as many believe, are also included in the Nièpce collection, together with some results of early photo-engraving.

Madame Nièpce and family have been left, I regret to say, in very straitened circumstances, for the busy philosopher in his lifetime had but the pay of a subordinate officer in the French Army to subsist on. She has placed in the possession of the Photographic Society this valuable collection of her late husband, and it is proposed to exhibit it at the next meeting of the Society on the 13th inst., and any institution or individual desiring to become possessed of some of the specimens will be readily furnished with information by H. BADEN PRITCHARD

GANOT'S PHYSICS *

GANOT'S Physics is so well known in this country that our task is very different from that of reviewing a new work, and we can do little more than compare this edition with the previous. It is unusual for any large scientific work to pass through five editions in about ten years, and the value of the book may be estimated by this fact. It has passed through more than twice the above number of editions in France, and has been translated into various European languages. In the present edition the type has been altered, and the size of the page somewhat increased, while twenty-eight new illustrations have been added, and the text has been augmented.

The doctrine of energy has of late been so largely developed that we are surprised to find so small an amount of space given to the subject. No more than two pages are devoted to it, while the term "transmutation of energy," does not appear in the index. Neither do we find the terms "Kinetics" and "Kinematics;" yet we imagine that the student who presented himself as a candidate for a Science Scholarship at any of our Uni-

* An Elementary Treatise on Physics, Experimental and Applied. Translated and Edited from Ganot's "Eléments de Physique," by E. Atkinson, Ph.D., F.C.S. Fifth Edition, Revised and Enlarged. 828 pp. 8vo. (London: Longmans and Co. 1872.)

versities, not knowing the meaning of these terms, might find himself quite at sea in some of the questions. In-

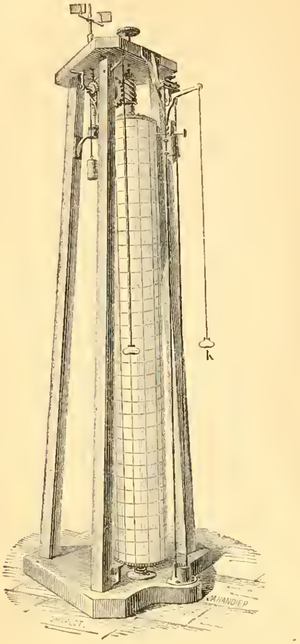


Fig. 1.

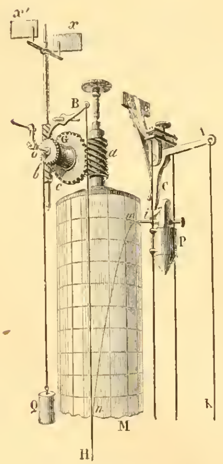


Fig. 2.

deed we do not find much introduction of the terms of the Thomsonian Physics, and this is surely to be regretted; for just as the philosophy of Francis Bacon used to be called the "New Philosophy," so might the Natural Philosophy developed in the treatise of Tait and Thomson be called the "New Physics." The experimental science of the future must be based, we conceive, upon the system therein elaborated.

We are glad to notice a very good account of Morin's apparatus for demonstrating the laws of falling bodies (p. 49), which does not appear in the 1868 edition. The principle of this, it will be remembered, is to cause a falling body to trace its own path upon a rotating cylinder. The accompanying diagram (Figs. 1, 2) needs no explanation. The vanes are for the purpose of producing uniformity of motion in the revolving cylinder; the falling weight is a mass of iron, P, furnished with a pencil, which presses against the paper on the revolving cylinder. The curve traced can be proved to be a parabola, and the paths

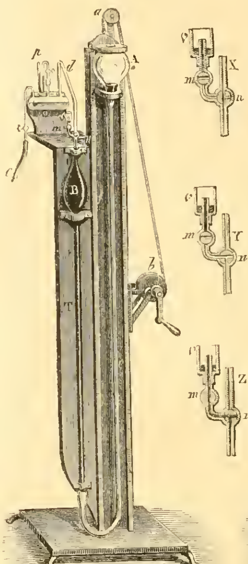


Fig. 3.

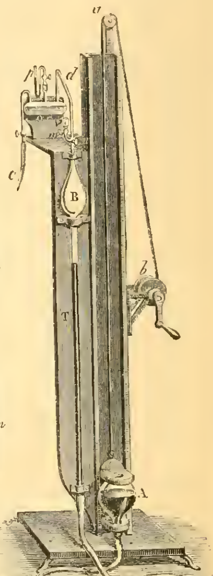


Fig. 4.

traversed in the direction of the descent are shown to vary directly as the squares of the lines in the direction of rotation.

Under the head of "Endosmose of Gases" (p. 97) we find no account of the cause of diffusion of gases, the experiments of Graham, the determination of the relative velocity of atoms by Clausius, and the explanation of such facts as the rate of diffusion of hydrogen being four times greater than that of oxygen. But it may be argued that this rather belongs to Chemistry.

We are glad to see that the law which relates to the volume of gases under varying pressures is now called after its true discoverer, "Boyle's Law," but the experiment, demonstrating at once the incompressibility of fluids and the porosity of dense bodies, is, as usual, attributed to the members of the Accademia del Cimento, while it was in reality proved twenty years earlier with a hollow sphere of lead by Francis Bacon. Again "Mariotte's Tube," as it is called (p. 120), is described and figured by Robert Boyle fourteen years before Mariotte mentions it. Morren's mercury pump for slow but accurate exhaustion is described and figured on p. 141 (Figs. 3, 4); by its means a vacuum of one-tenth of a millimetre of mercury may be obtained.

The Acoustics has been considerably augmented, for while in the 1868 edition it occupied fifty-two pages, it now fills fifty-five larger pages. We notice, among other things, an account and woodcut of König's stethoscope, and of his cylindrical resonator; of Helmholtz's apparatus for the synthesis of sounds; and various new woodcuts of manometric flames. We do not observe any mention of singing or sensitive flames. In the section devoted to heat, we do not find an account of Prof. Guthrie's experiments on the conduction of heat by liquids; or of the recent observations regarding the heat of the moon and certain stars; and the portion relating to the "Mechanical Equivalent of Heat" is still very meagre and insufficient.

The magnetism of iron ships might with advantage be alluded to in the account of Magnetism; and M. Noë's very powerful thermo-electric battery is also worthy of notice. On pp. 596 and 597 we are glad to observe capital figures and descriptions of the electrical machines of Bertsch and Carre; the latter appears to be a most desirable addition to the Physical Laboratory, as, even without a condenser, plates of 49 centimetres diameter give sparks 18 centimetres long, and the machine is not much affected by moisture. The apparatus figured on pp. 678—670 for demonstrating the attraction and repulsion of electric currents by currents, consists of new and improved forms of those devised by Ampère, and is extremely ingenious; as is also the form of solenoid described on p. 690. (Fig. 5.)

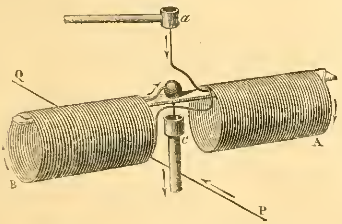


Fig. 5.

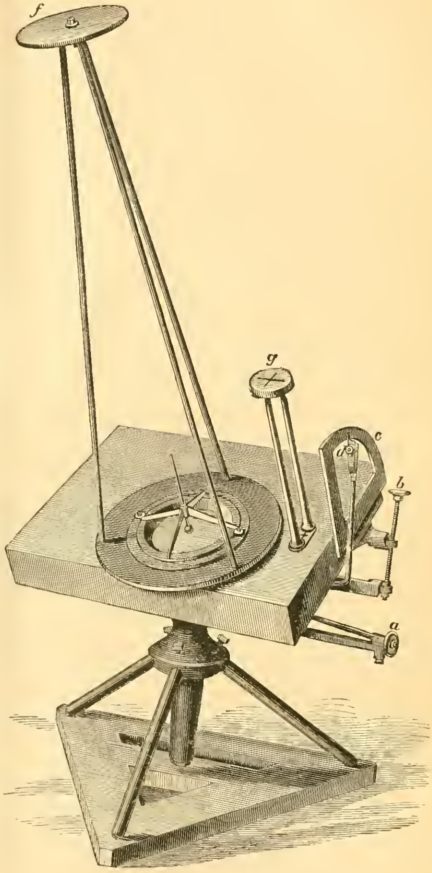
A few alterations in the text would be advisable if a table of errata is introduced; thus (p. 750) no explanation is given of the stoppage of a cube of copper when caused to rotate between the poles of a powerful electro-magnet, as soon as the magnet is made; neither is reference given to the explanation which in another form is given elsewhere. Again (p. 628) we read :— . . . "Kirchhoff has concluded that the motion of electricity in a wire in which it meets with no resistance is," &c. A very few clerical errors are observable :—p. 185, M. Costa should be M. *Corti;* p. 246, topmost line, "substances by which their action," &c., should read "*which by* their action;" p. 289, line ten from the top, p should be p'; and p. 524, line 4, we find "*plain* polarised light."

These, however, are quite minor matters; the book was a good one at the outset of its career, and each succeeding edition has rendered it more and more complete. The above remarks are made rather as suggestions than in any spirit of adverse criticism. Ganot's Physics is a great addition to our scientific literature, and neither student nor *savant* could spare it from his library.

G. F. Rodwell

THE SOLAR ATMOSPHERE

The object of the investigation discussed in Nature (No. 101, pp. 449-452) being merely that of ascertaining whether the incandescent matter contained in the solar atmosphere transmits radiant heat of sufficient energy to admit of thermometric measurement, no particular statement was deemed necessary regarding the spectrum which appeared on the bulb of the focal thermometer after shutting out the rays from the photosphere



during the experiments. The appearance of this spectrum has in the meantime been carefully considered. Its extent and position suggest that the depth of the solar atmosphere far exceeds the limits hitherto assumed.

The accompanying illustration represents an apparatus constructed by the writer to facilitate the investigation. Evidently the expedient of shutting out the photosphere while examining the effect produced by the rays emanating from the chromosphere calls for means by which the sun may be kept accurately in focus during the period required to complete the observations. The main features of the apparatus being shown by the illustration, a brief description will suffice. The parabolic reflector which concentrates the rays from the chromosphere (described in the previous article) is placed in the cavity of a conical dish of cast-iron, secured to the top of a table suspended on two horizontal journals, and revolving on a vertical axle. The latter, slightly taper, turns in a cast-iron socket which is bushed with brass and supported by three legs stepped on a triangular base, resting on friction-rollers. The horizontal journals referred to turn in bearings attached to a rigid bar of wrought-iron situated under the table, firmly secured to the upper end of the vertical axle. The horizontal angular position of the table is adjusted by a screw operated by the small hand-wheel *a*, the inclination being regulated by another screw turned by the hand-wheel *b*. A graduated quadrant, *c*, is attached to the end of the table in order to afford means of ascertaining the sun's zenith distance at any moment. The index *d*, which marks the degree of inclination, is stationary, being secured to the rigid bar before described. The rays from the photosphere are shut out by a circular disc, *f*, composed of sheet metal turned to exact size, and supported by three diagonal rods of steel. These rods are secured to the circumference of the conical dish by screws and adjustable nuts in such a manner that the centre of the disc *f* may readily be brought in a direct line with the axis of the reflector. The mechanism adopted for adjusting the position of the table by the hand-wheels *a* and *b* requires no explanation; but the device which enables the operator to ascertain when the axis of the reflector is pointed exactly towards the centre of the sun demands particular notice. A shallow cylindrical box, *g*, provided with a flat lid and open at the bottom, excepting a narrow flange extending round the circumference, is firmly held by two columns secured to the top of the table. A convex lens of 26 inches focus is inserted in the cylindrical box, the narrow flange mentioned affording necessary support. The lid is perforated by two openings at right angles, 0·05 inch wide, 2·5 inches long, forming a cross, the lens being so adjusted that its axis passes through the central point of intersection of the cross. The face of the table being turned at right angles to the sun, or nearly so, it will be evident that the rays passing through the perforations and through the lens will produce, at a certain distance, a brilliantly illuminated cross of small size and sharp outline. A piece of ivory, or white paper, on which parallel lines are drawn intersecting each other at right angles, is attached to the top of the table in such a position that the centre of intersection of the said lines coincides with the axis of the lens. This axis being parallel with the line passing through the centre of the disc *f* and the focus of the reflector, it will be perceived that the operator, in directing the table, has only to bring the illuminated cross within the intersecting parallel lines on the piece of ivory. Ample practice has shown that by this arrangement an attentive person can easily keep the disc *f* accurately in line with the focus of the reflector and the centre of the sun during any desirable length of time. The absence of any perceptible motion of the column of the focal thermometer during the experiments which have been made furnishes the best evidence that the sun's rays have been effectually shut out by the intervening disc, which, it should be remembered, is only large enough to screen the aperture of the reflector from the rays projected by the photosphere. It may be noticed that actinometric observations cannot be accurately made unless the instrument is attached to a table capable of being directed in the manner described; nor is it possible to measure the dynamic energy transmitted by solar radiation unless the calorimeter employed for the purpose faces the sun with the same precision as our parabolic reflector. It is worthy of notice that the lightness of the illustrated apparatus renders exact adjustment easy, since screws of small diameter and fine pitch may be employed. It only remains to be stated that in order to admit of accurate examination of the spectrum before referred to, the thermometer is removed during investigations which do not relate to temperature, a cylindrical stem of metal, 0·25 inch diameter, coated with lamp-black, being introduced in its place.

With reference to the result of recent experiments, it is proper to state that, at the present time, the sun's zenith distance being now nearly 60° at noon, no perceptible heating takes place in the focus of the parabolic reflector. The observations relating to temperature mentioned in the previous article, were made when the zenith distance was only one-third of what it is at present. The consequent increase of atmospheric depth, at this time, has completely changed the colour of the spectrum, and rendered the same so feeble that its extent cannot be determined. As seen last summer, before the earth had receded far from the aphelion, the termination of the spectrum reached so far down that an addition of 0·15 inch to the radius of the disc *f* would scarcely have shut it out. Now an addition of 0·15 inch to the radius of the disc corresponds to an angular distance of 9′ 45″; hence, assuming the radius of the photosphere to be 426 300 miles, the depth of the solar atmosphere cannot be less than 255 000 miles. And, judging from the appearance at the period referred to, there can be little doubt that a larger and more perfect reflector will enable us to trace the spectrum still further down. Consequently, a further enlargement of the disc *f* will be required to extinguish wholly the reflected light from the solar atmosphere. It is reasonable, therefore, to suppose that the depth of the solar atmosphere will ultimately be found to exceed very considerably the foregoing computation.

It has been suggested regarding the instituted investigations of the radiant heat transmitted by the chromosphere, that the thermo-electric pile ought to be employed in combination with the parabolic reflector. The object of the investigation being simply that of proving by the feebleness of the radiant power transmitted to the surface of the earth that the chromosphere and outer strata of the sun's envelope do not possess radiant energy of sufficient intensity to influence solar temperature as supposed by Secchi, tests of the suggested extreme nicety are not called for.

With reference to the effect of increased depth, the small amount of retardation suffered by the rays in passing through the highly attenuated atmosphere of the sun, previously established, shows that the question of solar temperature will not be materially affected, even should it be found that the depth of the envelope is greater than the radius of the photosphere.

J. ERICSSON

THE RIGIDITY OF THE EARTH

SIR WILLIAM THOMSON'S views regarding the rigidity of the earth have been hitherto received in silence by those who entertain different opinions from him; but it does not follow on this account that they regard his position as unassailable. It is more satisfactory to attempt to establish positive results in science, than to criticise the labours of others; but as Sir William Thomson, by his letter in NATURE for January 18, manifestly invites discussion, I hope I shall be excused for making the following remarks.

When nearly ten years since I saw the abstract in the Proceedings of the Royal Society which he appends to his letter, I resolved to suspend my judgment until I had an opportunity for reading his papers *in extenso*. To such of your readers as happen to be interested in this question, and who have not yet seen these publications, I would venture to recommend a similar course. In the

"Philosophical Transactions" for 1862, the memoir on the rigidity of the earth is fully printed, and immediately following it is another designated "dynamical problems regarding elastic spheroidal shells and spheroids of incompressible liquid." The conclusions arrived at in the first are essentially and admittedly dependent on the investigations presented in the second. Not long after they were published I gave my best attention to the study of both, and it soon appeared to me that the problems treated in the second could have no physical bearing on the question of the earth's structure. The very title of this memoir partly reveals its character in this respect. In order to apply the results obtained in this memoir to the earth, it is supposed to be a spheroidal homogeneous elastic shell filled with incompressible fluid; whereas in such an inquiry the earth can scarcely be supposed to be otherwise than a heterogeneous solid envelope containing a fluid whose properties are not inconsistent with those of fluids coming under our notice. Under this form I have treated the hypothesis in the "Philosophical Transactions" for 1851, and also in subsequent publications.

Incompressibility is not a property of any known fluid; and Neumann, when referring in his comprehensive treatise on geology to the influence of pressure in promoting the density of the interior parts of the earth, expresses what is very generally admitted among philosophical geologists as well as physical inquirers, when he says that "fluid bodies are endowed with *far more* compressibility than solids."* Hypotheses are often indispensable in physical inquiries where we are proceeding from the known to the unknown, but there are two conditions to which they should conform; first, they should be capable of verification by a comparison of the results to which they lead with those of observation, and secondly, they should not contradict established physical laws or the known properties of matter, unless the contradiction is specially explained and fully accounted for. The second of these conditions is clearly violated when the internal fluid of the earth is supposed to differ from all known fluids by being supposed to be incompressible. And this violation is especially flagrant when the solid matter enclosing the incompressible fluid is supposed to be at the same time elastic and therefore compressible, and when, moreover, the line of reasoning adopted as to the earth's internal structure pointedly depends upon these assumptions as to the properties of its fluid and solid portions. Sir William Thomson endeavours to prove, by a process of *reductio ad absurdum* that the interior of the earth is for the most part or altogether solid; in other words, he supposes the interior to be fluid, and then tries to show that the tidal actions produced in this fluid by the sun and moon must cause oscillations in the crust which have not been observed. He may justly claim to have proved that the earth does not consist of an elastic solid envelope enclosing a mass of the ideal substance called an incompressible liquid, but he has not proved the point which he intended to establish, namely, the absence of an interior fluid nucleus endowed with the properties commonly attributed to fluids. He also supposes throughout his investigations, in the same manner as was supposed by Mr. Hopkins, that the transition from the solidity of the shell to the fluidity of the nucleus is not gradual but abrupt. Those who maintain the validity of the hypothesis of the interior fluidity of the earth are far from holding this opinion. On the contrary, all observations hitherto made on the materials of the earth lead to the conclusion that the solid shell is so constituted as to present first a superficial coating whose mechanical properties we can partly ascertain by direct experiment; secondly, a mass whose density and rigidity probably increase with the depth from the outer surface; thirdly, an interior coating in which the effects of pressure are resisted by those of temperature, and where an imperfectly fluid and pasty mass is in contact at one side with the solid shell, and on the other with the more perfect fluid. This mass should be manifestly much more yielding and compressible than the perfectly solidified shell; for if compression tends to increase the rigidity of solid matter, the middle division of the shell, as just described, should be more rigid than its superficial portion, and very much more rigid than the interior pasty mass. The work performed by small changes of shape in the fluid nucleus due to the action of exterior disturbing bodies should thus be expended partly in producing small variations of density among the compressible strata of which it is composed, and partly in changing the shape of the yielding matter of the inner surface of the shell. The deformations of a shell consisting of homogeneous elastic matter, such as steel acted upon by exterior forces, must be the resultants of all the elementary deformations among its particles summed up or integrated. It would behave somewhat like a vibrating bell; but such is not the behaviour to be expected in a mass of discontinuous and heterogeneous materials. Vibratory motions in such bodies are for the most part extinguished by interferences, or their amplitudes are at least very much reduced.

If the conclusions deduced by M. Perrey of Dijon from his voluminous labours so often referred to by Mr. Mallet in his Reports on Earthquakes, be correct, some connection between these disturbances and the phases of the moon seems to be established which may be due to such comparatively feeble vibratory actions. Sir William Thomson's conclusions rightly interpreted show that the constitution of the fluid nucleus and the nature of the materials of the shell must be essentially different from what he supposes in order to establish these conclusions. A person who never saw a railway train might as justly reason as to the impossibility of travelling in it at high rates of speed, by demonstrating that the shocks experienced by perfectly rigid carriages connected without any compressible arrangements would be too great for travellers to endure, if not too great for the permanent integrity of the carriages themselves. In assuming the incompressibility of the fluid nucleus for the purposes of his indirect demonstration of the rigidity of the earth, Sir William Thomson makes a *petitio principii* nearly as vital as shocks incident to influence of buffers in reasoning on the the omission of the railway carriages.

I am at a loss to know where any warrant was found for affixing the property of incompressibility to the supposed fluid nucleus of the earth; and those who maintain the hypothesis of the interior fluidity of the earth are entitled to repudiate an assumption fastened on that hypothesis not only in opposition to evidence derived from experiments on fluids, but in direct contradiction to the arguments employed by them in discussing the question of the earth's structure.

HENRY HENNESSY

* Lehrbuch der Geologie, i. p. 268, 2nd edition.

THE LANDSLIPS AT NORTHWICH

IN the "Notes" of the number of NATURE, for Jan. 25, I find one referring to the landslips at Northwich in Cheshire, by mistake called Nantwich. As the description given of these landslips and their cause is scarcely accurate, your readers may like to see a short account of them.

Northwich is the great centre of the Cheshire salt trade. The manufacture is principally carried on now at Northwich and Winsford, both towns lying in the valley of the River Weaver, though formerly Nantwich was engaged in this trade, and Middlewich still continues so to be. The position of the latter is indicated by its name, it lying between Northwich and Nantwich. The salt is found lying in two beds, called the upper and lower rock salt. The first bed is met with in the neighbourhood of Northwich at the depth of about forty yards, and is twenty-five yards thick. Although brine springs had been known and worked as early as the time of the Norman Conquest or earlier, yet the bed of rock salt was only discovered in 1670 when searching for coal at Marbury, about a mile to the north of Northwich. During the last 200 years this rock salt has been worked, or to speak more correctly, for more than a century the upper bed was worked, when an agent of the Duke of Bridgewater sank lower still, and, after passing through about ten yards of hard clay and stone, with small veins of rock salt running through it, the lower bed of rock salt was discovered. This lower bed is between thirty and forty yards thick, but only about five yards of the purest of it is "got." This good portion lies at a depth of from 100 to 110 yards, according to the locality. In the neighbourhood of Winsford both beds are met with at a much greater depth. The whole of the rock salt obtained is got now from the lower bed, and last year it reached nearly 150,000 tons, probably the largest quantity ever obtained in one year. It may as well be said that this mining of rock salt has had nothing whatever to do with the subsidences spoken of, though the wording of the note would lead readers to expect the contrary. At present there is no danger to be expected from the lower bed of rock salt. The whole danger arises from the upper bed, as will be seen from the following account:—The salt trade of Cheshire is a very extensive one, and during the year 1871 upwards of 1,250,000 tons of white salt have been sent from the various works in that county. The whole of this immense quantity has been manufactured from a natural brine which is found in and around Northwich and Winsford, as well as in several other smaller places. This brine is produced by fresh water finding its way to the surface of the upper bed of rock salt, technically called the Rock Head. The fresh water dissolves the rock salt, and becomes saturated with salt. The ordinary proportion of pure salt in the brine is 25 per cent. To obtain the quantity of salt above mentioned, it would be necessary to pump 5,000,000 tons of brine. The pumping of brine is incessantly going on, and as a natural consequence the bed of rock salt is being gradually dissolved and pumped up. As the surface of the salt is eaten away, the land above it subsides. This subsidence is not spread over the whole surface, but seems to follow depressions in it, thus forming underground valleys with streams of brine running to the great centres of pumping. Wherever a stream of brine runs, there the subsidence occurs, and in many localities the sinking is very rapid and serious, but fortunately is almost always gradual and continuous. An immense lake, more than half a mile in length, and nearly as much in breadth, has been formed along the course of a small brook that ran into the river Weaver, and this lake is extending continually. Besides this gradual continuous sinking, which affects the town of Northwich very seriously, causing the removal and rebuilding of houses or the raising of them by screw-jacks in the American fashion, the raising of the streets and so on, there is a sudden sinking of large patches of ground, leaving large deep cavities such as described in your Note. These latter are more terrifying and dangerous. They are in the majority of cases caused by the falling-in of old disused mines in the upper bed of rock salt. These old mines were worked so as to leave but a thin crust of rock salt between the superincumbent layers of earth and the mines. The roof of the mine is supported by pillars of rock salt at intervals. Of course the weakest and most dangerous point is the old filled-up shaft. As most of these mines have been disused for nearly a century, the position of the old shafts is unknown. When the brine has eaten away the layer of rock salt left as a roof, the whole of the earth lying above falls into the mine, and an enormous crater-like hole, some 100 feet or more in depth, is formed, which in process of time becomes filled up with water, the mine itself being choked with earthy matter. In the immediate neighbourhood of Northwich there are a great number of these rock pit holes, as they are called, and it is nothing very unusual for one to fall in.

The rock miners, as they are called, were at work in the lower mine last year when one of these sudden subsidences occurred. They knew nothing of it. I have been myself under this hole, and it was a fearful one to look at when it first went in. There is no communication between the upper and lower beds, and the miners have about thirty yards of hard clayey stone and rock salt between them and the upper old mines. The subsidence more particularly alluded to in your Notes is not in the immediate neighbourhood of Northwich, but rather midway between Northwich and Winsford, near Marton Hall. It is rather difficult to know what is its cause, as there is no record of any mines ever being worked in that neighbourhood. The general belief is that the rock salt, which undoubtedly underlies the whole neighbourhood, has been gradually dissolved, and that a sinking has commenced as at Northwich; then that, owing to some peculiarity of the particular overlying strata—probably to their sandy nature, as quicksands are known to exist about Northwich—the earthy and sandy matter of the immediately overlying strata has been carried away by the brine streams till a large hollow has been formed. This has continued till the superincumbent mass could not be borne up any longer, and thus suddenly fell in, filling up the lower cavity, but opening a large crater-like pit from the surface.

A Government inspector has been to the neighbourhood, and his report is expected very shortly.

The whole neighbourhood of Northwich is well worthy of more attention than it has received, and it is surprising that our geologists have not been able to give a better account of the rock salt formation than has yet been done.

THOS. WARD

NOTES

WE are glad to be able to state that the severe sentence passed upon M. E. Reclus has been changed, in consequence of the representations of the scientific men of this and other countries, into the comparatively mild one of exile from France.

WE understand that the Chair of Anatomy in the new German University of Strasburg has been offered to, and declined by, Prof. Gegenbaur, who has done so much to raise the scientific reputation of the University of Jena. A similar offer has also been made to Gegenbaur's distinguished colleague, Haeckel, the result of which is not yet announced.

THE Master and Senior Fellows of St. John's College, Cambridge, have elected Mr. J. B. Bradbury, M.D., of Downing College, Linacre Lecturer in Medicine in the room of Dr. Paget, who has been elected Regius Professor of Physic.

THE Royal Commission on Scientific Instruction and the Advancement of Science recommenced their sittings yesterday.

THE two Smith's Prizes of the University of Cambridge have been this year awarded to the First and Second Wranglers respectively.

WE regret to learn that the Australian Eclipse Expedition has proved a failure, through the unfavourable state of the weather at the point of observation.

IT is with great regret we have to record the death on Wednesday, January 31, at Torquay, of Dr. G. E. Day, F.R.S., late Chandos Professor of Medicine in the University of St. Andrew, at the age of 56. Our columns have borne frequent evidence of the extent of Dr. Day's acquirements in many branches of

Natural History. He was one of the founders of the Pathological and Cavendish Societies.

THE name of Colonel Chesney, F.R.S., of the Royal Artillery, who died on Tuesday, the 30th ult., at his residence near Kilkeel, Co. Down, Ireland, in the 83rd year of his age, was almost more familiar to the last generation than to this. Among his various titles to eminence as traveller, *savan*, and military critic, he will be chiefly known as "the pioneer of the overland route to India." It is now nearly forty years since General, then Captain, Chesney returned from his explorations of the Euphrates for the purpose of establishing steam communication with India *viâ* Egypt and Asia Minor, to ask the Government to give him the command of an expedition. The demand was granted; two vessels, the *Tigris* and the *Euphrates*, were placed at his disposal. The indefatigable manner in which he prosecuted his scheme, in the face of many disappointments and discouragements, is well known. He has himself written the history of his travels and adventures; and the lines of communication now in existence bear witness to the practical value of his projects. General Chesney has for many years back enjoyed the repose which was the fitting reward of much arduous toil; and now leaves behind him the record of a useful, honourable, and well-spent life.

DR. WILLIAM BAIRD, F.R.S., whose death we recorded last week, after a long and painful illness, was born at Eccles, in Berwickshire, in the year 1803, educated at Edinburgh, and received in 1823 an appointment as surgeon from the East India Company. While in this office he visited India, China, and many other countries the natural history of which he carefully studied. In 1831 he published a paper "On the Luminosity of the Sea," in *Loudon's Magazine of Natural History*, and from that time became a frequent contributor to the scientific journals, more especially to the "Transactions" of the Berwickshire Naturalists' Club. In 1838 he compiled a Cyclopædia of the Natural Sciences. In September 1841 he was appointed an Assistant in the Zoological Department of the British Museum, which office he filled till his death. In 1851 his monograph on the British Entomostracous Crustacea, a work of great ability and research, was published by the Ray Society. Between the years 1838 and 1863 he contributed a number of papers on the Entomostraca to the "Annals of Natural History," and the "Proceedings" of the Zoological Society. During the latter years of his life his attention was principally given to the Entozoa, of the then known species of which he had as early as 1843 drawn up a catalogue, which was published by the trustees of the British Museum. Numerous papers on the same subject were also contributed by him to the "Proceedings" of the Zoological Society, the "Transactions" of the Linnean Society, &c. Latterly he was engaged in preparing a general catalogue of the Entozoa, for which he had accumulated a vast amount of material. His knowledge of some other branches of natural history was equally extensive and profound, and his death will leave a gap among those who were acquainted with his varied acquirements, and the courtesy and readiness to assist displayed to all who sought his help or advice.

THE *Academy* records the death of Prof. Trendelenburg, of Berlin, who had attained a two-fold eminence as a philologist and Aristotelian commentator, and as an original thinker.

THE Waynflete Professorship of Chemistry at the University of Oxford, will shortly become vacant by the resignation, through ill-health, of Sir Benjamin Collins Brodie, Bart., M.A. The Waynflete Professorship of Chemistry was directed by the ordinance of the University Commissioners of 1854, relating to Magdalen College, to be founded in that college in lieu of certain prælectorships mentioned in its ancient statutes, and to be maintained by a stipend of 600*l*. per annum. The Professor is elected by the Chancellor of the University, the Visitor and President of the College, and the Presidents of the Royal Society and of the College of Physicians. Prof. Brodie was elected in 1865, and was the first professor under the new ordinance, having previously resigned the Aldrichian Professorship of Chemistry, which he had held since the resignation of the late Dr. Daubeny, and which chair was suppressed in 1866, the revenues being applied to the payment of a salary of a Demonstrator, and to the purchase of chemical apparatus or other means towards the study of chemistry in the University.

IN the *Gazette of India* is the following tribute to the memory of the late Archdeacon Pratt:—"The Governor-General in Council has received with deep regret official intimation of the death of the Venerable the Archdeacon of Calcutta, the Reverend J. H. Pratt, on the 28th ultimo, at Ghazeepore, in the North-Western Provinces. The Governor General in Council cannot allow the death of Archdeacon Pratt to pass unnoticed by the Government which he served so long and so well. Mr. Pratt entered the service in the year 1838, and was appointed Archdeacon of Calcutta by the late Bishop Wilson on the 6th October, 1849. Under the ordinary rules of the service, Mr. Pratt would have retired in October, 1867, but so efficiently had he filled his high office in the Church, that he was solicited by Government, with the full approval of Her Majesty's Secretary of State, to continue to hold it. In adopting this course the Government was moved not only by its own appreciation of the Archdeacon's services, but the strong recommendation of the late Bishop Cotton, who bore testimony to Archdeacon Pratt's eminent scientific attainments and university distinctions, to the active part which he had taken in the management of the diocese, and in the promotion of all good works, and to his personal piety and high Christian character. At a later date Her Majesty's Secretary of State, in sanctioning the retention of Archdeacon Pratt in the service until October 1872, remarked:—'I cannot refrain from expressing the high sense I entertain, in common with the present Bishop of Calcutta, the Lieutenant-Governor of Bengal, and your Excellency in Council, of the zeal and ability with which he has for so many years faithfully and laboriously discharged the duties of his Office.' The Governor-General in Council feels assured that the death of the Venerable Archdeacon will be mourned by the entire Christian community in India."

IT is announced that Professor Flower will commence his annual Hunterian Lectures on Comparative Anatomy in the Theatre of the Royal College of Surgeons on Friday, the 16th inst., at four o'clock. The lectures will be continued at the same hour every Monday, Wednesday, and Friday until the 27th of March. The subjects to be embraced by the present course are the modifications of the organs of digestion, including the mouth, tongue, salivary glands, alimentary canal, liver, and pancreas. These will be treated of in detail in the various animals composing the class Mammalia, and if time should permit, a review of the principal variations of the same parts in the other Vertebrata will follow. The lectures will be illustrated as fully as possible by specimens from the Museum, and by diagrams, and it should be added, are open without fee to any gentleman presenting his card at the door.

THE *Times of India* calls attention to the very scant recognition which literary or scientific merit has received in conferring the distinction of the Star of India. Although the Order of the Star of India was established for the reward of good service of every kind, and the soldier, the civilian, the diplomatist were not considered, on the institution of the Order, to have any better claim to the decoration than the man of science or the man of letters, yet on the list there is at the present time scarcely a single representative of literature, science, or art. The *Times* strongly commends the claims of Dr. Forbes Watson and Dr George Smith to this distinction, for the admirable work done in bringing the English public face to face with the arts and manufactures of the East, services which have as yet received no recognition whatever from the Crown.

THE brilliant display of the aurora borealis, seen in London on Sunday night, of which various accounts will be found in our columns, appears to have been observed in France, as well as in Wales. Scotland, and Ireland. The phenomenon was seen in Turkey and also in Egypt. A telegram from Alexandria says that a large space of sky was illuminated for five hours. The report of the Meteorological Department on Monday notices the wide extent of the display, and adds, "a considerable change in the weather seems likely."

AT his inauguration as Rector of the University of Edinburgh on Monday last, Sir Wm. Stirling-Maxwell is reported to have made the following pertinent observations on the medical education of women:—"He was in favour of teaching women everything that they desired to learn, and for opening to them the doors of the highest oral instruction as wide as the doors of book-learning. As to medical education, he said that so long as women would minister to their sick children and husbands, he must hear some argument more convincing than he had yet heard why they were to be debarred from learning the scientific grounds of the art of which they were so often the empirical practitioners, or the docile and intelligent instruments."

THE *Academy* for February 1 contains a reply, by Prof. Helmholtz, to Prof. Jevons's article on "The Axioms of Geometry," in our issue for October 19.

WE learn from the *British Medical Journal* that the Brown Institution for Sick Animals is likely to commence at once a work of great public utility. Aided by a handsome grant from the Chambers of Agriculture, Profs. Sanderson and Klein, and Mr. Duguid, will undertake an extended series of observations on the treatment and comparative pathology of pleuro-pneumonia, an epizootic which commits the most costly ravages among our herds.

"JUSTICES' Justice" has become a proverb. Here is a sample of justices' science:—At Chelmsford the county magistrates declined to grant the use of the Shire-hall for a lecture on the sun, illustrated by experiments in spectrum analysis, on the ground that the electric light might endanger the safety of the building!

THE *American Naturalist* for January reprints a correspondence between the Commissioner of Agriculture for the United States' Government, and Prof. Asa Gray, and other botanists, respecting the dismissal of Dr. C. C. Parry from his office of botanist to the department, which appears to have been performed in a very summary manner, and on slight grounds.

MR. M. C. COOKE, the well known mycologist, announces his intention, if the names of a sufficient number of subscribers can be obtained, to issue monthly a small journal, with illustrations, devoted absolutely to Cryptogamic Botany. It will serve as a sort of Appendix to the Lichen and Fungi Floras recently published, by recording and describing new species as they are found. Although British Cryptogamia will occupy the first place, it is intended to record from time to time what is doing abroad in all the Cryptogamic families (except ferns), and to keep the student acquainted with what is being published in foreign countries as well as his own. Monographs of genera and families, critical observations on species, and all kindred subjects, will receive attention. The co-operation is promised of the Rev. W. A. Leighton, Dr. Lauder Lindsay, Dr. Braithwaite, F. Kitton, and other specialists.

THE *Journal of Botany* states that a re-issue is in course of preparation of Lindley and Hutton's "Fossil Flora of Great Britain," originally published in 1837, and now very scarce. A supplementary volume will be added by Mr. Carruthers, which will contain a critical revision of the species in the original book, and figures and descriptions of all the important additions to fossil botany made during the last thirty-five years.

A CLEVER application of science to commercial purposes has been made by an Italian gentleman, M. Eugenio de Zuccato, of Padua. By means of the invention any number of copies of a manuscript or design, traced upon a varnished metal plate, may be produced in an ordinary copying press. The *modus operandi* is very simple. To the bed and upper plate of a press are attached wires leading from a small battery, so that when the top of the instrument is screwed down the two metal surfaces come into contact, and an electric current passes. An iron plate resting upon the bed of the press is coated with varnish, and upon this surface is written with a steel point any communication it is desired to copy. The letters having thus been formed in bare metal, a few sheets of copying paper are impregnated with an acid solution of prussiate of potash, and placed upon the scratched plate, which is then subjected to pressure in the copying press. An electric current passes wherever the metal has been left bare (where the writing is therefore), and the prussiate solution acting upon the iron, there is found prussiate of iron, or Prussian blue characters, corresponding to those scratched upon the plate. The number of copies that may be produced by this electro-chemical action is almost unlimited, and the formation of the Prussian blue lines is, of course, instantaneous. The patent, which is, we believe, the property of Messrs. Waterlow and Sons, forms a remarkable instance of science serving as handmaiden to the man of business.

IT will be remembered that a process of engraving by means of a forcible jet of sand was recently invented in America by Mr. Tilghman, and applied to photography, a gelatine relief being used as the mask or shield containing the design. The *Photographic News* states that a further modification has been patented by Mr. Morse, who uses a new method of propelling the sand. He provides a simple box or hopper, from which depends a small tube about 5ft. long, and no machinery whatever beyond this is used. A mixture of corundum and emery, in the form of powder, is placed in the hopper and allowed to descend through the tube. The object to be engraved is held under the extremity of the tube, so that the engraving powder will fall upon it, and in a few minutes' time the most splendid ornamental designs are cut, with marvellous exactitude and surprising beauty. An American paper says:—"We have seen engraved effects, produced by this process, upon glass and silver ware, that altogether surpass anything that has ever been attempted by the most skilled hand labour. This simple and beautiful invention promises to revolutionise the art of plate and glass engraving. By its use the adornment of all kinds of wares, in the most superb manner, may be quickly accomplished, at a tithe of the cost of the ordinary methods."

A CATALOGUE is printed of the Meteoric Collection of Mr. Charles Upham Shepard deposited in the Wood's Building of Amherst College, Mass., U.S.A. It comprises 146 litholites or meteoric stones, which are considered unquestionably authentic, from all parts of the world, the time of fall varying from the year 1492 to 1871, and 93 siderites or meteoric irons, which fell between 1735 and 1870. The total weight of the collection is above twelve hundred pounds. The heaviest iron, that of Aeriotopos, weighs 438 pounds; the smallest, that of Otsego, half an ounce. The largest entire stone, that of New Concord, weighs 52 pounds; the smallest, from Hessle, less than 50 grains. The whole number of specimens exceeds five hundred. The collection embraces, besides numerous casts, an extensive series of doubtful meteorites, in which all the principal irons and stones of this descriptionare represented.

SCIENTIFIC INTELLIGENCE FROM AMERICA *

THE statement, by Professor J. D. Whitney, of the present condition of the geological survey of California, lately presented to the Governor of the State, gives a gratifying picture of the activity and success in accomplishing the objects for which the exploration was authorised. The State Geologist remarks that less has been done than he had hoped, in consequence of the suspension of the appropriations by a preceding Legislature. Since the work was resumed, however, as the result of renewed appropriations by the Legislature of 1869, the survey has been carried on as rapidly as the nature of the service would allow. Among the points particularly engaging the attention of the State Geologist was the completion of the topographical map of California, it being readily understood that this must be a necessary preliminary to a geological map. The survey of Central California was considered especially interesting and important, embracing, as it does, that portion of the State from Owen's Lake on the south to Lassen's Peak on the north, or between 36° and 40° 30′ north and south, and 117° 30′ and 123° east and west, the whole area comprising about one-third of the State, with probably ninety-five per cent. of the population residing in it. Of the portion included within these limits, represented upon four maps, three are entirely drawn and partially engraved, while the fourth is two-thirds drawn, with the field-work of the remaining third yet to be done. A preliminary map, however, of the whole of California, on a scale of eighteen miles to an inch, has been drawn, in compliance with the wish of the community, and will soon be ready for distribution. Besides these, other works connected with the same subject are reported by the State Geologist, being the new editions of the Yosemite Guide-book, and the publication of the first volume of the "Ornithology of California," which is characterised as a work exquisitely illustrated and admirably printed. The remaining volumes of the series of reports are so far completed as only to wait the continuance of appropriations to place them in hand and secure their early appearance. Arrangements have also been made with Mr. Lesquereux to work up the fossil plants of California, and with Dr. Leidy and Prof. Meek in regard to the fossils. Prof. Brewer, of the Survey, is well advanced in the work on the Botany of California, which, when completed, will doubtless be used extensively as a text-book. It is much to be hoped that very liberal appropriations will be made for these important objects, since its chief and his assistants are known to be among the very best specialists in America, and their work has commanded the highest respect among naturalists at home and abroad. The reports themselves are models of perfection in regard to typography and general execution, and are not to be surpassed by the finest European works, whether published by governments or private parties. It may be stated as a well-known fact that much interest has been excited throughout the scientific circles of Europe by the character of the work done under the auspices of the State, and the utmost admiration expressed in regard to its liberality and enterprise; this example being commended to European governments as eminently worthy of their imitation.—A letter from Captain Buddington, the sailing-master of Captain Hall's vessel, the *Polaris*, dated at Upernavik, reports that the party were in good health and spirits; and that Mr. Chester, the first mate, had gone up the coast to bring down Hans Christian, Dr. Kane's Esquimaux hunter, who was to join the expedition.—Among the many works published by the United States government, or at its expense, there are few that exceed in intrinsic value, as well as in beauty, the volumes hitherto printed belonging to the series of reports made by Mr. Clarence King, at his geological and other explorations of the region along the fortieth parallel of latitude. This expedition is still occupied in carrying out the work assigned to it by the engineer department of the army, while reports are now being made of such portions of the work as have been completed. It is nearly a year since the volume upon the mining industry of the Sierra Nevada and other mineral regions of the West was published, as prepared mainly by Mr. J. D. Hague (one of Mr. King's assistants), but including articles by Mr. King himself, and other members of the corps. This was accompanied by a large atlas of plates, and contained full details of all the methods of metallurgical operations and manipulations, together with drawings of machinery, plans of mines, sketches of mining geology, &c. This book has been received with great favour everywhere, and has redounded greatly to the credit of the United States, first in authorising the research, and then in publishing the results in so superior a style. We now have to chronicle the appearance of another volume of the series—namely, the Botany, as prepared under the direction of Mr. Sereno Watson, the botanist of the expedition. This constitutes volume five of Mr. King's reports, and number eighteen of the professional papers of the engineer department of the army. The work embraces a report upon the geography, meteorology, and physics of the region explored as connected with the general botany of the country, catalogues of the known plants investigated, descriptions of new genera and species, and various appendices; these accompanied by forty plates of new or rare species. Another volume of the series is now in press, and will include the zoological portion, as furnished by Mr. Robert Ridgway. This will probably appear in the course of a few months.—The scientific tendency of the age, manifested in the continual springing up of new associations in different parts of the country, receives an additional illustration in the estabiishment of the Natural History Society of Marquette, Michigan, whtch was organised during the month of December, under the presidency of Dr. Hewitt.

* Communicated by the Scientific Editor of *Harper's Weekly*.

ON THE CARPAL AND TARSAL BONES OF BIRDS *

THE author stated that he had followed with great interest the work of Huxley, Cope, Morse, and others, in tracing out the ornithic characters in the Dinosauria. While following these relations he had noticed a marked difference in the characters of the carpus and tarsus of the two classes. It seemed strange that a group of bones so persistent in the reptiles as well as in the mammalia should be so obscure or wanting in birds. Owen objects to the term tarso-metatarse, as he believes the existence of a tarsus has not been demonstrated. W. K. Parker, in 1861, on the osteology of Balæniceps, questions if the lower articular portion of the tibia is not the homologue of the mammalian astragalus and not an epiphysis. Gegenbaur has now shown that in one stage of the young bird there is a proximal tarsal ossicle, and a distal tarsal ossicle, the first one anchylosing with the tibia, the distal one likewise anchylosing with the metatarse. Thus, the term tarso-metatarse is quite proper. While this was a great step toward a proper understanding of these parts, Mr. Morse believed that a nearer relation would be found in the discovery of another proximal tarsal bone. In those reptiles he had examined, whatever the number of tarsal bones, there were always in the proximal series one corresponding to the tibia, and another corresponding to the fibula. He had found this feature in birds. In studying the embryos of the eave swallow, bank swallow, king bird, sand piper, blackbird, cow blackbird, bluebird, chirping sparrow, yellow warbler, and Wilson's thrush, he had found three distinct tarsal bones, two in the proximal series answering to the tibia and fibula, and one in the distal series. The first two early anchylose, and present an hour-glass-shaped articular surface as Prof. Cope has described in the astragalus of Lælaps. The final anchylosis of these conjoined ossicles with the tibia, formed the bicondylar trochlea so peculiar to the distal end of a bird's tibia. The distal tarsal ossicle became united with the proximal ends of the metatarse, as has been shown. In the carpus he had found four perfectly distinct ossicles, the distal carpal bones becoming united to the base of the mid and outer metacarpals, the other two remaining free, though the ulnar carpal in some cases anchylosed with the ulna. In the king bird and yellow warbler, he had found a fifth carpal on the radial side.

* Abstract of paper by Prof. E. S. Morse, read at the Indianapolis meeting of the American Association for the Advancement of Science. Reprinted from the *American Naturalist*.

SCIENTIFIC SERIALS

THE *Journal of Anatomy and Physiology*, Second series. No. ix., November 1871.—The first article in this number is by Prof. Humphry, "On the Anatomy of the Muscles and Nerves of *Cryptobranchus Japonicus*," an animal which has been only rarely dissected. The muscular system presents no points of great peculiarity or interest, resembling very closely that of other *Urodela*. With respect to the nerves, no trace of the third, fourth, or sixth cranial could be found in either orbit, though the third and fourth, both of very small size, were found in the cranial cavity; previous dissectors had described the sixth as a branch from the fifth in the orbit, but this could not be found in the present specimen. The three divisions of the fifth cranial nerve were distinct, but the ophthalmic and supra maxiliary left the skull by a common foramen. The vagus gave off branches answering to the spinal accessory, and also a large lateral nerve which ran back along the body, giving off no branches until it reached the great lateral muscles of the tail, and in that differing from the corresponding nerve of fishes. The spinal nerves resembled in most points those of man very closely, the brachial and crural plexuses were, however, much more simple, which Prof. Humphry thinks is associated with a less perfect specialisation of the action of the limb muscles; and below knee and elbow the course of the nerve trunks in the fore and hind limbs was almost identical.—The next paper is by Prof. Flower, "On the composition of the Carpus of the Dog." The os centrale had previously never been recognised in Carnivora, and both Cuvier and Owen regarded it, in those animals in which it is present, as a dismemberment of some element of the carpus; Gegenbaur, however, regarded it as itself a true carpal element, though never able to discover the state of things in those cases in which it was absent. However, in the skeleton of a dog six weeks old, Prof. Flower finds that the so-called scapholunar bone consists of three distinct pieces, viz., a distinct scaphoid and lunar, and a third piece evidently answering to the os centrale; thus confirming the view that the latter is a true primitive carpal element.—Dr. Messenger Bradley gives an account of the brain of an idiot, who during life could taste and hear well, and could repeat a few words in a parrot-like manner, but was congenitally blind, and never recognised any one, or, although not paralysed, made any attempt at locomotion. His bones were extremely fragile, fracturing invariably if he jerked a limb against the bed. The brain when removed weighed twenty-eight ounces: most of the fissures and lobes of the cerebrum were present, but (notwithstanding the small size of the hemispheres) were relatively small. The island of Reil was small and very simple. The corpora quadrigemina were very small, which is interesting, taken in connection with his blindness. The cerebellum was relatively large, the vermiform process was imperfect, the pyramid and short commissure entirely absent, and the left hemisphere considerably lighter than the right. The bones throughout the body when examined microscopically were found permeated with oil drops and granular matter, but when these were washed away normal bone structure could be made out except an unusually large size of the Haversian canals.—Prof. Young contributes some facts in the anatomy of the shoulder girdle of birds, showing that the only movement of the humerus in flight which is anatomically possible, is that in a figure of eight.—A short description by Mr. Watson, of the digestive, circulatory, and respiratory organs of the Indian elephant, follows.—The action of the chlorides of platinum, iridium, and palladium when introduced into the blood of dogs is the subject of an interesting paper by Dr. Blake, of San Francisco.—Prof. Turner describes the variations of nerves in the human body which he has lately met with, and then follows a paper by Prof. Struthers on the Great Fin Whale, the most interesting points being a careful account of the muscles of the fore-limb, helping to clear up some points as to the homologies of the bones; and the discovery, for the first time in this species, of a bony rudiment of the femur, though Prof. Flower had previously noticed a cartilaginous one.—Mr. Garrod gives some observations made on himself showing that the exposure of the nude body to a temperature below 70° F. causes a rise in the internal temperature of the body; which is greater the lower the temperature of the surrounding air down to 45°, the lowest point at which observations have been made. This he attributes to a contraction of the cutaneous vessels driving the blood inwards, and also lessening the conducting power of the skin. Exposure to a temperature of 70° causes no rise.—A detailed description of the anatomy of the Malayan Tapir, by Dr. Murie, and of the muscles and nerves of the chimpanzee and anubis, by Mr. Champneys, do not admit of a short abstract being given of either of them.—The Report of the Progress of Physiology, by Drs. Brunton and Ferrier, is very full, and contains short accounts of many matters of great interest. The anatomy report is postponed.

SOCIETIES AND ACADEMIES

London

Geological Society, January 24.—Mr. Joseph Prestwich, F.R.S., president, in the chair.—The following communications were read:—(1) "On the Foraminifera of the Family Rotalinæ (Carpenter) found in the Cretaceous Formations, with Notes on their Tertiary and Recent Representatives," by Prof. T. Rupert Jones, and Mr. W. K. Parker, F.R.S. The authors enumerated the Rotalinæ which have been found in the Cretaceous rocks of Europe, and showed by tabular synopses the range of the species and notable varieties in the different formations of the Cretaceous system. For the comparison of the Tertiary Rotalinæ with those of the Cretaceous period the following Tertiary formations were selected:—the Kessenberg beds in the Northern Alps, the Paris Tertiaries, the London Clay, the Tertiary beds of the Vienna Basin, and the English and Antwerp Crags. The authors also enumerated the recent Foraminifera of the Atlantic Ocean. The authors stated that of *Planorbulina* several species and important varieties of the compact, conical form occur throughout the Cretaceous series, and that those of the Nautiloid group are still more abundant. The plano-convex forms are represented throughout the series by *P.* (*Truncatulina*) *lobatula*; but the flat concentric growths had not yet come in. *Planorbulina* extends down to the Lias and Trias. *Pulvinulina repanda* is feebly represented in the uppermost Chalk, but forms of the "*Menardii*" group abound throughout the series. Species of the "*elegans*" group are peculiarly characteristic of the Gault, and some of the "*Schreibersii*" group are scattered throughout. These two groups extend far back in the Secondary period. The typical *Rotalia Beccarii* is not a Cretaceous form, but the nearly allied *R. umbilicata* is common. *Tinoporus* and *Patellina* occur at several stages; *Calcarina* only in the Upper Chalk. The above-mentioned types are for the most part still living, but the "*auricula*" group of *Pulvinulina* is wanting in the Cretaceous series, as also are *Spirillina* and *Cymbalopora*, except that the latter occurs in the Maestricht Chalk. *Discorbina* and *Calcarina* make their first appearance in the uppermost Chalk. The chief distinction between the Cretaceous and the existing Rotalinæ was said to consist in the progressively increasing number of modifications. The authors concluded by disputing the propriety of regarding the Atlantic ooze as homologous with the Chalk. The president suggested the possibility of some of the minute Foraminifera being transported fossils derived from earlier beds than those in which they are now found. Dr. Carpenter observed that the mode of examination to be adopted with Foraminifera was different in character from that which was applicable to higher organisms. The range in variation was so great that an imperfect examination of Nummulites had sufficed to make M. d'Archiac reduce the number of species by one half; and all the speaker's subsequent studies had impressed upon him the variety in form and in sculpturing of surface on individuals of the same species. When out of some thousands of specimens of *Operculina*, say, a dozen pronounced forms had been selected, such as by themselves seemed well marked and distinct, it might turn out that after all there was but one species present with intermediate varieties connecting all these different forms. He thought the same held good with Rotalinæ, and that there were osculant forms which might connect, not only the species, but even the genera into which they had been subdivided. This fact had an important bearing on their genetic succession, especially as it appeared that some of the best-marked types were due to the conditions under which they lived. The temperature in tropical seas differed in accordance with the depth so much, that when 2,000 fathoms were reached a degree of cold was attained such as was to be found in high latitudes; and in consequence the deep-sea forms in tropical latitudes assumed the dwarfed character of those in shallower seas and nearer the pole. He suggested caution in drawing inferences from forms so subject to modification, both spontaneous and due to the depth of the sea, especially as connected with abundance of food. Prof. Ramsay remarked that geologists would be pleased to find Foraminifera exhibiting, like other organisms, changes in some degree connected with the lapse of time. These low forms, however, could hardly afford criteria for judging of the age of geological formations, while at the same time such ample means were afforded by the higher organisms for coming to a conclusion. He cited, for instance, the Cephalopoda, as proving how different were the more important forms of marine life in Cretaceous times from those of the present day. He thought that no one who had thoroughly studied the forms of ancient life would be led to ignore the differences they presented, as a whole, from those now existing.—Prof. Jones, in reply, observed that the question of whether the Foraminifera in a given bed were derived or not was to be solved partly by their condition and partly by their relative proportions, but that in most cases sufficient data existed on which to found a judgment. He agreed with Dr. Carpenter as to the existence of extreme modifications, and it had been his object to ignore such as seem due to ordinary and local causes, and to group the forms in accordance with certain characteristics. Whether the classification was right or wrong, it was necessary, for the sake of increasing knowledge, that fossils of this kind should be arranged in groups; and whether these were to be regarded as truly generic was a minor consideration. In forming their types and subtypes the authors had carefully avoided minor differences; but they still thought that the modifications which were capable of being substantiated were significant of a great lapse of time. A variation once established never returned completely to the original type. In *Globigerina*, he stated that there were in Cretaceous times 8 forms, in Tertiary 12, at the present time 14; and these modifications he regarded as equivalent to the specific changes in higher animals.—(2.) "On the Infralias in Yorkshire," by the Rev. J. F. Blake. The Infralias, *i.e.*, the zones of *Ammonites planorbis* and *Am. angulatus*, have been recorded hitherto only from Redcar, to the beds at which place the author referred; but the chief object of the paper was to describe some sections at Cliff, near Market Weighton, where these and lower beds are well exposed, and have yielded a numerous suite of fossils. He considered, however, that these beds did not belong to the typical Yorkshire area, but were the thin end of the series which stretches across England. He supposed there had been a barrier in Carboniferous times, which had separated the coal-fields of Yorkshire and Durham, prevented the continuity of the Permian beds, and curved round the secondary rocks to the north of it, to form the real Yorkshire basin, while these beds at Cliff were immediately to the south of it. The sections described were six in number, the first pit yielding the great majority of the fossils, and the third showing best the succession of the beds. The fossils could be mostly identified with known forms, and showed a striking similarity to the Hettangian fauna. In all the clays of the Infralias Foraminifera were numerous and varied. The section in pit No. 3 showed, commencing at the top:—1 Stone bed with *Am. angulatus* (the fossiliferous bed of pit No. 1). 2. Thick clays, with bands of stone characterised by *Am. Johnstoni*. 3 One band of clay with *Am. planorbis*. 4. Thin-bedded stones and clays, some of them oyster-bands. 5. Clays without Foraminifera, and with impressions of *Anatina* (White Lias). The *Avicula contorta* series is not reached, nor are there any signs of the bone-bed, as the junction with the Keuper marls, which are found three miles off, is not seen. The paper was followed by references to the fossils mentioned, including the description of those that are considered new. Prof. Duncan remarked that English geologists had been backward in receiving the term Infralias, which he had suggested with respect to the Sutton Down beds some years ago, and the propriety of which was shown by the term having been applied to the same beds by French geologists at a still earlier period. As to the White Lias, he regarded it as a mere local deposit, not to be found out of England. He traced the existence of the Infralias from Luxembourg through France into South Wales, where corals were abundant. In Yorkshire, though one fine coral had been found, the Ammonites seemed to point to a difference in condition. Mr. Hughes remarked that the lithological character of the beds, as described by the author, did not agree with that of the Infralias in the S W. of England or the N. of Italy, and that the palæontological evidence which had been laid before the Society did not confirm the view that they were Infralias, the author having especially noticed the absence of *Avicula contorta* where he expected that it should occur. Also, by reference to the author's section, Mr. Hughes pointed out that below what he described as Infralias he drew other beds which were not Trias, the author having explained that some beds which had been called Trias were only stained beds of Liassic age.—The Rev. J. F. Blake, in reply, acknowledged the difference between the Yorkshire section and those of the neighbourhood of Bath, but insisted on the similarity of the fossils.

Linnean Society, February 1.—Dr. J. D. Hooker, F.R.S., vice-president, in the chair. "On the Classification and Distribution of Compositæ," by G. Bentham, F.R.S., president. The order Compositæ, or Synantheræ, is remarkable, not only from its enormous size, but from its extremely natural and well-marked characters, there being not a single instance in which it is doubtful whether a plant should be referred to this order or not. All the essential characters of the andrœcium, pistil, structure of the fruit, structure of the seed, and inflorescence are absolutely constant throughout the 10,000 species comprised within it. This very fact, however, renders its sub-division into tribes and genera a matter of extreme difficulty, the systematist being compelled to adopt characters as generic, which, in other orders, would hardly be considered as even specific. After briefly reviewing the labours of Linnæus, Jussieu, Cassini, Don, Lessing, Schultz Bipontinus, De Candolle, Asa Gray, Hildebrand, Delpino, and other botanists who have paid special attention to this subject, the author spoke of the special opportunities he had had in the preparation of the "Genera Plantarum," in conjunction with Dr. Hooker, for examining himself nearly the whole of the genera comprised within the limits of the order, and then proceeded to the consideration of the value of the several characters available for the distinction of genera and tribes: 1. Sexual differences in the florets contained in the capitulum, which may either have both the male and the female organs perfect, or the female organs sterile in the central florets, or the male organs or both sets abortive or wanting in the marginal florets. These distinctions formed the basis of Linnæus's order, but have been considered of less and less importance by subsequent writers. The author finds them sometimes constant in large genera or subtribes, sometimes variable in closely-allied species. 2. Di- and tri-morphism, very rare in Compositæ, except as connected with sexual differences. 3. Differences in the pistil. The ovary and ovule are uniform throughout the order, and the style nearly so when it acts only as the female organ; but the modifications of its extremity, in so far as they are destined to sweep the pollen out of the anther tube, supply some of the most important differential characters for genera, and even for tribes. These characters, first brought forward by Cassini, formed the basis of Lessing's and De Candolle's classifications, but have in many instances been too implicitly relied upon. 4. Differences in the fruit and its pappus. The structure of the fruit and seed is uniform in the order, but the outer shape of the achene and its ribs, angles, or wings have been made much use of, especially by Schultz Bipontinus, and the pappus presents such infinite variations so easily observed that it has been applied to the distinction of innumerable genera often very artificial. 5. Differences in the andrœcium. The male organs are as uniform in their structure, number, insertion, and relative position as other essential parts of the flower, but appendages often observed at the base of the anthers, usually called tails, having no apparent function to perform, are, however, so constant in their presence or absence, as to supply most valuable tribual characters. 6. Differences in the corolla, which, though uniform as to essential points in its structure and position, shows modifications of the limo or lamina, which are of great importance as distinctive characters: (1) the pentamerous ligula of Cichoraceæ truncate at the end with five short equal teeth; (2) the regular tubular corolla, either slender and equal to the end, or expanded upwards into an equally toothed or lobed limb; (3) the bilabiate corolla, in which the two inner lobes forming the inner lip are usually shorter or smaller or more deeply divided than the three outer; and (4) the trimerous ligulate corolla forming the ray of most heterogamous capitula, in which the two inner lobes are deficient or rarely represented by minute slender teeth. 7. Differences in the calyx. This organ is so reduced as to supply no characters except such as are derived from the ribs and pappus of the ripe fruit, and are considered under that head. 8. Differences in the ultimate inflorescence and bracts, *i.e.*, in the capitulum, its involucre, receptacle, and paleæ, the modifications of which acquire a great degree of constancy and consequent importance in the distinction of genera or even of tribes, as might be expected from the increased functions imposed upon them by the abortion of the calyx. 9. Differences in foliage. There is no type of foliage in Compositæ which may not be found in several other orders, although the leaves are never compound with articulate leaflets, but the opposition or alternation of the leaves are of great assistance as characters of some of the tribes, differences in habit, stature, and general inflorescence, rarely giving absolute characters excepting where numerous capitula are crowded on a common receptacle into a kind of compound capitulum. 10. Differences in geographical distribution, which, if considered in as far as it may be attributed to origin independently of climatological considerations and modern colonisations, may be of great use in determining natural genera. In the portion of the paper now laid before the society and read in abstract the author enters into considerable detail with regard to the above several series of available characters, and concludes with a summary of the thirteen tribes which he has adopted for the "Genera Plantarum," reserving for a future meeting the second part relating to the geographical distribution of the order.

Chemical Society, February 1.—Dr. Frankland, F.R.S., president, in the chair.—When the ordinary business of the Society had been transacted, a note "On the crystalline principle of Barbadoes aloes" was read by the author, Dr. W. A. Tilden, in which he described anew derivative of aloin. This is chloraloin, which crystallises from boiling-water in yellow silky needles, bearing considerable resemblance to the corresponding bromime compound bromaloin.—Dr. C. R. A. Wright then read an elaborate paper "On the relations between the atomic hypothesis and the condensed symbolic expression of chemical facts and changes known as dissected (structural) formulæ," in the first part of which he showed the possibility of expressing chemical facts without reference to the atomic theory; and in the second examined how far these facts could be accounted for by the atomic hypothesis. A long and very interesting discussion ensued, in which some of the speakers advocated the employment of the atomic theory to a greater or less extent, as promoting the progress of chemical science, whilst others desired its abolition.

PARIS

Academy of Sciences, January 29.—A note by M. J. Boussinesq on the integration of the equation with partial derivatives of the isostatic cylinders produced in a homogeneous and ductile solid, was presented by M. de Saint-Venant.—M. A. Ledieu read a note containing objections to the marine gyroscope proposed by M. E. Dubois at the meeting of January 22.—M. J. A. Serret presented a memoir on the pendulum of Léon Foucault.—M. Jamin presented a note by MM. A. Cornu and E. Mercadier on melodic musical intervals, confirmatory of their previous results.—A note by M. J. Violle on the induction currents produced in the polar masses of Foucault's apparatus was read.—M. Daubrée presented a note by M. Peslin on the bands of the solar spectrum, in which the author indicates a very simple relation between the most important bands.—M. Delaunay communicated a note by M. Fron on the prevision of certain earthquakes.—A further note by Father Secchi, on the temperature of the sun, was read, in which the author still maintains his opinion as to the enormous temperature of that body.—A note by M. E. Liais on absolute meridian observations in the low latitudes of the southern hemisphere was read, with especial reference to the observatory of Rio de Janeiro. Upon this paper MM. Le Verrier and Laugier made some remarks.—M. S. Meunier communicated a paper on the methods which concur in demonstrating the stratigraphy of Meteorites.—M. Delaunay made some remarks upon the note presented to the last meeting of the Academy by M. Renou with regard to the Meteorological Manual of the Paris Observatory for 1872, and presented to the Academy the first number of a monthly Meteorological Bulletin published by the Observatory.—M. P. Thenard presented some observations upon the preservation of wines by heating, in connection with a recent note by M. Balard. He claimed the discovery of the action of heat upon wines for MM. Appert and de Verguette.—M. Chevreul read a note upon the investigations upon dyeing carried out by M. Paul Havrez; MM. Montefeore-Levi and Kunzel presented a reply to a claim of priority made by MM. de Ruolz and Fontenay with respect to the discovery of phosphorus bronze and its employment in the manufacture of ordnance; M. Wurtz presented a note by M. L. C. Coppet on the supersaturation of the solution of chloride of sodium; and M. C. Bernard communicated a note on the analysis of the gases of the blood by MM. A. Estor and C. Saint-Pierre.—The lively discussion commenced two or three meetings ago on fermentation and heterogeny was reopened by a long paper on fermentations by M. E. Fremy, and continued by MM. Balard and Wurtz.—M. C. Martins read an important paper on the normal position of the hand in man and in the vertebrate series.

BOOKS RECEIVED

ENGLISH.—The Highlands of Central India: Capt. J. Forsyth (Longmans).—Rude Stone Monuments in all Countries: J. Fergusson (J. Murray).—Hints and Facts on the Origin of Man: P. Melia (Longmans).—A Dictionary of Chemistry, Supplement: H. Watts (Longmans).—Gandeamus: Humorous Poems translated from the German by C. G. Leland (Trübner).—Geometrical Conic Sections: J. S. Jackson (Macmillans).—Arithmetic in Theory and Practice: J. Brook Smith (Macmillans).—Worms, a Series of Lectures on Practical Helminthology: Dr. T. S. Cobbold (Churchill).

FOREIGN.—Medizinische Jahrbücher, 1871; Heft 4: S. Stricker.—Mittheilungen der Naturforschenden Gesellschaft in Berne, 1870.—Nouveaux Mémoires de la Société Helvétique des Sciences Naturelles en Berne, Vol. xxiv.—Beiträge zur Kritik der Darwinsche Lehre: Dr. E. Askenasy.

DIARY

THURSDAY, FEBRUARY 8.

ROYAL SOCIETY, at 8.30.—Experiments concerning the Evolution of Life from Lifeless Matter: W. N. Hartley.—Experiments on the Directive Power of Large Steel Magnets, of Bars of Magnetised Soft Iron, and of Galvanic Coils, in their Action on External Small Magnets; with Appendix, containing an Investigation of the Attraction of a Galvanic Coil on a Small Magnetic Mass: James Stuart, M.A.

SOCIETY OF ANTIQUARIES, 8.30.—On the Hunnebedden of Holland: A. W. Franks.—On an Inscribed Saxon Knife: J. Evans, F.R.S.—On a Sword Found in Spain: Col. Lane Fox.

MATHEMATICAL SOCIETY, at 8.—On the Factors of the Differences of Powers, with especial reference to a theorem of Fermat's: W. Barrett Davis.—On an Algebraical Form and the Geometry of its dual connection with a polygon, plane, or spherical: T. Cotterill.

FRIDAY, FEBRUARY 9.

ASTRONOMICAL SOCIETY, at 3.—Anniversary Meeting.

ROYAL INSTITUTION, at 3.—On Sleep: Prof. Humphry, F.R.S.

QUEKETT MICROSCOPICAL CLUB, at 8.

SATURDAY, FEBRUARY 10.

ROYAL INSTITUTION, at 3.—On the Theatre in Shakespeare's Time: Wm. B. Donne.

SUNDAY, FEBRUARY 11.

SUNDAY LECTURE SOCIETY, at 4.—On the Skeleton of the Higher Vertebrates: Dr. T. S. Cobbold, F.R.S.

MONDAY, FEBRUARY 12.

GEOGRAPHICAL SOCIETY, at 8.30

LONDON INSTITUTION, at 4.—Elementary Chemistry: Prof. Odling, F.R.S.

TUESDAY, FEBRUARY 13.

ROYAL INSTITUTION, at 3.—On the Circulatory and Nervous Systems: Dr. W. Rutherford, F.R.S.E.

PHOTOGRAPHIC SOCIETY, at 8.—Anniversary Meeting.—On a Comparison of the Different Modes of Plate Cleaning: Dr Anthony. The Niepce de St. Victor specimens will be shown.

WEDNESDAY, FEBRUARY 14.

SOCIETY OF ARTS, at 8.—On the Study of Economic Botany: J. Collins.

THURSDAY, FEBRUARY 15.

ROYAL INSTITUTION, at 3.—On the Chemistry of Alkalies and Alkali Manufacture; Prof Odling, F.R.S.

ROYAL SOCIETY, at 8.30.

SOCIETY OF ANTIQUARIES, at 8.30.

LINNEAN SOCIETY, at 8.—On a Chinese Artichoke Gall: A. Müller, F.L.S.—On the Habits, Structure, &c., of the three-banded Armadillo: Dr. J. Murie, F.L.S.—Comparative Geographical Distribution of Butterflies and Birds: W. F. Kirby.

CHEMICAL SOCIETY, at 8.

CONTENTS

NOTICE

We beg leave to state that we decline to return rejected communications, and to this rule we can make no exception. Communications respecting Subscriptions or Advertisements must be addressed to the Publishers, NOT *to the Editor.*

THURSDAY, FEBRUARY 15, 1872

THE POSITION OF THE CENTRE OF GRAVITY IN INSECTS

MY researches on the conditions of equilibrium in living beings, have led me to the conclusion that a complete knowledge of them is only possible when the position of the centre of gravity in each is known.

At present the knowledge of the mechanism of the Articulata has made considerable progress, thanks to the use of processes of investigation borrowed from Physics; and it appeared to me, that there would be real utility in the description of an easy method for the discovery of the centre of gravity in the Articulata, and the results which its application to insects has allowed me to obtain. I am, unfortunately, unable in a simple *résumé* to give a description of the instrument which I have employed. A very short description without an engraving is necessarily obscure, and loses all utility. I shall simply say that the instrument in question is a reproduction, on a small scale and with some improvements, of that which Barelli has invented for the determination of the centre of gravity in man. And with regard to the results of my experiments, I must also renounce the idea of giving them in the form they assumed in my work, that is, without the considerable number of figures combined in tables. I shall confine myself to the enunciation of the general conclusions I have been able to deduce, and to supporting them, as required, by several examples.

(1.) The centre of gravity in an insect is situated in the vertical and medial plane which passes along the longitudinal axis of the body.

(2.) It occupies a position almost identical in insects of the same species, the same sex, and in the same attitude.

(3.) The exterior form of the body rarely permits the determination of the exact position of the centre of gravity *without experiment*. I shall cite the results with which the family of Odonates have furnished me as examples. All its representatives have nearly the same exterior aspect; and yet, notwithstanding this *quasi* identity of structure, I have found in the relative position of the centre of gravity the following differences:—

Agrion puella,	female	1st third of the 3rd abdominal ring.
„ sanguinea	,,	Posterior border of the 2nd abdominal ring.
Libellula conspurcata	,,	,, ,, of metathorax.
Libellula vulgata	,,	Groove between thorax and abdomen.
Æschna grandis	,,	Middle of 2nd abdominal ring.

(4.) The centre of gravity does not occupy the same position in the two sexes of one species. It is sometimes less and sometimes more to the rear in the females than in the males; and its situation depends on the relations existing between the different dimensions of the individuals. One would suppose that the centre of gravity would always be situated further back in females than in males, as the abdomen of the former is in general more bulky than that of the males. During the metamorphosis from larva to perfect insect, the relative centre of gravity approaches the head; the absolute centre, on the contrary, recedes from it.* This apparent contradiction is very easily explained; the thorax of the larva is generally much reduced, and the abdominal rings numerous. In the perfect insect the thorax has acquired considerable dimensions, and the number of abdominal rings has diminished. The thorax, prolonging itself more to the rear, has approached, so to speak, the centre of gravity, which also remains in the medial region of the body; and the abdomen shortening itself, the distance of its extremity from the point in question diminishes.

(5.) While standing, the centre of gravity is placed at the base of the abdomen, or in the posterior portion of the thorax, and usually in the centre of the length of the body.

(6.) When an insect is walking, its centre of gravity undergoes constant displacement about a mean point, but the distances of displacement are too small to be measured. In fact, if experiments are made with leaping Orthoptera, grass-hoppers, or Acridians, it is ascertained that the displacement of their enormous posterior members leads to changes in the situation of the centre of gravity, but these changes are so small that one arrives at the conclusion that it is impossible to measure them in ordinary insects.

(7.) The displacement of the centre of gravity, when the insect passes from the state of repose to that of flight, cannot be ascertained except with those species where the wings lie folded on the back when in a state of repose. The displacement is horizontal and from back to front.

For example, in the following species the displacement is:—

Dytiscus dimidiatus .	0·045	of the total length of the body.
Hydrophilus piceus .	0·028	,, ,,
Melolontha vulgaris .	0·053	,, ,,
Notonecta glauca .	0·032	,, ,,
Locusta viridissima .	0·054	,, ,,
Vespa vulgaris . .	0·023	,, ,,
Plusia gamma . .	0·025	,, ,,
Eristalis tenax . .	0·037	,, ,,

(8.) During active flight the centre of gravity oscillates continually about a mean position, which corresponds with the instants when the extremities of the wings pass the point of crossing of the 8-shaped curve which they describe in the air.

(9.) In aquatic insects the centre of gravity is nearer to the lower than to the upper surface of the body.

(10.) During swimming, the movements of the posterior feet, acting like oars, determine the oscillation of the centre of gravity around a mean position, which answers to the position of the swimming feet placed at the middle of their course. These oscillations of the centre of gravity lead to a continual swaying of the body about a transverse axis passing through the mean centre of gravity, and it ought, consequently, to follow a gently undulating course.

FELIX PLATEAU

* In my work I have called the relative position of the centre of gravity, its position as regards any portion of the body, as rings, hip *(hanche)*, &c.; and I have named the absolute position of the centre of gravity the number which is obtained by calculating the relation between the distance of the centre of gravity from the posterior extremity of the body and the total length of the animal. The quotients, 0·50, 0·67, for example, obtained in this manner, mean that the distance of the centre of gravity from the posterior extremity is $\frac{5}{10}$ or $\frac{67}{100}$ of the total length of the body. They show immediately, and independently of the form and thinness of the rings, whether the centre of gravity is in the centre of the insect, nearer to the head, or nearer to the anal orifice.

ON THE COLOURING-MATTERS FOUND IN FUNGI

DURING the last autumn I studied very carefully the colouring-matters occurring in such fungi as I was able to find in my own district. For the correct specific determination of many of them I am much inbebted to Mr. M. C. Cooke. Though the number examined was small, compared with the total number of British species, it was sufficient to lead to some interesting conclusions, and at the same time to point out the necessity of the examination of many more, which so far have not fallen under my notice. It therefore appears to me better to postpone the description of the individual colouring-matters until I can include a greater number, and compare them as a whole with those found in algæ, lichens, and other natural orders; but at the same time it may be well to give a short general account of some of the conclusions to which I have been led by the facts already observed.

So far I have been able to determine, by means of their optical and other properties, the existence of at least thirty distinct colouring-matters, and I feel persuaded that further examination will greatly extend the list. The majority of fungi contain at least two, and many contain several, different coloured substances. which can be separated, or perfectly well distinguished by other means. Closely allied species sometimes contain two or more in common, but very often one or more differ; whilst, at the same time, species belonging to somewhat widely separated genera are occasionally coloured by identical substances—for example, *Stereum hirsutum* and *Peziza aurantia*. Notwithstanding this, on the whole, there does appear to be a very decided connection between the general organisation of the plant and the particular kind of colouring-matter developed in it. There is, however, a considerable variation, even in different individuals of the same species—one developes much of one substance, and another of another—and thus we can easily understand why we often find them of very different colours, with every intermediate tint. The connection between general organisation and the coloured products is still more decidedly proved by comparing those met with in fungi with those found in other natural orders. As already mentioned, I have been able to distinguish at least thirty different kinds in fungi. Of these fully twenty have such well-marked optical characters that they could be recognised without difficulty in other plants. Some of the rest could not be easily distinguished when mixed with any of the modifications of tannic acid, and therefore nothing very positive can be said about their presence or absence in certain plants. Confining our attention to those about which there is no such doubt, I may say that only one is known to occur in any plant not a fungus. This is the fine orange colour, soluble in bi-sulphide of carbon, found in *Catocera viscosa*, which agrees perfectly with the more orange-coloured xanthophyll of some faded leaves, and of the exterior layer of the root of the carrot. The rest have hitherto been found only in various fungi. Neglecting individual differences, and taking into consideration only such general characters as are most useful in dividing colouring-matters into natural groups, there is also a remarkable difference between those of fungi and of some other natural orders. In several previous papers I have described how colouring-matters may be divided into three groups by the manner in which they are acted upon by sulphite of soda. In group A the detached absorption is removed, even when the solution contains free ammonia; in group B it is removed only when the solution contains excess of a weak acid, whilst group C is not changed in either case. So far, with only two exceptions, all the colouring-matters found in fungi belong to group C, even when they are blue or red, whereas with only two exceptions all the blue and red colouring-matters in the petals and leaves of flowering plants belong to groups A and B. A larger proportion of those of group C occurs in fruits, and a still larger in coloured woods, and thus the colouring-matters of fungi are much more closely related to those in woods than to those in flowers or leaves. As far as my observations extend, there is little or no specific agreement between the substances found in fungi and those in algæ and lichens. These latter orders are, however, closely related in this respect, for the greater part of the specific colouring-matters found in algæ occur in lichens, along with others similar to, but perhaps not identical with, those met with in fungi. Substances analogous to tannic acid are not of common occurrence, but are found in a few, as for example in *Agaricus sublateritius*, passing by oxidisation into a very insoluble brown colouring-matter, as in the case of faded leaves in autumn.

I am most willing to admit that much still remains to be learned; but, at the same time, these various facts appear to prove that there is some definite re tion between the organisation of plants and the chemical and optical characters of the compounds formed during their growth. If further research should establish this conclusion, one may perhaps indulge the hope that it will throw much light on certain questions in vegetable physiology.

H. C. SORBY.

SCHMIDT'S COMPARATIVE ANATOMY

Handbuch der Vergleichenden Anatomie. Eduard Oscar Schmidt. Sechste Auflage. (Jena, 1872.) Pp. 402.

IT is now more than twenty years since the first edition of this manual appeared. The plan is that of a companion to the author's lectures as Professor in the University of Gratz. It begins with a somewhat lengthy introduction on the general principles of Morphology and Physiology. In discussing the distinction between animals and plants, the author appositely quotes Buffon's dictum, "Il n'y a aucune différence absolument essentielle et générale entre les animaux et les végétaux." He also does full justice to the pre-eminent importance of Cuvier's labours in palæontology as well as in comparative anatomy and classification; but it is strange to find the name of Hunter conspicuous by its absence, even in a brief sketch of scientific biology. The lines which the author has chosen for the motto of his book,

Alle Gestalten sind ähnlich, und keine gleichet der andern,
Und so deutet der Chor auf ein geheimes Gesetz,

have, he believes, now received their solution. For Prof. Oscar Schmidt is a convert to the Darwinian creed. He says, "I have not freed myself from my old geological orthodoxy without much difficulty; and I am therefore pleased to have finished this new edition, in which the breach is complete." The contents of the book show that this is no half-hearted conversion.

It is divided into chapters, each of which treats of the anatomy of one of the primary groups of the animal kingdom, and the following table of contents, not given in the work itself, sufficiently indicates the principles on which the arrangement is made. 1, Protista and Pro tozoa ; 2, Cœlenterata ; 3, Echinodermata ; 4, Vermes ; 5, Arthropoda ; 6, Mollusca ; 7, Tunicata ; 8, Vertebrata. There is a good account of the Tunicata, or "Primeval Vertebrates" (Urwirbelthiere), from which the following is an extract.

After describing the characters of the ascidian larva as known before Kowalevsky's researches, the author continues :

"When the yelk-division has taken place, the ovum becomes first flat and then hollow on one side. A depression is thus formed, lined by two layers of cells (germinal laminæ). From the more superficial of these are developed the skin and nervous system, from the deeper the notochord, muscles, and alimentary canal, the muscles arising in a secondary layer of cells derived from the deeper original one. A dorsal groove bounded by two longitudinal folds becomes rapidly converted into a tube, the spinal canal, and this is immediately followed by formation of the tadpole-like tail. . . . The primitive digestive tract is the depression described above, which first closes and then forms a new opening on to the surface, the future mouth. The branchial sack, alimentary canal, and cloaca keep pace with the other organs (those, namely, which are derived from the superficial or serous layer), and when the larva becomes fixed, the latter either disappear altogether, like the notochord, or undergo retrograde change, like the nervous system. Thus the original likeness of the larva to the vertebrate type becomes lost."

Each chapter begins with a pretty full survey of the classes, orders, and other sub-divisions in the group of which it treats, with their several characters. In looking through these, some points appear worthy of note. No mention is made of Gregarinida. Sponges are kept among the Protozoa. The account of this class is not so full as might have been expected from the author's familiarity with it ; and with respect to its relation to the Cœlenterata, he merely remarks : "The early form of calcareous sponges, as well as the adult condition of certain genera, suggest a comparison with the Cœlenterate type." The Tunicata are removed from the worms, but Infusoria are added to this heterogeneous group, which, with Prof. Schmidt and most German naturalists, includes Bryozoa and Annulata, and probably contains as many distinct types as it did when Linnæus first defined it. Among the Arthropoda, *Limulus* is placed between the Amphipoda and Branchiopoda, as the type of the Crustacean order Pœcilopoda, while the Myriopoda do not appear at all. The Pteropoda form an order of the Gasteropoda, or (as they are inconveniently called) Cephalophora. The Vertebrata are divided into seven classes, *Amphioxus* and the Cyclostomi being both separated from Pisces, and made into independent primary divisions. Dipnoi appear as the highest order of fishes, separated from the Ganoids by Teleostei. Among the monodelphous mammals it is surprising to see the Sirenia still united in the same order with the true Cetacea ; while, on the other hand, the Pinnipedia are separated from the other Carnivora. The order Primates is broken up by the exclusion of *Homo* altogether, and the separation of the Lemurs (Prosimiæ). The author agrees with Haeckel and Gegenbaur in regarding this last order as the lowest of the Discophorous Mammalia, and as representing the ancestors of that group.

The morphological description in each of the above chapters embraces in most cases too wide a subject for the space allotted to it. Even in Gegenbaur's work one finds the Vertebrata, and still more the Vermes, too extensive for the anatomy of the whole group to be conveniently considered at one view, and, not only is Schmidt's style less concise, but is not illustrated by diagrams of any sort. The account of the vertebrate skull and of the specialisation of the somites of Arthropoda are instances of the deficiency referred to. Moreover, there is generally much too cursory an account of Embryology in comparison with other subjects. Indeed the development of Vertebrata is entirely omitted. The bibliography is evidently intended as a guide for students to the latest and most accurate works in each department, and for that purpose is fairly complete and well selected ; but there are some remarkable omissions, as of Mr. Parker's monograph on the shoulder girdle.

On the whole, this expanded syllabus is interesting, as a fresh instance of the progress which "the new zoology" is making abroad ; but its chief practical value will probably be to those who have the advantage of hearing the author's lectures. For them the wish with which he sends out the present edition will no doubt be amply fulfilled : "I hope that it will remain what it has been, a book for students, and will keep me in that active intercourse with young minds which ensures to a university teacher the freshness of thought, the imagination and openness to new ideas, which he can so ill afford to lose."

P. H. PYE SMITH

OUR BOOK SHELF

Text-Books of Science. Technical Arithmetic and Mensuration. By Charles W. Merrifield, F.R.S. (Longmans and Co.)

ARITHMETIC is a science as well as an art, and although the title of this book points solely to the art of arithmetic, we are bound to examine how far it has supported its right to a place in the series of text-books of science. The author says in the preface that "his experience has led him to believe that there is not much practical connection between successful teaching and logical sequence. The province of logic is to test ideas, not to impart them." We venture to demur entirely to these propositions, and to assert that each successive idea acquired by the pupil should be made to follow logically from the ideas previously existing in the mind, and that ideas which cannot stand the test of logic are, in an educational point of view, worthless.

We proceed to select a few instances of the disregard of logical sequence which the author considers compatible with successful teaching. (1.) The only definition of division given is the following :—"The object of division is to find how many times one number is contained in another. This number of times is called the quotient." A few pages further on is given the method of dividing *l. s. d.* by 365, and no hint is given that a different interpretation of division is required, viz., distribution of the money into so many parts, and not finding how many times the number 365 is contained in so much money, which is meaningless. (2.) Multiplication is said to be only a shorter method of "getting at" a particular kind of summation; but when we come to fractions we are told parenthetically that to multiply 7 pence by $\frac{3}{4}$ is to take three-quarters of it, without any discussion of the extension of the very meaning of the word multiplication that must be made before this interpretation is intelligible. In the same way we are told that one way of writing $289 \div 17$ is $\frac{289}{17}$ before the important truth has been impressed on the pupil that $\frac{1}{4}$ of $3 = \frac{3}{4}$ of 1, so that a symbol is used in two distinct senses before the identity of those senses has been shown. Throughout the book all difficulties are slurred over with half reasons, which are to be accepted by the pupil as whole ones. There is no attempt to lead the pupil to discover the rules for himself, or to trace the way in which they were originally arrived at; while at every turn we meet with such expressions as "evidently," "it is clear," "it is easy to see," "there is no mystery about decimals," as substitutes for the considerations which should really connect the new rules with the previous knowledge of the pupil. We might pick out specimens of this want of thoroughness from almost every page, but we must now turn to the art of arithmetic. The author says "care has been taken not to introduce anything in the way of mathematical invention or discovery," but surely care should also have been taken that the book should not be behind those already published in the brevity and completeness of the methods given. The rule for contracted multiplication is given, but its application to complicated calculations, such as practice, interest, stocks, &c., is left untouched. Contracted division is mentioned, but is not applied to the only case where it is indispensable, division by an interminable decimal. Decimalisation of money is taught, but by the old clumsy method; while a mode of approximate decimalisation is given, which is of no use if the result required be greater than the given amount. The latter portion of the book is devoted to mensuration, in which considerations that belong to the higher mathematics are described as evident, while all mention of the mensuration of rectangles and the difference between linear, square, and cubic feet is omitted. The book is below the level of the more advanced thought of the age, and unworthy to take rank in the series which contains "Miller's Inorganic Chemistry," and "Maxwell's Theory of Heat." H. A. N.

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 Total Eclipse as seen at Ootacamund

As a photographer and an ardent lover of science, I was of course anxious to catch an image or two of the eclipse, as a memorial of the grand scene of the morning of the 12th inst. Unfortunately for me, I read a short time ago an article by Mr. Brothers, of Manchester, on photographing eclipses, in which he says that it is useless to attempt a photograph of an eclipse without an equatorial stand to fix the camera to. Inquiries soon convinced me that in a primitive place like this it was impossible to get such a stand, and, in consequence, I gave up all idea of making an attempt at taking a photograph of the eclipse. The eclipse, however, no sooner commenced, than I laid aside my telescope and brought my camera into use to watch the progress of the eclipse, with the aid of a strong magnifying glass on the focusing screen of the camera. Here I saw that the progressive movement was scarcely perceptible; and that, with a short exposure of three seconds, I might get an image: though not perfectly sharp, yet it might show all details necessary for forming an interesting memorial of the eclipse.

I prepared one plate some time before totality, washed it, so that it would keep good for an hour or so, and some time after totality had commenced I exposed it for three seconds, and developed it some time after totality. As far as I know I exposed the plate 75 seconds after the commencement of totality, and the result was the plate I had the pleasure to hand to you, and the prints you saw in my place were printed from it. I may add that the plate was taken with a No. 6 D. Dallmeyer's lens, with the full opening and without a stop.

I will digress for a moment and express my surprise that I hear that the photographers of the Expedition parties obtained only five or six plates during totality, and that they gave exposures of about 15 seconds. If I had exposed my plate 15 seconds instead of three, I should have had nothing remaining but "a foggyghost." There must have been a great want of proper balance in their chemicals. Again, they seem to have been provided with a number of slides, or camera-backs, to hold a certain number of prepared plates. Now, had I known that I could obtain tolerable results without an equatorial stand, I would have presented you with a plate of at least eighteen different photos of totality. As my idea may be useful on a future occasion, I will shortly mention it. Photographers are in the habit of taking 2, 4, 6, or even eight cartes de visite photos on one single plate, and often also with one lens only, by an arrangement which we call repeating backs. A slight modification of the repeating back would have enabled me, or any one else, to take in quick succession, without loss of a second, at least 18 or 20 photos of the eclipse on one single plate. Different exposures might have been given to some or to all, and a treble number of photos to what has been obtained might have been secured without additional expense, and with less trouble. Any apparatus-maker would furnish such a slide for about 40*s.*, and as the operator would have only to pay attention to one slide and one plate, he would work with more certainty and comfort. If any one will say that one good photo of the eclipse was all that was needed, I must say that I differ from him. I observed most distinctly that the shape of the corona was undergoing a regular dissolving view process, and had not for two seconds exactly the same shape. Of this more hereafter.

The eventful day commenced here in the centre of Ooty, with the sky overcast both in the east and west with dark grey clouds. The camp of the Expedition on Dadabetta appeared enveloped in fog, so that the early prospects of the members stationed there must have been rather gloomy. In the town where I was the eclipse was visible from the very first commencement to the last moment; only once, for a few seconds, during the earliest stage, a small cloud obscured the sun. The grey, gloomy clouds receded (as if inspired with fear) as the eclipse advanced, in the direction of all four points of the compass, and the atmosphere between the earth and the sun and moon appeared of that absolute pure and blue hue, which can seldom be seen anywhere else except in high mountains. The scene as seen from the centre of Ooty was a grand sight. Every eye was turned to the east, the curious play of colours around the hidden sun, the general gloom or want of light, the ghostly shadows thrown by trees and other objects, the clear appearance of the stars in the west, combined with the solemn stillness (which we enjoyed at Ooty) not a breath nor a leaf moving, combined with all the other novelties of a total eclipse, formed a scene which is easier imagined than described. Chickens and fowls were of opinion that the day was ended, and retreated to their roosts, and many old people (natives of course) hid themselves in their huts, filled with anxious expectations of the things which were to come. The whole scene was still more enhanced by a large assembly of natives which had assembled near my place. Their exclamations of fear, of terror, and awe, were very amusing if not distracting. Now their fear showed itself by short and earnest incantations or prayers to a certain good deity to deliver the sun from the cruel fate of being swallowed by the large serpent, which, in their opinion, constantly pursues the sun, and overtakes it during an eclipse, and when only the interference of a good deity can save the sun from the fearful fate of having to undergo digestion in the belly of the terrible serpent. Some began to smite their breasts, and pluck their hair, accompanying these acts with exclamations which betrayed no small amount of mental agony about the probable fate of Father Sol; others watched in trembling silence, awaiting the end with fear, but coupled with hopes that the prophecies of the holy Brahmin might yet be fulfilled. Higher and higher rose the excitement, until the entire sun was engulphed in the terrible serpent's mouth. But it happened, as the Brahmins had foretold, a powerful good deity cut off with one blow the big serpent's head, and the sun, instead of going down the serpent's throat, emerged slowly in all his glory from the opposite side.

If science gains as much in knowledge by the observations made by the different eclipse parties as the wily Brahmins have gained by this late eclipse in money, then a great deal of knowledge will have been gained about all those mysterious phenomena by which Father Sol is still surrounded; for every village in India, as far as the country was affected by the eclipse, paid willing contributions to the Brahmins, that these holy men might use all their influence (by prayers, fastings, and offerings) with their deities in order to induce them to come to the rescue of the sun in the hour of his great danger and need; and I hear that the Brahmins hereabouts had an abundant harvest in money from the poor villagers, to whom they preached months before the great danger impending over the sun; and as these poor people are not yet bold enough to doubt a single word of these heaven-born Brahmins, they contributed to the best of their abilities to the Brahmins, in whose hands, as they believe, rests not only the fate of men but of the whole universe, as the Brahmins are the connecting links between men and the deities ruling this and other worlds. An event like the eclipse shows how much importance is to be attached to all the reports and writings about the great progress in enlightenment of the people of India. Knowledge does not reform their manners; many well-informed and educated natives performed all the superstitious ceremonies connected with the eclipse, with just as much zeal as the ignorant ryot, and many of those who talk to us Europeans about the folly of all the old superstitions, went back again, and performed their rites in the manner of their forefathers, fearing, that if they did not do so, Father Sol might be lost for good, and that we might have to end our remaining days in the constant gloom of starlight.

I have already mentioned that, as far as my observations go, I observed that the shape or form of the corona or glory which surrounded the eclipsed sun underwent changes in form even during the short space of two minutes; but you will easily see that an observer with no other means than an ordinary good telescope, his naked eye, and a photographic camera, was quite incompetent to draw any conclusion; suffice it therefore to say that the changes in the shape of the corona during totality can but be compared to the slow transformation of forms in a dissolving-view apparatus, or perhaps more correctly to the changes of form and shape we observe in isolated thin clouds. I will not express more of my opinion on the nature of the corona than that I believe it consists or partakes of the nature of shining, illuminated ether, perhaps somewhat of the same nature as the aurora borealis; why I think so will appear below.

About eight or ten seconds before totality ended, the moon appeared as if it had made a jerk (stumbled against something), and that jerk was accompanied by a tremendous flickering movement and momentary brightening up of the corona. This momentary phenomenon (for all passed in less or not more than one second) I am unable to describe more clearly, and I cannot compare it to anything except to those flickering movements and brightenings up observable in the aurora borealis. I spent one entire night during the winter of 1845 in watching a grand aurora borealis in North Germany, but had nearly forgotten all about it, but the above appearance in the corona towards the close of totality reminded me so forcibly of it that I hold that something similar is connected with the corona. I was watching the eclipse with a strong magnifier in the camera obscura, and three gentlemen near me used telescopes, and we all observed the same—I in the camera, and they with their telescopes—and the flickering caused us all to express some surprise, such as "Look! look!"

In the evening I had some conversation on the eclipse in general with the telegraph master, a very scientific gentleman, who, without my saying anything about the matter, told me that he observed such a phenomenon.

I think this is about all I can say, as the play and changes of colours which were visible are quite beyond my sphere; I can only say I saw them, but I do not remember their order and succession, nor changes.

In conclusion I must once more repeat that what I say must be taken for what it may be worth. I merely speak of the appearances without accounting, or being able to account, for them; and this will not be surprising when those who spend their lives in these studies can often only offer conjectures as to the real nature of these matters.

Ootacamund, Dec. 22, 1871 J. BOESINGER

Natural Science at Oxford

THE regulations relating to Natural Science at Oxford, reprinted in a recent number of NATURE,* will have considerable interest for those who follow the progress of such studies at the Universities.

The Natural Science School is one of the five "Final Schools." There are examinations which take place at the end of the University course; in any one or more of them it is open to candidates to seek for honours. Hitherto the Natural Science School has offered a threefold division of its subjects, namely, Biology, Physics, and Chemistry. A candidate was allowed to select any of these three divisions, and was expected to show, in the first place, a general acquaintance with the subject matter; and in the second, a detailed knowledge of some particular branch of it. The selection of the "special subject" was left entirely to the candidate, but the liberty of choice (in theory a most valuable one) was frequently altogether abused. The object was, apparently, in many cases, to turn the tables on the examiners, and by selecting matters likely to be out of the way of their reading, to make the examination almost fictitious. It is to remedy this that the new Board of Studies has laid down the scope of the general and special knowledge which will be required from candidates for the future.

The regulations at present published relate only to Biology. I venture to think that they by no means form such a philosophically-arranged course as might have been expected.

The first paragraph states the nature of the *general* knowledge which will be demanded. This is defined to consist of General and Comparative Anatomy, Human and Comparative Physiology and Physiological Chemistry, and the general philosophy of the subject. The books recommended are the best commentary on the meaning attached to these headings. The list certainly does not err from defect of copiousness, yet it is noticeable that although it contains all the common zoological text books, it does not include any distinctively botanical book whatever. I do not mean to say that some of the authors named in it do not touch on Botany, but this is so far accidental that they apparently owe their position on the list to their bearing on zoological matters. It appears to me therefore that the only conclusion which can be arrived at from the regulations is that by Biology is not intended General Biology, but only Biology from a zoological standpoint. This is, I think, to be regretted. A general acquaintance with the principal forms of vegetable life ought to form part of a comprehensive biological course, and should be required even of those who intend to devote their strength to the study of the animal economy alone.

The fifth paragraph appears to admit of Botany being taken up to a certain extent as an alternative subject, but this does not remedy its practical absence from the general scheme. I can see nothing in the regulations to preclude a candidate taking high honours in "Biology" who shall, for example, be quite ignorant of the anatomical differences between a cycad and a palm, or shall be quite unable to indicate any points of agreement between a mushroom and a mould. Any one in this predicament might perhaps excuse himself as a zoologist, but he can hardly be allowed to claim the whole of Biology as his province.

W. T. THISELTON DYER

Auroral Statistics

HAVING had already to answer many questions and calm some fears touching the recent brilliant aurora, and its prototype in October 1870, "when the Franco-German war was raging," I beg to send you some condensed statistical returns of auroral phenomena during the last eleven years, prepared and printed before the recent manifestation, and to be published in a few days, but as a part of a ponderous volume not likely to be generally accessible, viz., vol. xiii. of the "Edinburgh Astronomical Observations."

In that book I have endeavoured, amongst other subjects of professional duty, to exhibit the final mean results of nearly 7,025,000 meteorological observations of all kinds, by 55 observers of the Scottish Meteorological Society, spread over the country at as many stations; and, after a preliminary process of compression into 32 numerical tables, the quintessence of the whole appears on a single page, whereof the 28th line gives a numerical expression for each month of the year; combining the

* See NATURE, No. 118, p. 270.

number of times that aurora was visible with the extent of country over which it was observed, and the numbers stand thus:—

January	29·7
February	42·5
March	35·0
April	27·5
May	4·8
June	0·0
July	0·5
August	12·6
September	36·0
October	49·4
November	32·4
December	28·8

It will thus be seen that October and February are precisely the two months when brilliant auroras are most likely to be seen; and that of these two maxima of the annual cycle October has rather the advantage.

The lightning return, prepared on the same principle, is not uninstructive to be compared against the aurora; for, though both in its aërial altitude and actual numerical returns, lightning may be the very opposite of aurora, yet it exhibits a tendency to a similar double maximum in the course of the year; and not a few of the lightning storms of that second, or winter maximum, are locomotive "meteors," travelling from S.W. to N.E., and having undoubtedly a very wide-spread earth-influence and physical signification. The actual numbers are these:—

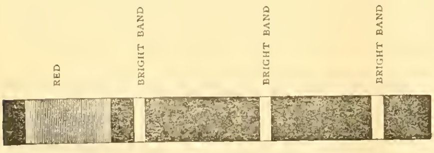
January	24·0
February	14·4
March	7·0
April	15·4
May	37·4
June	48·0
July	53·2
August	38·4
September	22·4
October	20·8
November	15·0
December	15·0

C. PIAZZI SMYTH

15, Royal Terrace, Edinburgh, Feb. 10

The Aurora of February 4

I WILL not attempt to describe the wonderfully gorgeous display of aurora which I witnessed on Sunday night, February 4. I merely wish to mention a circumstance connected with it which may have some interest. I was watching for the zodiacal light at about 5.30, and, having perceived faint traces of it, I presently saw some peculiar red clouds a little above it; from their rapid change of form I soon became aware that this was the light of an aurora. From that time, and from that spot, it spread rapidly; a bright white arch extending high overhead from W. to E., while a segment of blue sky stretched low down in the



S E. in the magnetic meridian, the space between being filled with brilliant colours. Shortly after this a radiating point became very striking, not in the zenith, but at one-third the distance from the Pleiades to Capella; and then the folds of gorgeous light-red, white, and faint green, interspersed with dark shading, spread from it, like a canopy, down on all sides except in the N.W. I never witnessed or read of such a display in these latitudes. With one of Browning's small star spectroscopes the spectrum consisted of a small portion of brilliant red, then a bright band rather close to it, and then two others beyond; the two latter being rather nearer together than the first and second; that at the more refrangible end being the faintest, and that near the red the strongest. I enclose a sketch showing the spectrum, the slit being wide open.

The maximum display was between 6.45 and 7 P.M; at 7.15 it was fading rapidly. Clouds covered the sky at 7.30, and some smart electric showers fell; still I could see that the display was going on; and at 11 P.M., in spite of dense clouds, the light was sufficient to enable me to read large print.

HENRY COOPER KEY

Stretton Rectory, Hereford, Feb. 6

ON Sunday evening 4th inst., a beautiful display of aurora was observed here (lat. 51° 26′ 0″ N., long. 0° 20′ 53″ W.). My attention was first directed to it at 6h. 4m. (G.M.T.) at which time there was a fiery glow over a considerable portion of the southern sky, much resembling the reflection of a distant conflagration. Shortly after, an almost complete auroral arch, of faint orange red light, similar to that at first observed, was noticed, extending from E., above and partly embracing δ, ϵ, and ζ Orionis, to W., its altitude (by estimation) at the centre being about 40°, and its extent something like 120°. For a short time this glow was most intense in S.S.E. at a great altitude, but the display attained its greatest intensity about 6h. 15m., when a number of rays or streamers of whitish blue and orange red light appeared as if radiating from a point near δ, α, and κ Persei. At 6h. 20m. nothing was observed but a widely diffused fiery glow, which must have continued more or less during the whole evening, as it was again observed by me at 8h. 25m.

JOHN JAMES HALL

Fulwell, near Twickenham

THERE was a fine display of the above phenomenon here on Sunday night, February 4. At five o'clock a muddy undefined redness made its appearance in the N.E. and W., especially in the former, which continued for some time without any very marked change. Towards half-past six the redness became more concentrated, gradually brightened, and finally became of a most intense brilliancy—indeed, so much so that it fairly baffles description, the landscape and the countenances of those standing near being visibly tinged. Streamers soon began to form, and shoot gradually upwards from the horizon in all directions from N.E. by S. to W., some intensely red, some very white, while others were of a greenish hue. The red and white being very brilliant, were finely intermingled, especially in a N.E. direction, while a muddy green prevailed chiefly in the S., and a reddish tinge in the W. By seven o'clock that rare phenomenon, a corona, was formed overhead, assuming a variety of shapes. The most curious part of the display (as far as my experience goes) was the entire absence up to this time of any streamers or coloured haze in a W. by N. to N.E. direction, the sky being cloudless, perfectly clear, and the stars shining with their usual brightness. On the formation of the corona a sheet of fan-shaped sea-green haze shot from it in a N. direction, spreading rapidly as it advanced, but did not proceed for more than 20°, when it suddenly disappeared. The streamers were remarkably steady throughout and straight, unlike those during the display of November 10 of last year, which were wave-like, rapid, and flickering. By half-past seven the entire sky had assumed a greenish tinge, wi h a reddish glow in some places, and a few resplendent beams of white light from the E. chiefly. At a quarter to eight red streamers became visible in a N. directi n, at a considerable elevation, resting on a greenish haze, itself emanating from a very indistinctly white arch spread across the N. At nine the sky was still tinged, and a streamer here and there visible, but by ten the display was over, as clouds had obscured the heavens. Although the red colours were so intense and deep, the stars could be distinctly seen through them, and when the streamers suddenly changed to white, &c., it was possible to see the time on a watch, though the night under ordinary circumstances would have been dark. A common dipping needle which marked 56° at noon changed to 45° before the aurora became visible. Barometer corrected and reduced, 29 748. Temperature, 37° at the time. Solar radiator during the day, 77°. A few shooting stars darted across the heavens in a south from east direction, mainly during the aurora. A wet night afterwards set in.

THOMAS FAWCETT

Blencowe School, Cumberland, Feb. 5

A VIEW of the magnificent aurora of Feb. 4 was much interrupted here by great masses of cloud, which frequently drifted over large tracts of the illuminated sky, and towards 8 o'clock collected and descended in a general downpour of rain. Nevertheless enough of it was seen to produce a very striking impression. It began to tinge the southern sky at a considerable altitude so early in the evening that I thought it must have been the reflection of a crimson sunset; nor was I undeceived till I had been to the other side of the house, where I found the western horizon glowing with amber light, in which was no trace of the expected ruddiness. Red continued throughout to be the prevailing hue, chiefly in great diffused masses, but occasionally broken up into filaments and streamers; there was, however, no absence of sheets and columns of the more usual pale green light. The clouds, chiefly heavy cumuli, assumed a strange aspect; sometimes, when opposite to the crimson illumination, reflecting a dull and sombre red, at others, when projected in front of it and enlightened from the other side by the twilight, or the green aurora, standing out in lurid and ghastly contrast. At one period the northern part of the sky, up to a great altitude, though clear and studded with stars, appeared at first sight almost like a black cloud from its contrast to the greenish white sheet which bordered it abruptly at a considerable height on the west; this again passing into crimson masses in the south, and sending out a whitish stream to meet another from the east, and form, probably, for a few moments, a complete bright ring, somewhat south of the zenith, of which, however, only one half could be seen from the post of observation. The light was so intense that even after it had been a good deal obscured by cloud, a large print might have been read without much difficulty. A miniature spectroscope (one of Browning's) brought out some interesting features. The usual yellowish green auroral line was distinct everywhere, and could be perceived even when the instrument was directed to masses of dense cloud; and as was observed by Birmingham on a former occasion, could be made out in the reflection from any suitable terrestrial object; white paper for example exhibited it very obviously. As shown in the brighter greenish patches in the sky, it remained visible even when the slit was so much contracted that the sodium band of a common fire would have been thinned down almost to its smallest breadth before extinction. Such a diminution of light, however, was fatal to the rest of the spectrum, which was a very remarkable one. With a wider slit a crimson band, bearing a fair amount of contraction, was perceptible in the brighter patches of that hue, with a dark interval between it and the principal green band. On the opposite side of that green band, beyond a second similar dark space, was a considerable extent of greenish or bluish light, quite decided, but so feeble as to leave it undecided whether it was of uniform brightness, or (as I suspected) compounded of contiguous bands; beyond this again was another dark space, leading on to a faintly luminous band, too dim to show colour, but which must have taken its place somewhere in the blue. This band, and the darkness adjacent to it on the less refrangible side, were each about as broad as the intensely vivid yellowish green stripe. Could the light have borne sufficient reduction, we should certainly have had three narrow bright bands in the red, green, and blue, the two latter being wide apart, with either a faint separate continuous spectrum, through part of the interval, or possibly several feeble lines, which the widening of the slit fused into one lengthened area.

The peculiarity, first noted I believe by Otto Struve, was very obvious, that even where the naked eye recognised the strongest and fullest crimson without a trace of green, the greenish yellow band in the spectroscope far exceeded, perhaps three or four times, the red line in visibility. This display was distinguished from almost all that I can recollect to have witnessed through many years, by its very feeble development in all the northern portion of the sky.

Hardwick Vicarage, Hay T. W. WEBB

WILL you kindly permit me to correct an error which crept into my letter of last Monday on the aurora. The words "western" and "north-eastern" in the 14th line should have read respectively "eastern" and "north-western." Allow me also to call attention to the present condition of Jupiter. On Thursday evening last the equatorial ochre-tinted belt was lighter in colour than I have seen it of late years, but much and distinctly mottled with light and dark clouding, two dark hanging spots on the upper edge, with adjoining elliptical bright patches, being conspicuous, while the lower dark madder-brown edge was very unequal, being swollen and thick about one-third to the right from the centre, and thinning off towards each end. The dark belt above the equatorial zone had two knots or thickenings of considerable size upon it, and the whole series of belts presented ragged and dentated edges, and, to use the apt phrase of a lady who saw them, had a "mountainous" look.

On occasional glimpses I more than suspected a general mottling of the whole surface of the planet, which, moreover, presented a dull appearance, the dark and light belts and spaces not being, as I thought, so well contrasted as usual. The poles were coloured as in ordinary, the upper one warm and ochreish, the lower slate grey. The instrument used was Browning's $8\frac{1}{4}$ reflector, full aperture, with inserting achromatic eye-piece 306. A transit of a satellite and its shadow added to the general effect.

Guildown, Guildford, Feb. 10 J. R. CAPRON

ON Sunday, the 4th of February, at 10 P.M., I observed the central point of the "corona" of the aurora visible that evening to be situated between ι, 64 and 65 Geminorum, in R.A. 7h. 20m. and N. decl. 28°. Our latitude is N. 50° 50′ 55″, and longitude E. 0° 32′ 50″.

The "corona" drifted away very slowly towards the E. against a slight E. wind blowing at the time.

Perhaps some of your contributors can calculate the aurora's height from the earth from the above notes, and let us know the result through your journal. J. E. H. P.

St. Leonard's, Sussex, Feb. 12

NOT wishing to trouble you with a long description of the aurora observed by so many on the evening of the 4th, I will confine myself to a few remarks. The spectrum of the brighter portions, viewed through a five-prism direct instrument, consisted generally of the four lines mentioned by Captain Maclear; but when the spectroscope was turned towards the brightest of the curved streamers forming that splendid red and pink star, which so suddenly burst forth at 7·25, some degrees south of the zenith, the relative intensity of the lines was completely changed, the red line becoming more strongly marked even than the green.

The fact that the green line can always be detected, even where the unassisted eye fails to notice any trace of auroral light, might suggest the advisability of a daily observation with a small hand spectroscope for those who are desirous of forming a complete list of all auroral phenomena. Magnetic disturbances are a sure guide in the case of grand manifestations of aurora; but might not a very slight aurora be observable without the magnets being sensibly affected?

On the evening of the 4th the magnetic storm commenced about 2 P.M., and was at its height from 4 to 9, though the magnets were not steady again until after sunrise the next morning. S. J. PERRY

Stonyhurst Observatory

I WRITE a very short account of the great aurora of February 4, as seen by me in the south-east of France, between Chambéry and Macon. It may be of some interest, as a brilliant aurora is very unusual in those latitudes, and this was quite comparable in brilliancy to the auroras of October 1870, and November 1871, which I witnessed in Scotland. The sunset was very clear and bright, but as the sunlight gradually faded, light fleecy clouds appeared in different parts of the sky, with the ruddy tints characteristic of the Northern Lights. As it became darker the redness increased in intensity and extent, overspreading a large portion of the sky, especially towards the zenith, and was streaked with bands of greenish white light. On the eastern horizon a well-defined arch of this pale green light was visible for some time, while underneath the arch the sky was so black that but for a large star shining in the centre of the blackness, I should have supposed that the darkness was due to a heavy cloud. There were, in fact, no true clouds at the time in the sky, and the large stars were everywhere visible amid the shifting masses of nebulous light, which at one instant seemed to be the ruddy reflection of a great fire, and at another to be lighted up by the rays of a full moon. Long streamers of red and green light seemed to shoot up towards the zenith from almost every point of the horizon at various times; but singularly enough there appeared to be fewer displays of this sort in the north than in any other quarter of the heavens. Being, however, in a railway carriage in motion, and with mountains on every side, the true horizon was not visible, and it was impossible to make very accurate observations. The rosy clouds remained long after the coruscations had died away, but the chief splendour was displayed for an hour and a half after sunset.

If the aurora of this spring was not more brilliant than those of the last two autumns, it was, I think, more remarkable for its sharp contrasts of colour, and for the peculiar "coal-sacks," or areas of blackness, which seemed to be actually a part of the aurora as much as the red or green light.

DAVID WEDDERBURN

I HAVE to correct an important error in my account of the aurora of the 4th, published by you on the 8th. I stated that it was finest between 6 and 7. At 9 it appeared to be fading, and I ceased to watch it; but I learned afterwards that it rekindled, and was at its highest between 9 and 10. The colour was still red, and the columns of light met near the zenith.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, Feb. 12

The Great Comet of 1861

THE following observation may interest your readers. It is taken from a volume entitled, "The Industrial Progress of New South Wales," published by authority of the Colonial Government. Under the head of Astronomical Progress is a paper by Mr. Tebbutt, in which he says that, while observing in Australia on the morning of July 1, 1861 (*i. e.*, really, in the afternoon before sunset of our June 30), he noticed the widening out of the branches of the tail of the comet then visible. He remarks that this observation is very interesting when taken in connection with the announcement made by Mr. Hind, that "it appears not only possible, but even probable, that in the course of June 30, 1861, the earth passed through the tail of the comet, at a distance of perhaps two thirds of its length from the nucleus."

There were at least two observers in England of what was probably the opposite effect of perspective (viz., the closing up of the branches of the tail) on the evening of June 30. The rapid, angular motion of one of the streamers was separately observed by Mr. George Williams, of Liverpool, and the Rev. T. W. Webb, of Hardwick, the latter of whom has given a detailed account of his observations in the "Monthly Notices of the Royal Astronomical Society," vol. xxii., p. 311. According to these observations, our actual passage through the streamers of the tail must have taken place about sunset on the evening of June 30.

A. C. RANYARD

ON LUMINOUS MATTER IN THE ATMOSPHERE

MUCH has lately been written and lectured on atoms, molecules, organic matter suspended in the air, effects of the light passing through the sky, abstracting its blue colour, and changing it into red. May I therefore be allowed to add some facts which I noticed during a long and careful observation of a hitherto almost unknown phenomenon to which my attention was drawn by chance.

Some years ago I had directed my excellent six-feet of Merz, Munich, towards the sun in order to draw the sun-spots in the camera-obscura. One day (April 27, 1863), when the sun had scarcely passed, and I was pushing the instrument to get its disc again in the field, I was astonished to perceive a mass of luminous little bodies, apparently coming from the sun, and passing altogether with great velocity towards the east. They brightened in a white and sparkling light, and were as numerous as stars; but as their velocity was much too great, and as they disappeared when I followed them to some distance from the sun, I was inclined to take them for little bodies floating in the atmosphere, and getting their light from the sun, an opinion which soon became stronger when I grew aware that I had to draw out the eye-piece some millimetres in order to get them quite clear and distinct. As I had never heard of the existence of any such bodies, I resolved to give notice to Dr. Wolf, Director of the Observatory at Zurich, who convinced himself of the strange phenomenon, and, encouraging me to persist in my investigations, told me that the late Sig. Capocci, on the Capodimonte Observatory at Naples, had mentioned these little bodies appearing to him under similar circumstances on May 11, 1845. Since that time Prof. Dr. Edward Heis, of Münster, Westphalia, in his "Wochenschrift fur Astronomie," 1869, March 24, also gave full corroboration to this fact. I therefore went on, and uniting the investigation to the daily labour of observing and drawing the sun-spots, my arrangement of the camera-obscura improved and ensured these results as well. Convinced of the importance of the phenomenon, I resolved to direct my whole attention to it, and to examine it thoroughly. I decided to find out not only the distance, the size, the shape, the frequency, the velocity, and the nature of the light of these little bodies, but also to take notice of their daily direction by comparing it with the simultaneous direction of winds and clouds. I continued my observations during a period of three years.

As I mentioned above, I was obliged to draw out the eye-piece of the telescope in order to have the little objects more distinct. Now, everybody knows that the focal distance of any lens, or system of lenses, such as the telescope is, will differ according to whether the beams come from a more or less distant object. The little bodies did not appear distinct in the focus of the sun; I had to draw out the eye-piece; but if the focal distance was greater, their distance was smaller than that of the sun, and by means of a scale placed on the eye-piece, I soon obtained the result that these little bodies belong to our atmosphere, floating in a stratum of about 4,000 metres down to about 200 metres, the most numerous swarm passing almost always at a distance of not less than 500 metres. Here I remark that for my observations I had chosen the time of the sun being in, or about, the meridian, for then I was sure to have its light as strong, and the sky as clear as possible, while mostly preferring a magnifying power of only 48 diameters.

Taking the little bodies in the right focus, I was enabled not only to draw their shape, which I found very various, but also to measure their apparent diameter, which did not differ less, and depended much on distance, the nearer ones being larger, and, as I learned from the scale the accurate distance of every one, I calculated their diameter to vary from 10 to 59 millimetres, the average being 32 millimetres. Their shape was very various, too. The greater number were oblong, angular, resembling flakes, some few were orbicular, while some smaller ones were star-shaped, with transparent arms.

With respect to their frequency, I was surprised to find on certain days, especially in April and May, an incalculable number of little bodies in the field of the instrument, passing without interruption for hours. In general I found their number to be connected with the purity of the sky; and every day I noticed the average, the daily minimum occurring in the morning and evening hours, the maximum in the noon-tide hours; also the annual minimums in the summer and winter months, the chief maximum from April 20th to May 15th, the second, much lower maximum in August and September. I often saw their number increase soon after clouds had passed.

The velocity of the bodies, irregular in the lower strata, being about 2 metres in a second, became greater and more regular in the higher ones, where, for instance, at a distance of 3,000 metres, I found them to pass 8 metres during the same period, a rapidity agreeing closely with that of the *cirri*, which often passed at or above this distance. Whether far or near, all these little bodies glittered in a magnificent white light behind the sky, but as it retreated farther from the sun its blue colour became darker, the light of the bodies consequently diminished, and was more and more absorbed, when I followed them to some five or more degrees from the sun, in whose proximity they always brightened most, but passing over its disc, appeared to be rather dark, changing, however, suddenly into white when they emerged and entered the blue again. It became obvious that the little bodies I had before me were of small density, partly opaque, apparently of a white and reflecting surface, the edges of which were lit up by the sunbeams.

The course of the higher ones (at some 1,000 metres distance) being generally parallel, and their reciprocal velocity of about the same rate, I noticed much variety in the lower strata, where their flight was often of great inconstancy, changing their direction every moment, or falling, and second after second augmenting their focal distance, by the change of which, taken on the eye-piece scale, I learned that these bodies did not quite follow the law of gravitation, losing time; a fact not surprising to me, already convinced of their small consistency. In comparing the daily direction with the simultaneous course of winds and clouds, there was a remarkable conformity. Accepting the direction of the clouds to be the same as that of the wind in the stratum they pass through, a supposition not far from the truth, to which, of course, I was forced, having no weather-cock in such high regions, I found the direction of the little bodies and the clouds (in about the same stratum) to be (1) accurately the same in 31 per cent.; (2) differing not above 90 degrees in 49 per cent.; (3) differing not above 180 degrees in 67 per cent.; and (4) of quite opposite direction in only $1\frac{1}{2}$ per cent. This conformity is so evident that when the sky is cloudless, starting from the distance and direction of the ever-passing little bodies, one might easily learn the direction and perhaps the velocity of winds in the reciprocal strata, a fact of course of no little value to meteorologists and even mariners.

Taken altogether, these results could not but lead to the opinion that what I had to deal with were ice-crystals and flakes of snow. Here it may be recollected that already, in the seventeenth century, Mariotte, the renowned discoverer of the law of gas-expansion, pointed out that parhelions and mock-moons are caused by ice-crystals floating in the sky; and indeed, if we consider the above results, we are forced to believe him. Firstly, we learned that these bodies belong to the atmosphere; we also found them in its lower strata. Their average size of 32 millimetres, their flake-like shape, their incalculable number, will also strongly convince us. But while the minimum during the winter months might seem rather unaccountable, the chief maximum occurring in April and May, it may be remarked that from September to March the sun, although in the meridian, does not light up so strongly the rather misty sky; and that many days the sun will not appear at all. Now, referring to the chief maximum, from about April 20 to May 15, is it not astonishing that it occurs on the very same days which, especially those of May, were at all times well known from their low temperature, and called in Germany "the Latins" (Pancratius, May 12; Servatius, May 13, &c.), and were much feared by gardeners? But are the enormous masses of ice-crystals found in the atmosphere during these days the origin of its low temperature, or does the latter favour the formation of snow-masses? I only mention the fact that, for instance, heat is absorbed when snow is melting, and would be happy to direct the attention of meteorologists in any country to this phenomenon, inviting contributions of facts and correspondence. Finally, the velocity of the bodies being the same as that of the clouds, their reflected magnificent white light, their regular courses in the higher regions where strong winds are generally blowing, their irregular or even falling movement and small density in the lower ones, and their very remarkable conformity of direction with simultaneously passing clouds, will give much support to my explanation. HENRY WALDNER

Weinheim, near Heidelberg

THE MONGOOSE AND THE COBRA

IN reading the interesting account of a fight between these two animals, as given in NATURE for Jan. 11 (p. 204), the question arises, How does the mongoose survive the bite of the cobra? There are only two solutions of this question, viz.:—(1) That the mongoose has some antidote; and (2) that it is not affected by the cobra poison. With regard to the first, various observers give different antidotes, such as grass, *Aristolochia*, &c. (see Sir J. E. Tennent's "Natural History of Ceylon," p. 38). There is no *one* plant that the mongoose has been *proved* to go to as a remedy. 2. That the mongoose is not poisoned by the bite of the cobra has, I think, been proved by Dr. Fayrer, of Calcutta. I quote three of his experiments, which are published in the *Edinburgh Medical Journal*, April 1869, pp. 917-919:—"A young mongoose (*Herpestes Malacconsis*) was bitten two or three times by a full-grown cobra, at 1.24 P.M. on the 30th April 1868, on the inner side of the thigh from which the hair was first removed. Blood was drawn by the bites." This animal died in six minutes, but in the two following experiments no harm resulted to the mongoose. The second mongoose was also "bitten on the inner side of the thigh, and put into a cage immediately." It got no antidote except "raw meat," and was none the worse for the bite. The third mongoose was put into a large wire cage with a full-sized cobra at 1 P.M. (April 2, 1868). "The snake struck at the mongoose, and they grappled with each other frequently, and apparently the mongoose must have been bitten, as the snake held on to it about the neck or head. At 1.15 P.M. there was no effect on the mongoose; both it and the snake were much excited and angry, the snake hissing violently. 2.30; no effect on the mongoose. The snake is bitten about the head, and shows the bleeding wounds. 2.51; they are both occasionally darting at each other, but the mongoose jumps over the snake, and tries to avoid it. Next day at noon both were well; the snake frequently struck at the mongoose, but did not appear to injure it; both seemed very savage, but the mongoose would not bite the snake; he jumped over it. There had been two cobras in the cage during the night, both equally fierce, and striking each other and the mongoose; but the latter was uninjured. He was bitten once by the cobra rather severely on the head." JAMES W. EDMONDS

HARTWIG'S SUBTERRANEAN WORLD*

THE increasing demand for works of a semi-scientific character similar to that now under consideration, is in itself the most satisfactory proof that a desire for acquiring a more extended and accurate knowledge of the phenomena of Nature is gradually taking root within a

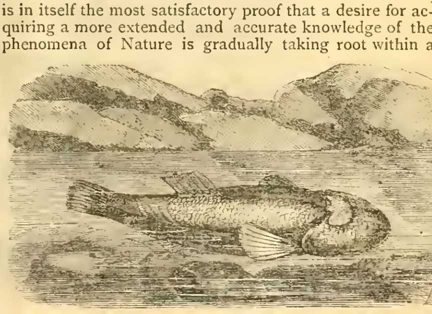


FIG. 1.—Blind Fish (*Amblyopsis spelæus*)

class of society, which, until of comparatively late years, had always contented itself with a very opposite style of literature. When it is observed, in many of the so-called popular scientific books, that accuracy has evidently been less

* "The Subterranean World." By Dr. George Hartwig. (London; Longmans, Green, and Co.)

carefully studied than what is termed sensational effect—a feature so characteristic of the period we live in—it is refreshing to find that Dr. Hartwig, in his description of the various phenomena of the subterranean world, has, without any such aid, succeeded admirably in conveying a vast amount of solid information, in so lucid and easy a style as to make even his unscientific readers quite interested, and likely to forget that he is treating of subjects usually considered as pertaining to the domain of dry Science. In so doing he seems also to have been assisted by having adopted a system of classification, or rather grouping, of the subjects which form his separate chapters, which, although not strictly scientific, is preferable in the present instance, as being more in accordance with popular notions.

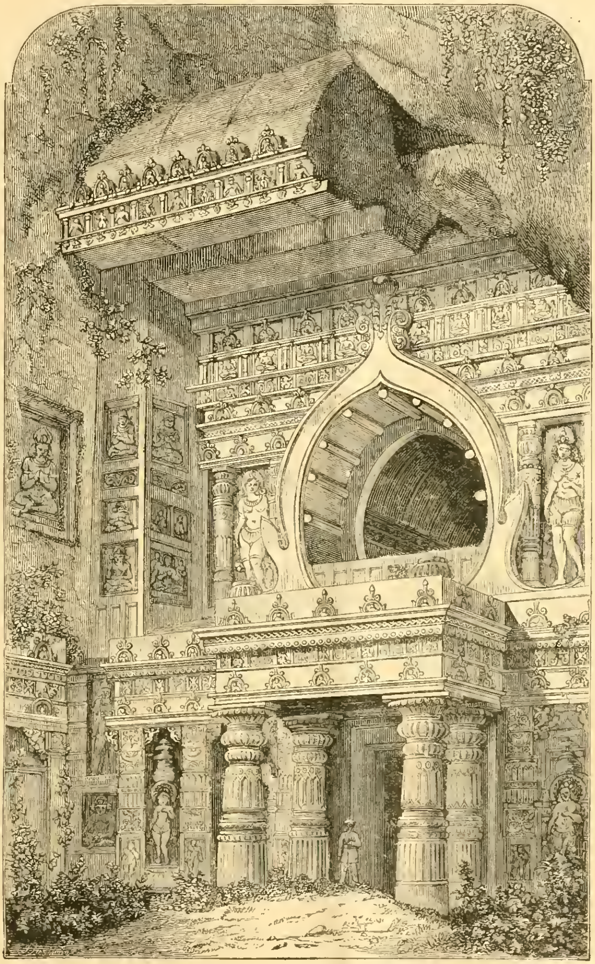
The work, besides being well got up, is abundantly

CARBONIFEROUS FOREST, CARBONIFEROUS PERIOD

illustrated; many of the woodcuts being of very superior character and execution, whilst the plates are, in general, good, and with one exception—that of the ideal view of the great earthquake at Lisbon in 1775—they are free from that objectionable sensational or exaggerated character so observable in the illustrations of French works on popular Science, several of which have lately been rendered into English. The two maps indicating the distribution of coal and metallic deposits in Great Britain and the Americas respectively are not on a par with the rest, owing to errors of omission; thus, amongst others, neither the central lead-producing district of Wales, nor the Northampton iron district, are shown in the former; nor have the auriferous deposits of Central America or British Columbia been indicated on the latter map.

In a work intended for the general British public, the temperature, when alluded to, should preferably have been stated in degrees of Fahrenheit's thermometer, since, although the scale of Celsius or centigrade is often made use of by men of science here, it will not be at all familiar to the majority of the readers of Dr. Hartwig's book, which it is to be hoped will have a very extended circulation. Several errors in the text might also be pointed out—as, for example, calling the usual Cornish ore or copper pyrites a bisulphuret of copper; titanium is stated to be a metal of a copper red colour, &c.; but when the great extent of scientific ground over which the author travels in this book is taken into consideration, some allowance must be made, and it must fairly be admitted that the work, as a whole, is singularly free from serious errors, and we would recommend it strongly, in the belief that from its at once easy entertaining and instructive style, it will be sure to interest many in the study of these natural phenomena, to whom the very name of Science is at present associated with all that is dry and uninviting.

D. F.



INDIAN ROCK-CUT TEMPLE: PORCH OF THE CHAITYA CAVE TEMPLE, AJUNTA

RECENT DISCOVERY OF PIT-DWELLINGS

DURING the late summer, while engaged in excavating a Roman building at Finkley, near Andover, a deep trench, 100 feet in length, was found, dilating at the opposite ends into large subterranean pits, which, from the primitive character of the articles met with in them, such as flint and bone implements, spindle-whorls of chalk, and a rude form of pottery, appeared to belong to an earlier period than the Roman. One of the labourers engaged in the explorations became quite an expert in the recognition of these rude objects; and he having lately been employed in digging a yard at a new railway station, situated on a hill, about half a mile distant from St. Mary Bourne, immediately overlooking the Upper Test Valley, found the subsoil so abundant in calcined stones, broken pottery, and other evidences of early occupation, that he called my attention to the circumstance, which led to the discovery of a group of pit-dwellings or hut-circles; and it is likely, from their mode of arrangement, that they form a portion of an extensive settlement or *vicus*. Some knowledge of nine of these has been obtained, although, from their situation, two only have been completely investigated, and five others partially.

The pits occupy the space of about a quarter of an acre, and have all entrance shafts, sloping gradually downwards from their inlets, and widening as they approach the pits. They may, with their contents, be described *seriatim*. No. 1 is oval or pear-shaped, having its entrance southwards. Its length is 22 feet from the end of the pit to the mouth of the alley; greatest diameter 12 feet; depth at the centre of the pit 5 feet. This was the only circle that contained flints, of which twelve cart-loads were removed from it; and as some of the stones were arranged in courses, without mortar, around its circumference and on each side of the alley, I have thought that the superstructure must have been of flint, and had fallen in. The relics found were chiefly at the centre, where the fire-place had evidently been; the smoke most likely escaping through the centre of the roof. They consisted of about a bushel of calcined flints, bones of a small species of *Bos*, probably *longifrons*, *Cervus elephus*, *Capra*, *Sus*, and *Canis*, besides broken vessels, chiefly of a very rude, hand-made kind, although a few pieces found about the pits bore wheel-marks. The bones had mostly been split open in order to obtain their marrow. They had further been exposed to fire, and bear impressions made by teeth and knives; and some of the smaller long bones had evidently been used as marrow-spoons, while other small splinters of bone had the appearance of having served the purpose of awls or needles. In this circle also part of a rude sandstone hand grain-rubber was found, besides some flint-flakes, a scraper, and some cores; and, in addition, the outer lip of a large cowry, which had been carefully cut from the shell, and had been used as a rasp, the crenulations in the lip being considerably worn down. It had further been employed as a polisher apparently, the enamel being worn away in places.

Pits 2 and 3 were only partially explored, as they extended beneath the station yard. One of them, however, was partly filled with calcined flints; and in it were found a piece of a grain-rubber and pottery and bones similar to those just described.

Pits 4 and 5 had only portions of their passages opened, as the pits extended beneath the Station Road. In these we found a few flint-flakes, and some calcined stones.

Pit 6 contained no remains, as it was evidently the passage only of a pit partly formed, and had not been occupied.

In digging a well in the station garden similar relics were thrown out, and it is evident that the shaft of the well passed through one of these pits; and, as additional evidence of British occupation, in clearing away the soil around the circles, one of the labourers picked up a Gaulish gold coin, which bears on its obverse and reverse degraded representations of more perfect figures. The coin, in short, is a slightly more perfect copy of the lowermost of the three coins depicted at p. 84 of "The Celt, the Roman, and the Saxon," 1st. ed.; which figure is there stated as being a rude copy of a gold stater of Philip of Macedon.

Pit 7 was fully explored. It was 42 ft. in length from the extremity of the pit to the mouth of the passage, which opened eastward; its widest diameter 13 ft. 6 in., and depth 5 ft. at the pit's centre. Here the fire-place had stood, as in No. 1, and around it we found bones similar to those discovered in Pit 1, with the addition of some teeth of a small species of horse, and bones of the hare or rabbit. The bones were, in most cases, broken, and some of them had been wrought for use as implements. Two flint arrow-heads were found in the alley, and the centre of the circle further contained flint-flakes, scrapers, cores, and arrow-heads, a fragment of a rude grain-rubber, and a flint muller showing use on one side. Here also occurred a whetstone, made from a piece of sandstone such as I have observed occurring in the drift of the Reading beds; and evidently from the same drift a lump of native ironstone, containing a large percentage of iron, which had been picked up by some occupant of the pit and used as a hammer. As throwing some small light on their domestic economy, a chalk spindle-whorl was found, and with it a small disc of pottery, bored at the centre, the direction of the hole showing that it had been suspended by a string, perhaps round its owner's neck. The whole of the fictile ware found here was of a rude hand-made type, and some of the "crocks" were scored with irregular zigzag lines, made apparently with a pointed stick.

At nine feet south of Pit 7 a circular hole in the chalk was cleared out. It was found to be 5 ft. in diameter and 3 ft. in depth. It contained a quantity of bones of animals similar to those already enumerated, with snail shells that had been exposed to fire; and beneath the bones a number of charred flints, with charcoal and ashes. It was evident that strong fire had been employed here, as the chalk was in places burnt through and discoloured to the depth of several inches, which led to the inference, coupled with its contiguity to Pit 7, that it was a cooking-hole. It is not unusual for uncivilised people, as the negroes, to have their cooking places outside their dwellings (see "Flint Chips," by E. T. Stevens, p. 59).

At another part of the same yard, about 10 ft. of well-built wall was removed. It was doubtless Roman, as near it a better kind of pottery was found, including a piece of Samian, besides two roof-nails and a bronze buckle.

The quantity of calcined stones everywhere present was the most striking feature in the remains. Some of them, I observed, were faced on one side, and a few had facets at right angles, and these, it occurred to me, might have been used in constructing ovens or fire-places. A large number, however, were perfectly circular, and had bright, clean surfaces; these might have been employed for the purpose of stone-boiling.

With traces of Roman occupation we have here these rude remains which show residence by an earlier people, who, doubtless, lived on after the advent of the Romans. I have, as yet, observed no entrenchments in the field; but there is no doubt that similar circles occupy a large space of the upper slope of the valley. The flint implements stamp the remains as Neolithic; and those found in the pits differ in no respect from the wrought flints occupying the subsoil of the yard, as well as occasionally occurring on the surface of the adjoining fields. The settlement is favourably situated to have enabled the occupants to obtain water from the river Test; and along the same side of the valley, within the space of two miles, I have discovered more than one working site, in which I have obtained a large and varied collection of tools and weapons both chipped and polished.

These huts must have been covered, some, perhaps, with stones, others with a wooden or wattle superstructure, covered with clay or sods of turf; and their poor inhabitants evidently cultivated, to a small extent, some of the cereals, had an early knowledge of weaving, and lived domesticated with oxen, goats, and swine. The red-deer were most likely obtained by hunting in the dense forest that then occupied the deep clay lands of North Hampshire, as an extension of the ancient forests of Harewood, and Chute, and Finkley. Further, these shallow pits might have been the summer residences of a people whose winter habitations were at Finkley.

J. STEVENS

INAUGURATION OF THE OBSERVATORY AT CORDOBA

AN interesting account of the inauguration of the Argentine Observatory at Cordoba in October last appears in the *Standard* of Buenos Ayres. The chief feature of the ceremonial was a very able address by Prof. Gould, the Director, from which we make the following extracts, as bearing specially on the work of the observatory :—

"In the year 1751 a French astronomer, the Abbé de la Caille, visited the Cape of Good Hope for the purpose of determining the positions of the principal southern stars. With a little telescope of comparatively insignificant dimensions, he succeeded in obtaining the materials for so complete a catalogue—as far as the limit of brightness which his telescope permitted—and in determining the positions of those stars so well, that this catalogue of about 9,800 stars constitutes to-day the chief reliance of astronomers for their knowledge of a large portion of the southern sky. Since that time a permanent observatory has been established by the British Government at the same place, and a large number of valuable observations have been made by various eminent men. Other observatories in the southern hemisphere have been founded at Paramatta, Santiago de Chile, and Melbourne, all of which have contributed essentially to our knowledge of the southern sky; as also has the observatory at Madras, which, although north of the equator, commands a view of the greater portion of the southern heavens. Yet how much remains to be done in this direction will be very evident when I state that, while the number of stars in the northern hemisphere whose positions and magnitudes have been determined cannot fall short of about 330,000, the number in the southern hemisphere whose observed places have been published does not probably exceed 50,000. But this is not all. The greater portion of those which have been observed lie in that part of the sky which is clearly visible in Europe; and if we consider the regions beyond 30°, there are scarcely 13,000 southern stars whose places and magnitudes have been determined and made available for scientific use, while the corresponding portion of the northern sky contains something like 164,000 such stars.

"The first undertaking now proposed for the Argentine Observatory is to do something towards filling this hiatus by determining the places of the principal stars situated between the tropics, where the observations of northern astronomers begin to become less numerous, and the polar circle, where Gilliss' observations commence. This work is best performed by dividing the sky into narrow zones or belts, and subjecting each zone to a special scrutiny for the purpose of measuring the positions of all stars of a sufficient brightness within its limits. If no unforeseen impediment presents itself, these observations should be completed within two years from their commencement.

"There is another most important investigation especially desirable in the present condition of our knowledge: this is the application of the newly-discovered methods of stellar photography to the more prominent objects in the southern heavens. The ingenious researches and inventions of Mr. Rutherford in New York have resulted in the development of methods by which the relative positions of clusters of stars may be permanently recorded by photographing them upon glass, and the numerical values subsequently determined by means of a measurement of the photographic impressions, with a degree of precision far greater than that of the ordinary methods. And this process possesses the signal and peculiar advantage, that the representations thus obtained of the stars' places at a given moment may be preserved, and the measurements repeated at any subsequent time. The process has not yet been introduced into European observatories, but it has been thoroughly tested in America, and valuable researches have already been made by this photographic method.

"During the greater part of the year we have had neither instruments nor building, and during the short time these have been available we have experienced an unexpected and most serious obstacle in the clouds of impalpable dust, which, rising from all sides, penetrate to the inmost crevices of every part of the instruments. This difficulty will, I think, be obviated to a great extent when vegetable growth shall have covered the soil; and to this end the Minister has given directions for providing as good a supply of water as may be possible, while the building and instruments have been provided with special and unusual protections against the evil. The position of the city of Cordoba renders this trouble inevitable, inasmuch as water for irrigation is only to be found in the valley, whilst an observatory must necessarily be placed upon high land. With the arrival of the rainy season I trust that a carpet of vegetation may remove this source of anxiety.

"A considerable time would, under any circumstances, have been requisite for computing the numerical table, and making the various other calculations needful for bringing the instruments into active service. The additional interval has been employed in an undertaking of a totally different sort, which may, I trust, be found in the end to possess as much scientific importance as the work originally intended. During this period of enforced delay we have succeeded in making a full catalogue of all those stars of the southern heavens which are visible to the naked eye, determining for each one the precise degree of its brightness. When, after the moon has set to-night, you raise your vision to the starry sky, and, as you look more intently, perceive one faint star after another reveal itself to your sight, you will yet succeed in discerning no star whose place and magnitude has not been recorded within the past year by some one or more of the observers in this institution—

"'Sidera cuncta notans tacito labentia cœlo.'

"The progress of the work so far has not failed to afford its due share of discoveries. It has given us the knowledge of a considerable number of stars which possess the singular character that their brightness is not always the same, but undergoes systematic variations. Some have been seen to rise to considerable brilliancy, and then fade away until telescopes of some power are needed for rendering them visible. Others still are now found to possess a brilliancy decidedly greater or decidedly less than that which has been assigned to them by more than one astronomer in times past. Such stars must be carefully watched, and the fact of any regular and periodic fluctuation in the amount of their light either established or disproved. Of such cases there are already many on our records, thanks to the assiduity and zeal of the assistant astronomers, no one of whom has failed to make manifest the existence of several. One of those most remarkable for the rapidity of its changes is a little star in the constellation "Musca," which is invisible to the unaided sight during one half its period, and visible during the other half; while the observations of Mr. Rock show that it goes through all its changes within the short interval of $21\frac{1}{4}$ hours. Another in the constellation of the "Southern Triangle," which has been regularly observed by Mr. Davis, exhibits regular fluctuations of light, comprised within a period of about $3\frac{1}{2}$ days, similarly alternating between visibility and invisibility. These two exhibit the most rapid changes of any of the stars which we have hitherto observed; but there are others not less interesting, observed not only by the two gentlemen mentioned, but also by Messrs. Thorne and Hathaway, who are likewise pursuing these investigations with much success."

NOTES

The retirement of Prof. Huxley from the London School Board throws a great responsibility upon the men of Science in London in general, and on Marylebone in particular. We are of opinion that of all the good work which Prof. Huxley has done, none will have a more lasting national importance than that which has resulted in the introduction of Science among the subjects to be taught in the London schools—and, therefore, in all the School-Board-schools throughout the country, for the force of public opinion will, in the long run, insist that the London model shall be everywhere followed. It is because we fear that this important advance may be arrested, unless steps are taken still to have the claims of Science represented on the Board, that we draw attention to the subject, which, in our opinion, is of sufficient importance to occupy the attention of the Royal Society, and the other scientific bodies, if their aid is necessary. Doubtless membership of the School Board involves sacrifice; but it is to be hoped that the clerical squabbles which have so interfered with the desired progress here, as it did, ineffectually, in other countries, are now as nearly over as they ever will be; and if this be so, then, instead of the 170 sittings given by some members last year, a much smaller number will suffice.

We have reason to know that many weak people have been alarmed, and many still weaker people made positively ill, by an announcement which has appeared in almost all the newspapers, to the effect that Prof. Plantamour, of Geneva, has discovered a comet of immense size, which is to "collide," as our American friends would say, with our planet on the 12th of August next. We fear that there is no foundation whatever for the rumour. In the present state of science nothing could be more acceptable than the appearance of a good large comet, and the nearer it comes to us the better, for the spectroscope has a long account to settle with the whole genus, which up to this present time has fairly eluded our grasp. But it is not too much to suppose that the laymen in these matters might imagine that discovery would be too dearly bought by the ruin of our planet. Doubtless, if such ruin were possible, or indeed probable—but let us discuss this point. Kepler, who was wont to say that there are as many comets in the sky as fishes in the ocean, has had his opinion endorsed in later times by Arago, who has estimated the number of these bodies which traverse the solar system as 17,500,000. But what follows from this? Surely that comets are very harmless bodies or the planetary system, the earth included, would have suffered from them long before this, even if we do not admit that the earth is as old as geologists would make it. But this is not all. It is well known that some among their number which have withal put on a very portentous appearance are merely the celestial equivalents of our terrestrial "wind-bags"—brought down to their proper level they would have shrunk into very small dimensions indeed. But there is more comfort still. The comet of 1770 positively got so near to Jupiter that it got entangled among his moons, the diameter of the smallest of which is only some 2,000 miles; but the moons pursued their courses as if nothing had happened, while the comet was so discomfited by the encounter that it returned by another road—*i.e.* astronomically speaking, its orbit was entirely changed. While, last of all, in our correspondence this week, will be found one fact the more in favour of the idea that, in 1861, we actually did pass through a comet. We have a suggestion for those weak people who are still alarmed by these celestial portents, and steadily refuse to acquaint themselves with the most elementary work on Astronomy, which would convince them how groundless their fears are. In India, during the last Eclipse, the priests reaped magnificent harvests from the offerings of the faithful. In England, possibly, it would be considered incorrect to make such offerings to the priest; but let them still be made—to the Royal Astronomical Society. In this way the English Philistine would approach nearer the standard of his less-civilised brother; Science would be benefited, and, doubtless, the omen would be averted —at all events they always have been.

The Anniversary Meeting of the Royal Astronomical Society was held on Friday last, when the president's address was read. The medal this year has been awarded to Prof. Schiaparelli for his brilliant demonstration of the identity which exists in the elements of the orbits of certain comets and known systems of meteors. Among the obituary notices for the year were those of Sir John Herschel, Prof. De Morgan, and Mr. Babbage.

The Council of the Geological Society have awarded the Wollaston Medal for the present year to Prof. J. D. Dana, of Yale College, Connecticut, and the balance of the proceeds of the Wollaston Fund to Mr. James Croll, of Edinburgh.

The Hopkins Prize, which was founded in memory of the late Mr. Hopkins, and is adjudged to the author of the best original memoir, invention, or discovery in connection with Mathematico-physical or Mathematico-experimental Science that may have been published during the three years immediately preceding (who is or has been a member of the University of Cambridge) has been awarded to Prof. J. Clerk Maxwell, F.R.S. The adjudicators were Profs. Stokes, Tait, and Clifton. The fund is vested in the Cambridge Philosophical Society.

We learn that, in addition to the scholarships for Natural Science at Cambridge, of which a list was given in our number for February 1, King's College offers an exhibition of the value of about 80*l*. per annum. The examination will commence on April 9, will include Physics, Chemistry, and Physiology, with one Classical and one Mathematical paper, and will be open to all candidates under twenty, and to undergraduates of the college in their first and second year. Names must be sent in, before March 10, to the Rev. A. A. Leigh, tutor of the college, from whom further information may be obtained.

Prof. George Rolleston has been elected a Fellow of Merton College, under the ordinance of 1854, which founded the Linacre Professorship of Physiology, and endowed it out of the revenues of this college. Prof. Rolleston graduated in 1850, and was afterwards elected Fellow of Pembroke College. In 1860 he was appointed to the Linacre Professorship of Physiology.

The Industrial Museum at Edinburgh has lost, by the death of J. Boyd Davies, its zoological director or manager. No one knows what the authorities are going to do, but it is to be hoped they will select a good man, not a talker but a worker. The monetary value of the post is 200*l*. to 250*l*. per annum. The Lectureship on Zoology at the High School is also vacant.

At a meeting of the Royal Geographical Society held on Monday evening last, the president, Sir H. C. Rawlinson, stated that, three days before, the expedition, consisting of Lieut. Dawson, R.N., Lieut. Henn, R.N., and Mr. Oswald Livingstone, the son of Dr. Livingstone, set sail in the first steamer despatched from the Thames to Zanzibar direct. The three gentlemen engaged in it had been given every assurance that their undertaking would be assisted at home in every possible way. The subscriptions to the fund for its maintenance amounted to 5,000*l*., of which upwards of 2,000*l*. was received from London alone; Edinburgh had contributed 350*l*.; and the little town of Hamilton, the native place of Dr. Livingstone, 200*l*.; while the corporation of the City of London had subscribed one hundred guineas, and the leading commercial firms of the City had come forward in an equally liberal manner. The Admiralty has refused to allow Lieut. Dawson his full pay while engaged on the expedition.

THE important article which we are able to give this week, on the Position of the Centre of Gravity in Insects," by M. Felix Plateau, is an abstract of a long memoir by that author, to be found in the "Bibliothèque Universelle, Archives des Sciences Physiques et Naturelles," vol. xliii., for 1872.

THE *Naval and Military Gazette* asserts that the *Challenger*, screw corvette, will be commissioned early in the summer for a voyage of exploration and research. Some scientific gentlemen will be accommodated on board the vessel, and it is probable that Captain George S. Nares, now serving in the surveying vessel *Shearwater*, in the Red Sea, will be placed in command The actual places which will be visited have not yet been determined, but it is anticipated that the groups of islands in the Pacific will have special attention bestowed upon them. This movement on the part of the Admiralty is in encouraging contrast to the fact that Arctic voyages have been abandoned to other nations, and to the late refusal of the Lords of the Treasury to grant any assistance whatever to the Livingstone search expedition.

THE following is the list of officers and council of the Royal Microscopical Society elected on the 7th of February:—President—Mr. W. K. Parker, F.R.S. Vice-Presidents—Dr. W. B. Carpenter, F.R.S., Dr. J. E. Gray, F.R.S., Sir John Lubbock, Bart., M.P., F.R.S., Mr. John Millar. Treasurer—Mr. John W. Stephenson. Secretaries—Mr. Henry J. Slack, Mr. Jabez Hogg. Council—Dr. Robert Braithwaite, Mr. John Berney, Mr. Charles Brooke, F.R.S., Mr. T. W. Burr, Dr. W. J. Gray, Dr. Henry Lawson, Mr. Henry Lee, Mr. S. J. M'Intire, Mr. Henry Perigal, Dr. G. W. Royston-Pigott, Mr. Charles Stewart, Mr. T. C. White.

THE International Scientific Series, to be published by Henry S. King and Co., is an indication of a movement of great importance. The series will be published simultaneously in New York by Messrs. D. Appleton and Co., in Paris by M. Germer Baillière, and in Leipzig by Messrs. Brockhaus. The first volume, by Prof. Tyndall, F.R.S., on "The Forms of Water, in Clouds, Rain, Rivers, Ice, and Glaciers," is now in the press, and will be published in March next. Among others already arranged for are Prof. T. H. Huxley, F.R.S., on Bodily Motion and Consciousness; Dr. W. B. Carpenter, F.R.S., on the Principles of Mental Physiology; Sir John Lubbock, Bart., F.R.S., on the Antiquity of Man; Prof. Rudolph Virchow, on Morbid Physiological Action; Prof. Alexander Bain, on Relations of Mind and Body; Prof. Balfour Stewart, F.R.S., on the Conservation of Energy; Mr. Walter Bagehot, on Physics and Politics; Dr. H. Charlton Bastian, F.R.S., on the Brain as an Organ of Mind; Mr. Herbert Spencer, on the Study of Sociology; Prof. William Odling, F.R.S., on the New Chemistry; Prof. W. Thiselton Dyer, on Form and Habit in Flowering Plants; Dr. Edward Smith, F.R.S., on Food and Diets; Prof. W. Clifford, on the First Principles of the Exact Sciences explained to the non-mathematical; Mr. J. N Lockyer, F.R.S., on Spectrum Analysis; Dr. W. Lauder Lindsay, on Mind in the Lower Animals; Dr. J. B. Pettigrew, F.R.S., on Animal Locomotion; Prof. A. C. Ramsay, F.R.S, on Earth Sculpture; Dr. Henry Maudsley, on Responsibility in Disease; Prof. W. Stanley Jevons, on the Logic of Statistics; Prof. Michael Foster, on Protoplasm and the Cell Theory; Rev. M. J. Berkeley, on Fungi: their nature, influences, and uses; Prof. Claude Bernard, on Physical and Metaphysical Phenomena of Life; Prof. A. Quetelet, on Social Physics; Prof. H. Sainte-Claire Deville, Introduction to General Chemistry; Prof. Wurtz, on Atoms and the Atomic Theory; Prof. Quatrefages, on the Negro Races; Prof. Lucaze-Duthiers, on Zoology since Cuvier; Prof. Berthelot, on Chemical Synthesis.

THE death of Dr. Harvey, Professor of Botany in the University of Dublin, arrested the progress of the Flora Capensis shortly after the publication of the third volume had brought the work half-way towards its completion. It is hoped that if the Cape Legislature will accede to Dr. Hooker's request for a renewal of the grant towards the expenses of printing, the remaining volumes may be at once taken in hand. The general supervision will be undertaken by Prof. Thiselton Dyer, who will probably receive assistance in monographing different families from Profs. Lawson and Perceval Wright, Drs. Sonder, Trimen, Masters, and MacNab, and from Messrs. Carruthers, A. W. Bennett, Hiern, Britten, and Baker.

DR. MILLER COUGHTREY is engaged on a long paper on the long-handled combs, Roman, Swiss, bone cave, Mexican, and other forms. It is now in proof for the Proceedings of the Antiquarian Society of Scotland.

WE note the appearance of the first number of a new monthly magazine, "The Earth: a popular magazine on Geology," whose object is "to collate and bring together facts and discoveries bearing on advanced and truthful views of Geology, and to oppose false and current opinions on the subject." Among the fallacies to be exposed are:—"That there has been an evolution of one creature into another," "that vegetable life either preceded or succeeded animal life on the globe," "that granite is a rock of fusion," &c.; and among the truths to be advocated are:—"That the configuration of the earth is a result of the agency of the winds and tides, of volcanic action, and of fluviatile and glacial action," "that there has been no evolution of species," and "that basalt is a crystallisation from solutions."

WE are glad to see that the labours of the English Strasburg Library Committee, consisting of Mr. Hepworth Dixon, Lord Houghton, Prof. Huxley, Lord Lytton, the Duke of Manchester, Sir J. G. Tollemache Sinclair, Bart. M.P., and Mr. Trübner, secretary, are being crowned with success. From the list we have just received of books already presented, we see that almost every department of Government has presented its publications. This remark also applies to the following scientific societies:—The University of Oxford, the Trustees of the British Museum, the Astronomer Royal, the Royal Geographical Society, the Royal Society of Edinburgh, the Botanical Society of Edinburgh, the Early English Text Society, the Historic Society of Lancashire and Cheshire, the Meteorological Society, the Radcliffe Observatory, Oxford, the Royal United Service Institution, the Philosophical Society of Glasgow, the Royal Institution of Great Britain, and Owens College, Manchester. In this list we may remark that some of the most important of our societies are still conspicuous by their absence.

THE problem, "What to do with our juvenile criminals," appears to have been solved by the Government of the State of New York in a most satisfactory manner. We have before us, and hope to be able to return to it again, a pamphlet issued by the "Department of Public Charities and Correction," bearing the title, inexplicable to English bumbledom, of "Cruise of School-ship *Mercury* in Tropical Atlantic Ocean." It is, in fact, an account of a cruise undertaken in the interests of science, and under the management of Prof. Henry Draper, containing a report "on the chemical and physical facts collected from the Deep Sea Researches made during the voyage of the nautical school-ship *Mercury*, undertaken in the Tropical Atlantic and Caribbean Sea in 1870-71; the "cruisers" being, not Dr. Carpenter, Prof. Wyville Thomson, and Mr. Gwyn Jeffreys, but the boys committed to the care of the Commissioners in New York for slight misdemeanours and vagrancy!

WE regret to hear that the Geology Class at Christ's Hospital, having gone through an introductory course of lectures, has stopped, and has not been replaced by a class of Botany or any sister science. It is greatly to be regretted that the Chemistry Class do not get beyond the simpler metals and easy testing; those who would wish to study Chemistry are restricted to the more elementary branches of inorganie chemistry alone.

PROF. HUGHES, F.R.G.S., gave two lectures at Christ's Hospital on February 3 and 10 on Physical Geography. In his introduction he, like Prof. Huxley, claimed for his science a position equal to that held by the German Erdkunde, defining both to be that which explained to us "the aspect of nature and natural phenomena." In his first lecture he dealt with "High Lands and Table Lands," somewhat overthrowing the popular idea of mountains gained from text books. In his second lecture he spoke of the "Ocean and Deep-Sea Currents," explaining clearly and advocating warmly the ingenious theories and proofs of Dr. Carpenter, about which there has been so much discussion in the pages of NATURE. We attach no little importance to these lectures, because they brought the hearers up to the present state of our knowledge of the deep sea and of the Himalayan Mountains, far further than the best text-books have yet brought us. It is only to be regretted that other gentlemen of like abilities and knowledge with Prof. Hughes do not come forward and offer to lecture to boys on other branches of Natural Science. It is hard for those who feel an interest in nature to feel themselves bound by the iron chains of verse composition.

Lippincott's Magazine for January contains an interesting and profusely-illustrated article on the New Port Storm Signals, by Prof. Thompson B. Maury.

PHYSICS

Preliminary Catalogue of the Bright Lines in the Spectrum of the Chromosphere*

THE following list contains the bright lines which have been observed by the writer in the spectrum of the chromosphere within the past four weeks. It includes, however, only those which have been seen twice at least; a number observed on one occasion (Sept. 7) still await verification.

The spectroscope employed is the same described in the Journal of the Franklin Institute for November 1870; but certain important modifications have since been effected in the instrument. The telescope and collimator have each a focal length of nearly 10 inches, and an aperture of ⅞ of an inch. The prism-train consists of five prisms (with refracting angles of 55°) and two half-prisms. The light is sent twice through the whole series by means of a prism of total reflection at the end of the train, so that the dispersive power is that of twelve prisms. The instrument distinctly divides the strong iron line at 1961 of Kirchhoff's scale, and separates B (not b) into its three components. Of course it easily shows everything that appears on the spectrum maps of Kirchhoff and Angström. The adjustment for "the position of minimum deviation" is automatic; *i.e.*, the different portions of the spectrum are brought to the centre of the field of view by a movement which at the same time also adjusts the prisms.

* Reprinted from the *American Journal of Science and Arts.*

The telescope to which the spectroscope is attached is the new equatorial recently mounted in the observatory of the College by Alvan Clark and Sons. It is a very perfect specimen of the admirable optical workmanship of this celebrated firm, and has an aperture of $9\frac{4}{10}$ inches, with a focal length of 12 feet.

In the table the first column contains simply the reference number. An asterisk denotes that the line affected by it has no well-marked corresponding dark line in the ordinary solar spectrum.

The second column gives the position of the line upon the scale of Kirchhoff's map—determined by direct comparison with the map at the time of observation. In some cases an interrogation mark is appended, which signifies not that the existence of the line is doubtful, but only that its precise place could not be determined, either because it fell in a shading of fine lines, or because it could not be decided in the case of some close double lines which of the two components was the bright one; or, finally, because there were no well-marked dark lines near enough to furnish the basis of reference for a perfectly accurate determination.

The third column gives the position of the line upon Angström's normal atlas of the solar spectrum. In this column an occasional interrogation mark denotes that there is some doubt as to the precise point of Angström's scale corresponding to Kirchhoff's. There is considerable difference between the two maps, owing to the omission of many faint lines by Angström, and the want of the fine gradations of shading observed by Kirchhoff, which renders the co-ordination of the two scales sometimes difficult, and makes the atlas of Kirchhoff far superior to the other for use in the observatory.

The numbers in the fourth column are intended to denote the percentage of frequency with which the corresponding lin s are visible in my instrument. They are to be regarded as only roughly approximative; it would of course require a much longer period of observation to furnish results of this kind worthy of much confidence.

In the fifth column the numbers denote the relative brilliance of the lines on a scale where 100 is the brightest and 1 the faintest. These numbers also, like those in the preceding column, are entitled to very little weight.

Ref. No.	Kirchhoff.	Angstrom.	Relative Frequency.	Relative Brightness.	Chemical Element.	Previous Observer.
1	534·5	7060·?	60	3		
2	654·5	6677·?	8	4		L.
3	C	6561 8	100	100	H.	L. J.
4	719·0	6495·7	2	2	Ba.	
5	734·0	6454·5	2	3		
6	743·?	6431.	2	2		
7	768·?	6370·	2	2		
8	816·8	6260·3	1	1	Ti.	
9	820·0	6253·2	1	2	Fe.	
10	874·2	6140·5	6	8	Ba.	L.
11	D_1	5894·8	10	10	Na.	L.
12	D_2	5889·0	10	10	Na.	L.
*13	1017·0	5871·	100	75		L. J.
14	1274·3	5534·0	6	8	Ba.	R. L.
15	1281·5	5526·0	1	1	Fe.	
16	1343·5	5454·5	1	2	Fe.	
17	1351·3	5445·9	1	2	Fe. Ti.	
18	1363·1	5433·0	1	1	Fe.	
*19	1366·0	5430·0	2	3		
20	1372·0	5424·5	3	4	Ba.	L.
21	1378·5 ?	5418·0 ?	1	2	Ti. ?	
*22	1382·5	5412·	1	1		
23	1391·2	5403·0	2	2	Fe. Ti.	
24	1397·8	5396·2	1	2	Fe.	
25	1421·5	5370·4	1	2	Fe.	R.
26	1431·3	5360·6	2	2		R. ?
27	1454·7	5332·0	2	2	Ti.	
28 {	1462·9	5327·7	1	3	Fe.	
29 {	1463·4	5327·2	1	3	Fe.	
30	1465·0 ?	5321·	2	2		
*31	Corona line 1474·1	5315·9	75	15	Fe?	L.

Ref. No.	Kirchhoff.	Angström.	Relative	Relative Brightness	Chemical Element.	Previous Observer.
32	1505·5	5283·	5	4		
33	1515 5	5275·0	7	5		L. R.
34	E_1	5269·5	1	3	Fe. Ca.	
35	E_2	5268·5	1	2	Fe.	
36	1528·0	5265·5	3	2	Fe. Co.	L.
37	1561·0	5239·0	1	1	Fe.	
38	1564·1	5236·2	1	1		
39	1567·7	5233·5	2	2	Mn.	R.
40	1569·7	5232·0	1	2	Fe.	
41	1577·3	5226·0	1	2	Fe.	
42	1580·5 ?	5224·5	1	1	Ti.	
43	1601·5	5207·3	3	3	Cr. Fe. ?	
44	1604·4	5205·3	3	3	Cr.	
45	1606·5	5203·7	3	3	Cr. Fe. ?	
46	1609·3	5201·6	1	2	Fe.	
47	1611.5	5199·5	1	1		
48	1615·6	5197·0	3	2		L. R.
49	b_1	5183·0	15	15	Mg.	L.
50	b_2	5172·0	15	15	Mg.	L.
51	b_3	5168·5	12	10	Ni.	L.
52	b_4	5166·5	10	10	Mg.	L.
53	1673·9	5153·2	1	1	Na.	
54	1678·0	5150·1	1	2	Fe.	
55	1778·5	5077·8	1	1	Fe.	
56	1866·8	5017·5	2	3		R.
57	1870·3	5015·?	2	2		R.
58	1989·5	4933·4	8	5	Ba.	L.
59	2001·5	4923·2	5	3	Fe.	R. L.
60	2003·2	4921·3	1	1		
61	2007·1	4918·1	3	3		L.
62	2031·0	4899·3	6	4	Ba.	L.
63	2051·5	4882·5	2	2		L.
64	F.	4860·6	100	75	H.	J. L.
65	2358·5	4629·0	1	1	Ti.	
66	2419·3	4583·5	1	1		
67	2435·5	4571·4	1	1	Li.	
68	2444·0	4564·6	1	1		
69	2446·6	4563·1	1	2	Ti.	
70	2457·8	4555·0	1	1	Ti.	
71	2461·2	4553·3	3	3	Ba.	
72	2467·7	4548·7	1	3	Ti.	
73	2486·8	4535·2	1	1	Ti. Ca. ?	
74	2489·5	4533·2	1	1	Fe.	
75	2490·6	4531·7	1	1	Ti.	
76	2502·5	4524·2	2	2	Ba.	
77	2505·8	4522·1	1	2	Ti.	
78	2537·3	4500·4	1	3	Ti.	
79	2553·?	4491·0 ?	1	1	Mn. ?	
80	2555·?	4489·5 ?	1	1	Mn. ?	
81	2566·5	4480·4	1	2	Mg.	L.
82	2581·5 ?	4471·4	75	8	A band rather than a line.	
83	2585·5	4468·6	1	1	Ti.	
84	2625·0	4443·0	1	1	Ti.	
85	2670·0	4414·6	1	1	Fe. Mn.	
86	2686·7	4404·3	1	2	Fe.	
87	2705·0	4393·5	3	2	Ti.	
88	2719·?	4384·8	1	1	Ca. ?	
89	2721·2	4382·7	1	2	Fe.	
90	2734·?	4372·	1	1		
91	2737·?	4369·3 ?	1	1	Cr.	
92	2775 8	4352·0	1	1	Fe. Cr.	
93	2796·0	4340·0	100	50	H.	L. J.
94	G.	4307·0	1	2	Fe. Ti. Ca.	
95	2870·0	4300·0	1	1	Ti.	
96		4297·5	1	1	Ti. Ca.	
97		4289·0	1	2	Cr.	
98		4274·5	1	2	Cr.	
99		4260·0	1	1	Fe.	
100		4245·2	1	1	Fe.[1]	
101		4226·5	1	1	Ca.	
102		4215·5	1	2	Fe. Ca.	
103	*h.*	4101·2	100	20	H.	R. L.

The sixth column contains the symbols of the chemical substances to which, according to the maps above referred to, the lines owe their origin.

There are no disagreements between the two authorities; in a majority of cases, however, Angström alone indicates the element, and there are several instances where the lines of more than one substance coincide with each other and with a line of the solar spectrum so closely as to make it impossible to decide between them.

In the seventh and last column the letters J., L., and R. denote that to my knowledge the line indicated has been observed and its place published by Janssen, Lockyer, or Rayet. It is altogether probable that a large portion of the other lines contained in the catalogue have before this been seen and located by one or the other of these keen and active observers, but if so I have as yet seen no account of such determinations.

I would call especial attention to the lines numbered 1 and 82 in the catalogue; they are very persistently present, though faint, and can be distinctly seen in the spectroscope to belong to the chromosphere as such, not being due, like most of the other lines, to the exceptional elevation of matter to heights where it does not properly belong. It would seem very probable that both these lines are due to the same substance which causes the D^3 line.

I do not know that the presence of titanium vapour in the prominences and chromosphere has before been ascertained. It comes out very clearly from the catalogue, as no less than 20 of the whole 103 lines are due to this metal.

Hanover, N.H., Sept 13, 1871 C. A. YOUNG

SCIENTIFIC SERIALS

THE *American Naturalist* for October 1871 commences with a paper by Dr. Jeffreys Wyman entitled, "Experiments with Vibrating Cilia," the chief points in which are some determinations of the rate of movement of the vibrating cilia on the gills of Mollusca, both in air and in water, and the description and drawing of an instrument by means of which this rapidity can be measured and exhibited so as to be seen over a large lecture-room. Prof. James Orton furnishes some contributions to the Natural History of the Valley of Quito (continued in the next number); and Dr. J. S. Billings contributes a paper on *Hysterium*, a genus of Ascomycetous Fungi, and some of its allies, illustrated by a plate. Mr. T. Martin Trippe has a very interesting paper on some differences between Eastern and Western Birds, in which he traces the difference in habits, note, time of breeding, &c., in the same species of bird in the eastern and newly-settled western portions of the American continent, and the manner in which the indigenous avifauna of the Western States is becoming gradually superseded by eastern forms, along with the advance of man.

The first paper in the number for November is by Grace Anna Lewis on Symmetrical Figures in Birds' Feathers, in illustration of the beauties furnished for the microscope by the feathers of birds. Dr. Elliott Coues gives a description and drawing of a little-known species of oriole, the only one which is a native of the Western States, and is known as Bullock's Oriole, *Xanthorthus Bullockii*, Swainson. Prof. George H. Perkins contributes some "Notes on the Geodes of Illinois;" and the remainder of the number is occupied by reviews, and the usual interesting items of Natural History Miscellany.

The number for December opens with an extremely interesting paper by the Editors on "The Mammoth Cave and its Inhabitants," an account of a visit paid to this extraordinary cavern in a hill of the sub-carboniferous limestone formation in Edmondson County, Kentucky, after the Indianapolis meeting of the American Association for the Advancement of Science. After a general description of the cave and history of its inhabitants, it contains a description, with drawings, of all the species of Crustacea and insects which are found in it. The Rev. Samuel Lockwood writes an account of "A Singing *Hesperomys* or Vesper-mouse," the species known as the jumping-mouse, wood-mouse, and white-footed mouse, with the notes of its song. This number concludes Vol. v. of this admirably-conducted magazine, which we commend to the notice of all interested in the study of natural history.

Journal of Botany for January. A memoir of the late lamented editor of this journal, Dr. Berthold Seemann, commences the new volume, now conducted by Dr. Trimen, assisted by Mr. J. G. Baker. The original articles are as follows:—"On the Genus *Albizzia*, nearly allied to *Acacia*," by Baron Ferd. von Mueller; "The *Erysiphei* of the United States," by Messrs. M. C. Cooke and Peck; a continuation of Mr. J. G. Baker's "Botany of the Lizard Peninsula;" and Lichenographical Notes, by J. A. Martindale. Short notes, reviews, and reprints, complete the programme of the number.

THE first article in the *Quarterly Journal of Science* for January is by Captain S. P. Oliver, on "The Dolmen Mounds and Amorpholithic Monuments of Brittany," in which he details the history and analogies of these mounds, classifying them into twelve distinct varieties. The article is apparently not complete. Next follows a short paper on "The Illumination of Beacons and Buoys," detailing the most recent inventions in this direction. The third article is on "Natural and Artificial Flight," detailing M. Marey's investigations on this subject, with numerous illustrative woodcuts. A paper on "The Coal Commissioners' Report" is simply a *résumé* of the evidence brought before the Commission. Mr. Mungo Ponton, on "The Spectroscope: its Imperfections and their Remedy," advocates the construction of an instrument on the diffracting principle, without which the writer maintains that accuracy, certainty, and uniformity of results cannot be attained. The last and longest article in the number is on "Modern Cannon Powder," with two steel plates. A larger proportion than usual of this number is occupied by notices of books, and details of the progress of the physical and mechanical sciences.

THE last published part of the "Memoirs of the Natural History Society of Danzig" ("Schriften der Naturforschenden Gesellschaft in Danzig," New Series, vol. ii., Heft 3 and 4) contains but few papers of general interest, although the special scientific importance of some of them is doubtless very great. Thus a great part of it is occupied by a number of tables giving the results of meteorological observations made in Danzig, with great care and astonishing labour, by M. F. Strehlke, during the years 1841–43, and by a series of tables of refraction for micrometers, by M. E. Kayser. Two other papers of almost purely local interest relate to the chemical composition of the water supplied to Danzig, and to its effects upon lead pipes. The preceding papers occupy more than half the number before us; the remainder all relate to natural history matters. M. C. G. H. Brischke continues his minor observations upon insects, the greater part of his present communication relating to the enemies of the rape-plant and their parasites. The dipterologist will find a new species of *Phytomyza* described under this head. The same author contributes a list of the Rhynchota of the Province of Prussia. The fourth section of M. A. Menge's Prussian Spiders completes the list of zoological contributions. In it the author describes the first two families of his third tribe (the Tubitelæ), ending with *Argyroneta aquatica*, as the 170th species here described by him. M. A. Ohlert's "Lichenological Aphorisms," the only botanical paper, contains some important and interesting observations.

THE following are the most important articles in the *Revue Scientifique*, Nos. 25—32. Prof. Lorain, of Paris, has an interesting article on the report of the Committee of 1870 on the liberty of higher instruction; Mr. Herbert Spencer contributes a paper on General Laws; report of M. Quatrefage's course of lectures on Anthropology at the Museum of Natural History; Helmholtz's address in memory of Prof. Magnus at the Academy of Sciences at Berlin; Herbert Spencer on the Classification of the Sciences, an elaboration of his essay "On the Genesis of Science," published in 1854; Berthelot on the state of bodies in solution; report of Prof. Bernard's course of lectures at the College of France on Experimental Medicine; abstracts of paper read at the Indianopolis Meeting of the American Association for the Advancement of Science; translations of Lockyer's, Maclear's, and Respighi's accounts of the Total Solar Eclipse, together with reports of M. Janssen's observations; an article by Herbert Spencer on the reasons why he dissents from the philosophy of Comte, being a reply to a review in the *Revue des Deux Mondes*; M. Verneuil on Surgical Pathology; report of the committee appointed by the Society of Physicians and Surgeons of the Paris Hospitals to visit the new Hôtel Dieu; M. Alglave on the scientific *réunions* at the Assembly; M. Hebert on the "Tithonic Stage," and the new German school. There are in addition a number of reports of proceedings of foreign societies.

SOCIETIES AND ACADEMIES

LONDON

Royal Institution, February 5.—Sir Frederick Pollock, Bart. vice-president, in the chair. Messrs. Alexander Brodie, John Cleghorn, Edward John Gayer, Arthur Edward Griffiths, William Grogan, the Hon. Frederick H North, Messrs. Samuel Wagstaff Smith, W. Soames, Henry Virtue Tebbs, Burney Yeo, Henry Yool, were elected members. The special thanks of the members were returned for the following donations to "The Fund for the Promotion of Experimental Researches:"—Prof. Tyndall (3rd donation) 30*l.*, Mr. Arthur Giles Puller (5th donation) 21*l.* The presents received since the last meeting were laid on the table, and the thanks of the members returned for the same.

Geologists' Association.—A special general meeting was held on the 2nd February, when a revised code of laws was adopted. Subsequently, at the annual meeting, the report for 1871 was adopted, and the officers for the ensuing year elected. At the ordinary meeting which followed, the Rev. J. Wiltshire, M.A., F.G.S., president, in the chair, a paper was read by the Rev. T. G. Bonney, M.A., F.G.S., tutor of St. John's College, Cambridge, "On the Chloritic marl, or Upper Greensand, of the neighbourhood of Cambridge." The author commenced by a brief sketch of the geology of the Cam valley, and the position of the seam, barely a foot in thickness, which rests upon the eroded surface of the Gault, and is full of green grains and dark nodules, rich in phosphate of lime. He described the matrix as a fine chalky marl, full of foraminifera, and minute fragments of organisms, with a considerable mixture of mud, insoluble in hydrochloric acid. The composition of the green grains (commonly called glauconite) was then discussed, and it was shown that they differed considerably from the typical mineral of that name; he had not satisfied himself that any were casts of foraminifera. After a few words on the phosphatic nodules, and some erratic rocks in the bed, he gave a sketch of the palæontology of the deposit, calling attention to the condition of the various fossil remains, and to the number and size of the pterodactyles and turtles. He then gave his reasons for considering this deposit as formed during the Upper Greensand epoch, but as containing many fossils which had been derived from the Upper Gault by slow denudation. The nodules he considered as mainly of concretionary origin; for they were too pure to be regarded as clay saturated by phosphate. He concluded by sketching out his conception of the physical geography of the East Anglian district in the Neocomian and lower part of the Cretaceous epoch.—Prof. Morris, after some remarks on the value of the paper, spoke of the composition of the green grains, and then traced the range of the deposit, which he agreed with Mr. Bonney in thinking was the formation of a very long period of time.—Mr. Lobley remarked upon the mineralogical and palæontological differences existing between the Cambridge deposit and the chloritic marl of Dorsetshire.—Mr. Bonney, in his reply, having referred to the great scarcity of fossils in the Gault of Cambridge, the Rev. T. Wiltshire stated that the Gault of Kent was in some places devoid of organisms.

Zoological Society, February 6.—Mr. R. Hudson, F.R.S. V.P., in the chair.—A communication was read from Dr. J. S. Bowerbank, F.R.S., containing the first portion of a series of papers, entitled "Contributions to a general History of the Spongiadæ," in which descriptions were given of several species of *Tethea*, and of *Halispongia choanoides*.—A communication was read from Dr. John Anderson, containing notes on a young living female of *Rhinoceros sumatrensis*, which had been captured in Chittagong, in February 1868, and had been removed to Calcutta on its way to England. These notes were accompanied by a photograph of the animal from life.—A second communication from Dr. Anderson contained notes on *Manouria* and *Scapia*, two supposed genera of Land-Tortoises, which Dr. Anderson showed to be identical with *Testudo emys* of Schlegel and Müller.—Mr. Sclater read a paper on Kaup's Cassowary (*Casuarius Kaupi*), of which the Society's collection contained a living specimen. To this was added a list of the other known species of the genus *Casuarius*, and an account of their geographical distribution.—A communication was read from Dr. A. Günther, F.R.S., on two specimens of Lizards of the genus *Hydrosaurus*, from the Philippine Islands, for one of which, being hitherto undescribed, Dr. Günther proposed the name *Hydrosaurus nuchalis*.—A second communication from Dr. A. Günther contained the description of a new genus and species of Characinoid Fishes from Demerara, proposed to be called *Nannostomus beckfordi.*—A communication was read from Lieutenant Reginald Beavan, of the Revenue Survey Department of India, containing descriptions of two new species of Cyprinoid Fishes from the Punjab.—Mr. Howard Saunders exhibited specimens of and described a new species of Green Woodpecker from Southern Spain, which he proposed to call *Gecinus sharpii.*

Anthropological Institute, February 5.—Dr. Charnock, vice-president, in the chair. W. J. Jeaffreson, M.A., was elected a member.—Lieut.-Col. G. G. Francis exhibited a series of flint, stone, and bone implements and human bones from Paviland, Gower.—Mr. George Harris, vice-president, read a paper "On the hereditary transmission of endowments and qualities of various kinds." Of the actual transmission of qualities no doubt could be entertained. Many thought they were mainly derived from the mother, and in some instances they were inherited from the grandparents. That was often observed in cases of disease. Endowments did not, however, always directly descend, but were transmitted in various ways, such as in the descent of particular talents. In other cases it was modified in the transmission; occasionally the various qualities of both parents seemed to be divided among the different members of the family. That was observable in the breeding of animals. Physical qualities were also transmitted in the same way, and artificial acquirements had been considered transmissible. The most extraordinary instances were related of the existence of complete continuity, both mental and moral, between the parents and the children. The author considered the subject to be one of deep interest, and suggestive of various theories, and irespecting which the observations of each might add to the common stock of knowledge.—A paper on "the Wallons," by Dr. Charnock and Dr. Carter Blake, was then read. The Wallons were descendants of the old Gallic Belgæ who held their ground in the Ardennes, when Gaul was overrun by the Germans. The Wallons were tall, somewhat slender, raw-boned, tough, rough, and hardy, and made excellent soldiers. Their hair was dark, eyes fiery, dark-brown, or blue, and deep sunk. The ordinary Wallons stood in a similar relation to Belgium to what the Irish peasant did to the Sassenach. They were poor, jovial, good-natured, superstitious, chaste, hospitable, quarrelsome, violent, and generous, like the Irish. They were poetical, rich in song, and fond of the dance. They surpassed the Flemish in adroitness, activity, and skill, and the French in earnestness, perseverance, and diligence. As evidence of their peculiar character, a Wallon would drag a pig from Namur to Ghent, or even to Bruges, to gain a few sous more than he could in his own district. Some of the most eminent of the modern statesmen of Belgium were of Wallon descent. Notwithstanding these general remarks, a special mental and moral character might be predicated of the Wallons of each district. The paper concluded with copious remarks on the language of the Wallons, together with their proverbs.

Society of Biblical Archæology, February 6.—Dr. Birch, president, in the chair.—The following gentlemen were duly proposed as members of the society:—Mr. T. H. Christy, Mr. James Collins, Mr. George C. Hale, Rev. Prof. Mahaffey. An important communication was received from M. Clermont Ganneau, on an "Inscription in Hebrew or Ancient Phœnician Characters of the time of the Kings of Judah, discovered at Siloam-el-Fokani, near Jerusalem" In this paper M. Ganneau related the discovery of two incised tablets, executed on the wall of a ruined rock-cut chamber or sacellum, near to the house of the Sheikh of Siloam. The inscriptions were in the old Archaic character, now familiar to the archæological world in the famous Moabite Stone. Some Christian hermit had, about the fourth century of our era, wilfully mutilated part of the writing, but enough still remained to attest its extreme value as a palæographic record. Portions of the first four lines of the first tablet the learned savant believed to contain the name of the divinity Baal, and to denote a votive dedication to him by a functionary, name illegible, about the period of the later Kings of Judah. The author inclined to think that the cave had been originally dedicated to Baal at a still earlier period, probably by one of Solomon's Moabitish wives, and that it was afterwards added to and finished in a subsequent reign. M. Ganneau promised, in conclusion, shortly to lay before the society a more perfect examination and conjectural restoration of the inscriptions on both tablets, and expressed a hope that the records in question would prove not inferior in importance to any other, as being themselves the oldest, or nearly the oldest, positively Hebrew inscriptions in existence.

Mathematical Society, February 8.—Prof. Cayley, vice-president, in the chair. The chairman mentioned that the president had made inquiries at the Home Office as to the mode of procedure requisite for obtaining a charter for the society, and that the matter would come on for consideration at the next subsequent meeting (March 14) when members would have an opportunity of stating their views upon the desirability of incorporation.—Mr. J. W. L. Glaisher was elected a member of the society.—Mr. Cotterill gave an account of his paper "On an Algebraical Form, and the geometry of its dual connection with a polygon, plane, or spherical." The chairman, Dr. Hirst, and Prof. Clifford took part in a discussion on the paper.

Entomological Society, February 5.—Prof. Westwood, president, in the chair.—Mr. McLachlan brought before the notice of the meeting an illustration of the manner in which the increase of plant-lice is checked by Hymenopterous parasites; a family of aphides collected round a poplar twig exhibited had been utterly destroyed by these parasites, there remaining only the inflated empty skins much resembling the egg of some large insect, and each with a circular hole whence the parasite had emerged.—Mr. Druce exhibited a selection from a large collection of butterflies formed in Costa Rica by Dr. Van Patten. It included about fifty new species and one new genus. Amongst the more striking forms were four new species of *Papilio*, three of *Morpho*, three or four of *Leptalis*, &c.—Prof. Westwood exhibited drawings and specimens of various interesting species of *Acarnia*, including forms new to Britain. One of these was allied to the poisonous *Argas persicus*, and had been found in the crypt of Canterbury Cathedral. Mr. Bond had also seen examples found in a church on a gentleman's coat after two young bats had fallen upon him from the roof. Another pertained to the genus *Trogulus*, and had been found in Dorsetshire.—Major Parry read a paper on new species of Leucanoid *Coleoptera*, which was followed by others by Prof. Westwood and M. Snellen van Sollenhoven, on insects of the same family.

Edinburgh

Royal Physical Society, January 25.—Dr. Robert Brown, president, in the chair.—Prof. Turner exhibited a large specimen of the electrical eel (*Gymnotus electricus*) of South America, which he had received a few weeks ago from Dr. Ridpath, surgeon, West India Mail Steam Packet Service. He described the arrangement of the electrical organs, and compared them with the corresponding organs in *Torpedo*, *Malapterurus*, and *Mormyrus*, and in the tail of the common skate. Dr. T. Strethill Wright made some remarks on the relation of these curious organs to various electrical apparatus. The organs of the electrical fishes were not properly batteries, but were probably condensing apparatus. Some time ago he made an artificial electrical eel, and with it he had performed all the experiments Prof. Faraday had done with the electrical eel itself, which he would exhibit and explain to the society. He gave a sketch on the board of condensing voltaic apparatus, which was probably analogous to that of the electrical fishes.—Various species of Pedunculated Cirripedes of Barnacles were exhibited from Shetland, Cornwall, the Black Sea, &c., by Mr. C. W. Peach. In October last Mr. Gatherer, of Lerwick, sent him a fine colony of *Lepas fascicularis* which had been taken floating off Kirkallister lighthouse by a gentleman fishing, and who saw a great many similar masses floating past his boat. They are each attached to a bulb like mass, and are in various stages of growth. About ten are left, some having fallen off. When very young they are attached by a short peduncle to feathers, cork, cinders, and seaweeds, or any other floating object. As they increase in size they form a bulb on the foot-stalk. This in time becomes so large that it falls off, and thus the animal is buoyed up with it—in fact, "paddles its own canoe." When thus afloat the animals multiply, and the bulb is enlarged also. It is far from rare, and found in all seas. In Cornwall, after long-continued south-west winds, it is thrown ashore by thousands.—"Remarks on the Diamond Fields of South Africa," by Mr. Andrew Taylor.

Dublin

Royal Geological Society of Ireland, January 10.—Dr. W. Frazer in the chair. Prof. E. Hull, F.R.S., read some notes on the Marble of Carrara.—Prof. Macalister read notes of some further "Researches on Conchospirals." He pointed out the geometrical properties of the logarithmic spirals of Mollusca, the special form of spiral in Ammonites, and the methods of deducing the individual specific parameters from (α) tangential measurements, (β) horizontal sections, and (γ) vertical sections.—The Chairman exhibited a human skull from Swan River, Australia, encrusted with shells and much acted on by water.

PARIS

Academy of Sciences, February 5.—M. Serret presented a note by M. A. Mannheim, containing generalisations of Meunier's theorem.—M. H. Resal presented a memoir on the mechanical effects of the American hammer.—A memoir was read by M. E. Duclanx on the laws of the flow of liquids in capillary spaces.—Mr. P. Blaserna presented a note on the solar atmosphere, in which he claims to have arrived at the same conclusions with M. Janssen, from his observations during the eclipse of December 22, 1870.—M. Renou replied to the observations made by M. Delaunay with regard to the Meteorological Annual of the Paris Observatory at the last meeting of the Society, and M. Le Verrier suggested the appointment of a committee to revise the meteorological observations presented to the Academy during the last century, and to bring out an authentic edition of them.—Communications, descriptive of the aurora observed in France and elsewhere on the evening of February 4, from MM. Frou, Salicis, Laussedat, and Chapelas, were read, as also an extract from a letter from M. Cornu to M. Fizeau upon the spectrum of the same aurora. The most important result obtained by the last-mentioned author was the determination of the existence of a yellowish-green band coinciding with that previously observed by Angström in 1867–68.—M. Prazmowski also presented a note on the spectral investigation of the aurora of Feb. 4. He described a green band about E of Fraunhofer (seemingly identical with that observed by M. Cornu), a red band near C, and two more very faint bands in the blue and violet, near F and G.—M. Bobierre communicated some chemical investigations on the Landes of Brittany, in which he noticed especially the constituents of the ashes of plants grown on those soils. They are chiefly remarkable for the great quantity of silica contained in them and their poverty in alkaline salts.—M. Cahours presented a note by M. G. Chancel, on the contraction of solutions of cane sugar at the moment of inversion, and on a new saccharimetric process. The author described the method employed by him, and stated that a solution of cane sugar, after inversion, has undergone an appreciable diminution of volume, which increases in proportion to the amount of sugar in solution. Upon this property he proposes to found a new method of saccharimetry.—M. Sacc presented an analysis of the linseed oil referred to in a recent memoir read to the Academy.—M. Dupuy de Lome read two long and exceedingly interesting papers upon the construction of a screw aerostat invented by him, and on the results of a trial trip made with it. The machine consists of an oblong balloon, with a boat-shaped car; the author describes it as presenting great stability. The propeller worked by eight men moved the balloon through the air with a velocity of 2·82 metres per second, or $10\frac{1}{4}$ kilometres (about $6\frac{1}{4}$ miles) per hour, so that a certain amount of power over the movements of the machine was obtained.—The warm discussion upon heterogeny and the nature of fermentation was continued at this meeting by a second communication on the latter subject by M. Fremy, who denies that the experiments of M. Pasteur have anything to do with fermentation. He also declared that his theory has nothing in common with that of Liebig, with which it was identified by M. Wurtz. The paper contained accounts of experiments made with malt, yeast, milk, and grape-wort, and upon the decomposition of organic bodies by the action of moulds.—MM. Dumas and Balard made some remarks on this communication, and M. V. Meunier presented a note in which he stated that organic bodies do frequently make their appearance in solutions treated after M. Pasteur's method, so that, he thought, the results obtained by that gentleman are not conclusive.—M. de Quatrefages presented a note by M. E. T. Hamy describing the occurrence of brachycephalous negroes among the Cammas on the shores of the Fernand-Vaz River in Western Africa.—M. Milne-Edwards described a self-regulating gas-heating apparatus in use in the zoological laboratory of the Museum; and M. Sichel *fils* forwarded the description of a new ophthalmoscope for simultaneous observations by two persons.

BOOKS RECEIVED

ENGLISH.—A Treatise on Attractions, Laplace's Functions, and the Figure of the Earth, 4th edition: J. H. Pratt (Macmillan and Co.)—Science and Humanity: Noah Potter (Hodder and Stoughton).—Solid Geometry and Conic Sections: J. M. Wilson (Macmillan and Co.)—Report by the Committee on Intemperance, for the Lower House of Convocation: (Jas. Clarke and Co.)—Our National Resources and how they are reached: W. Hoyle (Simpkin and Marshall).—Consumption, and the Breath re-breathed; Dr. H. M'Cormac (Longmans).

FOREIGN.—Bulletin de la Société Imperiale des Naturalistes de Moscou, 1871, Nos. 1 and 2.

DIARY

THURSDAY, FEBRUARY 15.

ROYAL SOCIETY, at 8.30.—On the Induction of Electric Currents in an Infinite Plane Conducting Sheet: Prof. Clerk Maxwell, F.R.S.—On some Derivatives of Uramido-benzoic Acid: J. P. Griess, F.R.S.

SOCIETY OF ANTIQUARIES, at 8.30.

LINNEAN SOCIETY, at 8.—On a Chinese Artichoke Gall: A. Müller, F.L.S.—On the Habits, Structure, &c., of the Three-banded Armadillo: Dr. J. Murie, F.L.S.—Comparative Geographical Distribution of Butterflies and Birds: W. F. Kirby.

CHEMICAL SOCIETY, at 8.

FRIDAY, FEBRUARY 16.

ROYAL INSTITUTION, at 3.—On the Crystallisation of Silver and other Metals; Dr. Gladstone, F.R.S.

GEOLOGICAL SOCIETY, at 1.—Anniversary Meeting.

SATURDAY, FEBRUARY 17.

ROYAL INSTITUTION, at 3.—On the Theatre in Shakespeare's Time: Wm. B. Donne.

SUNDAY, FEBRUARY 18.

SUNDAY LECTURE SOCIETY, at 4.—On the Human Hand, as Illustrating the Scheme of Creation: Lawson Tait.

MONDAY, FEBRUARY 19.

ENTOMOLOGICAL SOCIETY, at 7.

ANTHROPOLOGICAL INSTITUTE, at 8.—Strictures on Darwinism: H. H. Howorth.—Race-Characteristics as related to Civilisation: J. Gould Avery.

LONDON INSTITUTION, at 4.—Elementary Chemistry: Prof. Odling, F.R.S.

TUESDAY, FEBRUARY 20.

ROYAL INSTITUTION, at 3.—On the Circulatory and Nervous Systems: Dr. Rutherford.

ZOOLOGICAL SOCIETY, at 9.—Notes upon the Anatomy of the young Hippopotamus, as observed in the specimen which died in the Society's Gardens on the 10th January, 1872: J. W. Clark.—Contributions to a General History of the Spongiadæ. Part II: Dr. J. S. Bowerbank.—On the Spiders of Palestine and Syria; containing a general list with descriptions of numerous new species and characters of two new genera: Rev. O. P. Cambridge.

STATISTICAL SOCIETY, at 7.45.—On Prison Discipline and Statistics in Lower Bengal: Dr. Mouat.

WEDNESDAY, FEBRUARY 21.

GEOLOGICAL SOCIETY, at 8.—Migrations of the Graptolites: Prof. H. Alleyne Nicholson, F.G.S.—How the Parallel Roads of Glen Roy were Formed: Prof. James Nicol, F.G.S.—Notes on Atolls or Lagoon-islands: S. J. Whitnell.

SOCIETY OF ARTS, at 8.—On Prison Labour, as an Instrument of Punishment, Profit, and Reformation: F. J. Mouat.

ROYAL SOCIETY OF LITERATURE, at 8.30.—On Results of recent Excavations in Rome: Mr. Vaux.

METEOROLOGICAL SOCIETY, at 7.

THURSDAY, FEBRUARY 22.

ROYAL SOCIETY, at 8.30.

ROYAL INSTITUTION, at 3.—On the Chemistry of Alkalies and Alkali Manufacture; Prof Odling, F.R.S.

SOCIETY OF ANTIQUARIES, 8.30.

CONTENTS

THURSDAY, FEBRUARY 22, 1872

THE ROCK THERMOMETERS AT THE ROYAL OBSERVATORY, EDINBURGH

THE whole of the observations made with these instruments (reading to hundredths of a degree Fahrenheit) from 1837 to 1869 having been reduced on a uniform plan, and found to exhibit some well-marked supra-annual cycles, a paper on the subject and on their relations to the sun-spot cycles of similar period but diverse shape was sent in to the Royal Society, London, on March 2, 1870.

Since then two eminent astronomers, one of them being Mr. Stone, the newly appointed Astronomer Royal at the Cape of Good Hope, and the other Mr. Cleveland Abbe, Director of the Cincinnati Observatory, have published somewhat similar deductions touching atmospheric temperatures in reference to sun-spots; Mr. Stone basing on thirty years of South African temperature observed by his indefatigable predecessor Sir T. Maclear; and Mr. Abbe on sixty years' temperature observed on the elevated station of Hohenpeissenberg near Munich, under the superintendence of Dr. Lamont, the Bavarian Astronomer Royal; both parties, equally with myself, using the same famous series of observations of sun-spots, as made by M. Schwabe, and discussed both by Prof. Wolf and Prof. Balfour Stewart. More recently still a Canadian writer, employing the returns of the Toronto Observatory for many years past, considers that he has established a connection between the amount of annual rainfall there and the sun-spots; and of these again with the periods and dates of several interlacing streams of circum-solar meteors. And within the last few days the Radcliffe Astronomer announces in his report for 1871 that the *mean* azimuthal direction of the wind at Oxford, rigorously computed from automatic records during the last eight years, varies year by year through a range of 58° on the whole, between *maximum* and *minimum* of visible sun-spots; the tendency of the wind to a westward direction increasing with the number of spots, and with such west wind, it is to be presumed, the amount of rain also.

These results touch closely on the hopes of physicists to render meteorology more of an exact science by getting at its cosmical relations, but they also touch equally close on another point where the highest science is at present completely dumb, although too it is the very point where the utmost amount of benefit might be conferred on the largest numbers of the people, viz., some approximate indications of the character of the seasons for a year or two beforehand; or indeed, very much as I did make a first attempt, for the two winters of 1870-71 and 1871-72, in the paper presented to the Royal Society in the spring of 1870.

How intimately the well-being of the poor generally, as well as of the agricultural classes, depends on those characteristics of weather which no scientific society can at present foretell, and no Ministry prevent in their destructive effects to the national revenue when they do come, the following letter may serve as a better example than anything that I could prepare on theory alone:—

"Webb's Green, Hales Owen, June 12, 1871
"To C. Piazzi Smyth, Esq., Edinburgh
(Copy)

"Sir,—I am a reader of *Chambers' Journal* and a farmer of some 600 acres. In the publication of Messrs. Chambers I read that you had expressed an opinion from certain observations you had made that the late winter would be very severe. For the general run of weather prophets I have very little respect; but every respect for opinions that are the result of scientific induction.

"Consequently I conducted my farming operations with due regard to your prognostication, and as the result has been a profit to me, I write to thank you. Gratitude has been defined as 'a lively sense of favours to come,' and from that view and in consideration of the present weather if you could give me your opinion of the weather that you think likely to prevail for some time to come I should feel much obliged.

"I have not troubled you with this epistle entirely from a selfish point of view, for besides being a farmer I am unfortunately an employer of a very underpaid class of workmen, hand rail makers.

"Now that stocks of wheat are exhausted, meat is a luxury to which railers cannot aspire; and if the season continues ungenial, before the harvest of 1872 there may be absolute scarcity of bread. I want to get up a fund for emigration, but if you could give me any information as to the probabilities of season that would dispel my gloomy anticipations for next winter, I should rejoice.—I am, &c., &c. (Signed) "THOMAS BISSELL"

But I have so little desire to incur responsibility for any weather predictions that I have gladly availed myself of the opportunity of the publication of the 13th volume of the Edinburgh Astronomical Observations to lay before the public by means of the several Plates 11 to 15 inclusive a complete graphical representation of the whole series of Edinburgh rock-thermometer observations, and on which I will merely venture the following explanatory remarks:—

1. The most striking and positive feature of the whole series of observations is the great heat-wave which occurs every eleven years and a fraction, and nearly coincidently with the beginning of the *increase* of each sun-spot cycle of the same eleven-year duration. The last observed occurrences of such heat-wave, which is very short lived and of a totally different *shape* from the sun-spot curve, were in 1834·8, 1846·4, 1857·8, and 1868·8, whence, allowing for the greater uncertainty in the earlier observation, we may expect the next occurrence of the phenomenon in or about 1880·0.

2. The next largest feature is the extreme cold close on either side of the great heat-wave; this phenomenon is not quite so certain as the heat-wave, partly on account of the excessive depth and duration of the particular cold wave which followed the hot season of 1834·8. That exceedingly cold period, lasting as it did through the several successive years 1836, 37, and 38, was, however, apparently a rare consequence of an eleven year minimum occurring simultaneously with the minimum of a much longer cycle of some forty or more years, and which has not returned within itself since our observations began. Depending therefore chiefly on our later observed eleven-year periods, or from 1846·4 to 1857·8, and from the latter up to 1868·8, we may perhaps be justified in concluding that the minimum temperature of the present cold wave was reached in 1871·1, and that the next similar cold wave will occur in 1878·8.

3. Between the dates of these two cold waves there are located, according to all the cycles observed, even including that earlier one otherwise exceptional, three moderate and nearly equidistant heat-waves, with their two intervening and very moderate cold waves, but their characters are quite unimportant as compared with what is alluded to under heads 1 and 2; and with regard to all the waves, it may be just to state that there has been in observation more uniformity, and will be therefore in prediction more certainty for their dates than for their intensities. C. PIAZZI SMYTH

February 1872

DARWIN'S ORIGIN OF SPECIES

The Origin of Species by means of Natural Selection; or the Preservation of Favoured Races in the Struggle for Life. By Charles Darwin, M.A., F.R.S. Sixth edition, with additions and corrections. (London: J. Murray, 1872.)

FEW are the writers, scientific or otherwise, who ca afford, in every successive edition of their works, to place side by side the passages which they have seen reason to alter, from a change of view or any other cause. And yet to this point we find especial attention called in each succeeding edition of Mr. Darwin's "Origin of Species." And herein lies the true humility of the man of science. Science is often charged with being arrogant. But the true student of Nature cannot be otherwise than humble-minded. That man is unworthy of the name of a man of science who, whatever may be his special branch of study, has not materially altered his views on some important points within the last twelve years.* The means at our command for obtaining correct views of the laws which govern Nature are ever increasing, and if we only

Let knowledge grow from more to more,

this can but cause that

More of reverence in us dwell,

reverence for the eternal constancy of Nature's laws, with respect to which we even yet know so little. But a false pride more often tempts men to conceal than to avow their change of opinion. Mr. Darwin carries the contrary practice perhaps to an excess. But such a course necessarily disarms criticism of its sting; and if the learner sometimes ventures to point out wherein he differs from the master's conclusions, it is only in the hope that the interchange of opinion may lead to a removal of the difficulties which prevent a complete accord of thought.

The sixth edition of the "Origin of Species" is considerably smaller than its predecessors; but this does not arise from any diminution of matter, but from the use of smaller type. There has been, in fact, considerable addition, and our province will be simply to call attention to those points in which previous editions have been amended or amplified. Already, in the fifth edition, Mr. Darwin had stated that the able criticism of his work which appeared in the *North British Review* had induced him to modify his views with regard to the frequency of the occurrence of characters which are not useful to the individual; we find now, on some other points, a similar modification of opinion.

It has always seemed to us that one of the weakest parts of Mr. Darwin's statement of the theory of natural selection is the emphasis with which he asserts that single instances of departure from the law would prove the theory to be unsound. In the present edition, speaking of the rattle of the rattlesnake—the only effect of which has been stated to be to direct to the snake the attention of its enemies—he goes out of the way to repeat that "if it could be proved that any part of the structure of any one species had been formed for the exclusive good of another species, it would annihilate his theory." Why it would annihilate his theory, we must confess we are unable to understand; since Mr. Darwin repeats in this edition even more emphatically than in previous ones that "he is convinced that natural selection has been the main, *but not the exclusive*, means of modification of species." Since then other causes have been at work to cause the evolution of species, why may not some of these causes be able to produce parts beneficial to the race rather than to the species? In the special case, however, under consideration, the rattle of the rattlesnake, an American naturalist comes to the rescue of the Darwinian theory. Mr. Darwin was probably not aware at the time of writing that Prof. Shaler had stated his belief, from the result of observation, that the rattlesnake's rattle is actually beneficial to it, its object being to imitate the sound of the cicada or other insect which forms the food of many birds, thus attracting them within its power, and accounting for the apparent "fascination" of its prey, which must now be consigned to the limbo of travellers' tales.

The greater part of the additional matter in this edition is naturally devoted to a reply to the objections urged in Mr. Mivart's "Genesis of Species." In replying to Mr. Mivart's objection to the theory that "mimicry" has resulted by the process of natural selection, on the ground that the early stages of resemblance would have no useful tendency, the following sentences appear to us to be open to objection, or to be wanting in clearness:—"But in all the foregoing cases the insects, *in their original state*, no doubt presented some rude and accidental resemblance to an object commonly found in the stations frequented by them." "Assuming that an insect *originally happened* to resemble in some degree a dead twig or a decayed leaf." What is meant by the "original state" of an insect? Every insect-form must have been evolved from some previously existing simpler form by a gradual process, and the "rude or accidental resemblance" must be due to the operation of the same causes that produced the finished likeness. We must acknowledge that Mr. Darwin appears to us to fail to grapple with the difficulty in the way of the application of his theory, that either the early stages of the "mimicry" are useless, or that the exact reproduction of figure and pattern in the "mimicing" insect is a mere freak of nature. Mr. Darwin states his belief that "the sight of birds is probably sharper than ours," which would tell heavily against the utility of the first approaches towards resemblance; Mr. Wallace, if we recollect rightly, has expressed a contrary opinion.

Mr. Mivart's objection with regard to the curious fact

* The first edition of the "Origin of Species" was published in 1859.

that in the Pleuronectidæ, or Flat-fish, the eyes are opposite in the young state, and afterwards become placed both on the upper side of the head—that this change must have taken place suddenly, since any small approach to it would not be useful—is met by an ingenious argument, previously advanced by Malm. It is stated that "the Pleuronectidæ, whilst still very young and still symmetrical, with their eyes standing on opposite sides of the head, cannot long retain a vertical position, owing to the excessive depth of their bodies, the small size of their lateral fins, and to their being destitute of a swim-bladder. Hence, soon growing tired, they fall to the bottom on one side. While thus at rest, they often twist, as Malm observed, the lower eye upwards to see above them, and they do this so vigorously that the eye is pressed hard against the upper part of the orbit. The forehead between the eyes consequently becomes, as could be plainly seen, temporarily contracted in breadth. On one occasion Malm saw a young fish raise and depress the lower eye through an angular distance of about 70°."

The objections urged by Nägeli in his "Begriff und Entstehung der naturhistorischen Art," with respect to plants, that the families of plants differ chiefly from each other in morphological characters, which appear to be quite unimportant to the welfare of the species, are combated on the ground that we ought to be exceedingly cautious in pretending to decide what structures now are or have formerly been of use to each species. While admitting that in earlier editions he underrated the frequency and importance of modifications due to spontaneous variability, Mr. Darwin points out that many peculiarities of structure, lately supposed to be simply morphological, are now known to be intimately connected with facilities for fertilisation.

On the whole it seems to us that each succeeding edition of the "Origin of Species" lessens the distance between Mr. Darwin and those who believe that the influence of natural selection, though a *vera causa*, has been overrated as an element in the evolution of species. If it is admitted that important modifications are due to "spontaneous variability," that natural selection is not the exclusive means of modification, Darwinians and non-Darwinians have equally before them the problem to discover what these other laws are which are co-efficient in the production of new species, and what part each of these plays in producing the final result. Until this is accomplished we can hardly consider the great problem of the Origin of Species as solved. Towards the solution of it, however, the labours of Mr. Darwin will ever be held as having contributed a larger share than those of any other naturalist. When we look at the title-page, and see that a work which has produced a greater revolution in the scientific thought of the day than any published in this country since Newton's "Principia" is yet only in its eleventh thousand, and reflect that, although this is not a small sale for a scientific work, yet books which contain the germ of no new thought, and contribute not one iota to our sum of knowledge, have sold their hundreds of thousands, we cannot but think that in the coming age, when the people will really care about science, our descendants will regard this unworthy fact in the light that we do the unpopularity of the writings of Milton and Goldsmith during their lifetime.

We must not omit to mention a very useful addition, for the unscientific reader, made to this edition, in the shape of a glossary of the principal scientific terms used, prepared by Mr. W. S. Dallas.

ALFRED W. BENNETT

MAXWELL ON HEAT

Theory of Heat. By J. Clerk Maxwell, M.A., LL.D. (London: Longmans and Co. 1872.)

IT is very seldom that we meet with a book so instructive and delightful as Prof. Maxwell's "Theory of Heat." It has peculiar claims upon the student of Physics, inasmuch as it supplies a want which has been long and widely felt. The point of view is undoubtedly a new one, and to enable our readers to perceive the value of the book, we ought to make a few remarks upon the kinds of text-books that we have hitherto had. In these books the aim has been to inform the student's mind, and the fault to inform it too minutely and too exclusively. They have been of two classes—elementary books, in which the information is given in a popular manner, and advanced books, through the pages of which mathematical formulæ are very liberally interspersed.

In reading such a book the strength of the student's mind is devoted to one or at most two objects. If the book be elementary, he is bent upon acquiring a good knowledge of the facts, along with a knowledge, more or less complete, of the experimental methods by which these facts have been obtained. If, on the other hand, the book be an advanced one, his strength is devoted to grappling with and overcoming its analytical difficulties. But after he has studied both classes of text-books, he rises from their perusal with the belief that there is something wanting before he can have a thorough grasp of the subject, and a clear view of its truth and beauty. He has followed the experimenter only too zealously into his elaborate and accurate calculations, or it may be the mathematician into his profound investigations, and he now begins to realise the truth of the poet's saying—

> He who hath watched, not shared, the strife
> Knows how the day hath gone,

and to sigh for some elevated spot from which he may obtain a clear view of the whole field. He hears vague rumours that the caloric battalions and their allies the corpuscular forces, have lost the day, but he wishes to see their discomfiture more completely with his own eyes.

Such a point of view is afforded by Prof. Maxwell. He has—wisely, we think—confined himself to this one object, to give the student a clear logical view of the whole subject; nor has he broken the unity of his treatment by going into details, whether experimental or mathematical. Every true student of physics should read this book, and he will unquestionably find it a most delightful study. He will, we venture to say, rise from its perusal with a much truer and wider conception of the science of heat; and if he then wants more detailed information upon any branch, he may consult one of the ordinary text-books. Another beauty of the book is the accuracy and completeness of its historical notes. The author has successfully combined the part of historian and that of logician, and has given us very many valuable references to original memoirs, in which we may see for ourselves the first germs of the various developments. The only thing wanting in this respect is an index, into which the various facts and names of the book might have been collected with much advantage to the reader.

Another point of interest in the book is the prominence given to the graphical method of representing truth. The Isothermal and Adiabatic curves are largely dwelt upon, and their usefulness in leading us to detect new properties of bodies is well pointed out. We are glad to think that the importance of such graphical representations is becoming well recognised in many departments of science. Even in pure mathematics, if we have occasion to calculate a series of numerical values from a formula, by plotting them upon curve-paper we shall discover at once by the eye if we have made a mistake in our calculation. In like manner, if we plot the result of a series of careful experiments after the manner of Regnault and others, we shall probably be able to determine from the appearance of the curve whether or not we may trust to the accuracy of our determinations.

Finally, by a series of lines similar to those exhibited by Prof. Maxwell, we come to see with great ease the relation that exists between the various properties of bodies; for instance, we see at once and as a direct consequence of the definition, that the ratio between the two specific heats is the same as that between the two elasticities.

We cannot close this review without remarking upon the good English in which this excellent book is written; and this, we trust, will go far to convince the scientific public that the most profound and original treatment of physics is not inconsistent with purity of language.

B. STEWART

OUR BOOK SHELF

Queen Charlotte Islands: A Narrative of Discovery and Adventure in the North Pacific. By Francis Poole, C.E. Edited by John W. Lyndon. (London: Hurst and Blackett, 1872.)

MR. POOLE enjoys the distinction of being the only educated Englishman who has ever lived on Queen Charlotte Islands, where he spent two years in an endeavour to develop the mineral resources of the country. The volume therefore necessarily possesses the interest attaching to a narrative of a residence in an almost unknown country. We miss, however, those touches which add so much to the charm of books of travel, which indicate that the writer has visited many men and many cities, and is capable of contrasting the natural products or the habits of the people of one part of the world with those of another. The attraction for the author to these islands was the presence of copper, to work which a company was formed in 1862. There can be little doubt that copper-veins, and probably other minerals, do exist in the islands in quantities that would amply repay the investment of labour and capital in their working. The climate appears to be equable and agreeable, the harbours are magnificent, and the soil is rich and productive, so that we may hope that at some future time Queen Charlotte Islands will become a valuable dependency of the British Crown. If Mr. Poole's volume succeeds in drawing to their capabilities the attention of those who are competent to develop their resources, it will have performed good service.

Hints and Facts on the Origin of Man, and of his Intellectual Faculties. By Pius Melia, D.D. (London: Longmans and Co., 1872.)

THE writer of this little book states in his preface that "he has brought together systems, facts, statements, and reasons, taken from all available sources, with the view of elucidating several important truths about man, which are at the present day either called in question or absolutely denied." The extent to which he has consulted, or the accuracy with which he has quoted from, original sources, we gathered from the fact that he entirely passes over, as unworthy of notice, the systems of Goethe and Oken, and from the statement that the "Philosophie Zoologique" of G. B. Lemarck (*sic*) was published in 1830.

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 Zoological Station at Torquay

THE article on "The Formation of Zoological Stations," by Anton Dohrn, which appeared in NATURE of the 8th inst., was read at the meeting of the Torquay Natural History Society on the 14th inst., and was the subject of an animated conversation. I am happy to add that the scheme met the warm approval of the members, and that if a station be established at Torquay, the cordial co-operation of the society may certainly be reckoned on.

W. PENGELLY, Hon. Sec.

Museum, Torquay, Feb. 17

The Chicago Observatory

A LETTER, signed by one of the Professors of the University of Chicago, commenting on the impoverished state of the Chicago Observatory since the great fire in that city, having had an extensive circulation through the Press, I have to request the favour of the insertion in your columns of the following statement on the subject, just received by the Secretary of the Royal Astronomical Society from the Director of the University, Prof. T. H. Safford.

EDWIN DUNKIN,
Hon. Sec. to the Royal Astronomical Society

Royal Observatory, Greenwich, February 22

"Dearborn Observatory, Chicago, Jan. 29, 1872

"DEAR SIR,—As the enclosed article from the London *Daily News* (see also London *Times* of Januaiy 9) might convey the impression that the Observatory is to be closed, permit me to state exactly the facts.

"The Observatory—whose funds are separate from those of the University—has, during the few years of its existence, accumulated a large stock (perhaps too large) of unpublished and only partially discussed observations, especially upon stars between 35° and 40° of declination, in conection with the German Astronomical Society, on Argelander's plan. A few months before the fire arrangements had been in progress by which it would gradually acquire the means to discuss and publish these observations, and these arrangements have been interrupted.

"So far, then, as the City of Chicago is concerned, nothing further is to be expected for the present, and, perhaps, the coming year; but as business has revived, it is expected that the difficulty of providing means will not be permanent.

"For the present it is necessary for me to give a portion of my time to geodetic and geographic-astronomical work for the United States engineers, who are conducting large operations in the central portion of the country; and the publication of our observations will be in consequence delayed.

"It is but fit that I should here acknowledge the indebtedness of the Observatory to the Hon. J. Young Scammon, at whose sole expense the Dearborn Tower and the Meridian Circle Room were built, and upon whom the support of the Institution has mainly depended.

"Our thanks are especially due to those scientific friends who have so kindly given their works. Were it not for the Greenwich and other star-catalogues received by past donations, I should have found myself in no condition to accomplish the work which I am now doing for support.

"T. H. SAFFORD,
"Director of Dearborn Observatory

"To the Secretary of the Royal Astronomical Society."

Composition of Vibrations

WHILE holding one of König's large polished tuning-forks in my hand, I happened to give it a swaying movement on the plane on which its vibrations were being performed, and immediately noticed that the space through which the fork swung was occupied by a series of bright straight lines arranged in a fan-like form. The lines spread out, or drew together, as the rate of movement impressed on the fork increased or diminished. The case was clearly one of composition of vibrations, the bright lines being merely the edges of the prongs seen in positions of instantaneous rest, where the proper motion of a prong was equal and opposite to that communicated to it by the hand.

By taking forks of different pitch, and causing them to swing with equal velocities, the dependence of pitch on the number of vibrations performed in a given time was easily exhibited.

In case this simple observation has not yet been made or described I ask its insertion in NATURE.

Trinity College, Cambridge SEDLEY TAYLOR

Eclipse Photography

MR. J. BOESINGER, in the last number of NATURE, expresses his surprise at the ignorance of the photographers attached to the late expeditions, and favours them with hints, observations, and instructions still more surprising. Because he cannot see their reasons for employing equatorial stands, plates in separate frames, and long exposures, he concludes these were unnecessary; and affirms "there must have been a great want of balance in their chemicals." No doubt there is a want of balance somewhere, and I diffidently submit the probability that Mr. Boesinger has lost his.

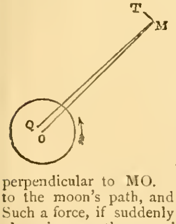
I would briefly state to those few of your readers who may have been misled by this correspondent, that equatorial stands driven by clock-work are absolutely necessary in the production of the best results, either by short or long exposure of photographic plates; a picture "not perfectly sharp but valuable as a memorial," was what Mr. Boesinger aimed at (and I sincerely hope he obtained it), but the expeditions had higher aims and greater expectations. Single large plates were exposed separately, that should a corona extending many degrees be actinically present, it might find ample room to put in an appearance; in such a case had "repeating backs" been used to give many pictures on one plate, there would have been great danger from the corona of one picture over-lapping that of another, to the ruin of all. Comparatively long exposures were found necessary to secure impression from the faint extremities of the rays.

HENRY DAVIS

Tidal Friction according to Thomson and Tait

I AM so afraid that this letter will convict me of hopeless stupidity that I conceal my name. For I am going to confess that I do not understand, and even feel inclined to dispute, the reasoning of Thomson and Tait, on pp. 191-194 in their great work, respecting the effect of tidal friction on the motion of the earth and moon. It will be a convenience to your readers if I quote the passage at full length:—

"Let us suppose the moon to be a uniform spherical body. The mutual action and reaction of gravitation between her mass and the earth's will be equivalent to a single force in some line through her centre, and must be such as to impede the earth's rotation as long as this is performed in a shorter period than the moon's motion round the earth. It must therefore lie in some such direction as the line MQ in the diagram, which represents, necessarily with enormous exaggeration, its deviation, OQ, from the earth's centre. Now, the actual force on the moon in the line MQ, may be regarded as consisting of a force in the line MO towards the earth's centre, sensibly equal in amount to the whole force, and a comparatively very small force in the line MT perpendicular to MO. This latter is very nearly tangential to the moon's path, and is in the direction *with* her motion. Such a force, if suddenly commencing to act, would, in the first place, increase the moon's velocity; but after a certain time she would have moved so much farther from the earth, in virtue of this acceleration, as to have lost, by moving against the earth's attraction as much velocity as she had gained by the tangential accelerating force."



The consequences are then shown to be that the moon's distance would be increased in the ratio 1 : 1 . 46, and her periodic time increased, and the earth's period of rotation lengthened.

This reasoning perplexes me; for if the effect of a certain amount of fluid friction is to throw the line of action of the force from MO to MQ, a fluid friction is conceivable which should throw it outside the earth altogether. Moreover, the line of attraction of the earth on the moon would be in a line not passing through the earth's centre, a result I cannot understand, especially if the fluid friction were increased as just suggested. Nor can I see that a force in MQ, the centre of the earth being free, would tend to stop the rotation of the earth.

As I view the matter, fluid friction generates a *couple* tending to stop the rotation of the earth, and it is impossible to combine this couple with the force in MO, and represent the resultant by a single force. The energy lost in the form of momentum of rotation of the earth is gained in the heat devolved by the fluid friction, which is ultimately dissipated. And the final result would be that the orbit of the moon would not be appreciably altered, while the period of rotation of the earth is gradually lengthened.

Am I wrong, for the thousandth time in my life? and if so will some one try and enlighten me. Perhaps Prof. Tait will spare a few minutes to an old friend. M. A.

Circumpolar Lands

IN NATURE (Feb. 8) Mr. Murphy seems to admit the soundness of the reasoning by which I endeavoured to show (Jan. 25) that the earth's form is probably undergoing a slow progressive change, but he thinks that the statements in the first and last parts of my letter are contradictory.

If Mr. Murphy will be good enough to read again the paragraph immediately following the one which he quotes, I think he will find that there is no contradiction. "Transmission of pressure towards the poles" must tend to elevate the land in those regions. How that pressure is produced and transmitted I have endeavoured to show in the same paragraph.

However, the main proposition which I sought to establish in my paper of 1857, before alluded to, is that any spheroid of equilibrium, whether earth, sun, or any other, in motion about an axis, in cooling from a fluid state, undergoes a change of form, and with this proposition Mr. Murphy seems to agree.

Mr. Murphy has inadvertently omitted part of a sentence in making his quotation from my letter, thus representing me as speaking of a *ratio* with *one* quantity only.

Queen's Coll., Liverpool, Feb. 16 GEORGE HAMILTON

The Spheroidal State of Water

I HAD the pleasure a few days ago of visiting Messrs. Johnson's celebrated iron wire manufactory in Manchester. There may be seen a series of furnaces and rolling mills which in twenty-four hours can convert a truck load of the best Swedish iron into the bright and polished galvanised wire which is now being so extensively employed to complete our very perfect system of Post Office telegraphs. Every stage of the process passes beneath the eye of the observer; the melting of the pigs, the formation of the billets, the puddling of the bloom, the shingling of the balls, the rolling of the bars, and their subsequent extension by further rolling, and drawing into telegraph wire.

The bars are cut off into 10ft. lengths, and are placed in a Siemen's regenerative furnace, where they are raised to a brilliant white heat. They are then drawn out of the mouth of the glowing furnace, and pass through a series of consecutive rollers of varying dimensions, and rotating with varying speed, ultimately flowing out in a continuous stream of iron wire. In fact, the metal is at such a high temperature and so plastic that the curves it takes in falling convey the idea of a thin, fine unbroken jet of liquid matter.

The rollers are kept cool by the constant play upon them of jets of water. The first pair of rollers is fixed close to the mouth of the furnace, which is partially closed by a moveable screw that is only raised when the attendant sprite requires to direct another bar to the attenuating process of the continuous rollers. The jet of water that cools the first pair of rollers in one furnace fell in a broken shower upon the foot-plate of the mouth of the furnace, which, from its proximity to the fire, was raised to a very high temperature, and therefore converted the drops of water into the spheroidal state. There they bounded and danced and rolled about like pith balls under an excited electrical receiver. Their constant rotation and well-known rippling motion gave them an opaque appearance which caused them to resemble closely a fine fall of hail. In fact, those to whom I pointed out the phenomenon likened their appearance to a fall of dusty snow at the mouth of a furnace. The sight was very striking and interesting. The workmen had taken these spheroids to be particles of scale and dust swaying about in the currents of air at the mouth of the furnace.

I have seen many times the experimental illustration of "Leidenfrost's phenomenon" at the mouth of a furnace, but I had never before seen its practical, though accidental, development, and in the incident which I have narrated above the interest chiefly attaches to the great antithesis of the fact and its appearance—snow at the mouth of a fiery furnace.

W. H. PREECE

The American Eclipse Expedition

I DEEM it but proper and just that I should correct a mistake that has just met my eye in Dr. Schellen's excellent work on Spectrum Analysis.

On page 332 of the 2nd German edition we find "Die erstere Expedition wählte unter der Anführung von Professor Morton die Stationen im Staate Iowa.

"(1) Burlington mit den Beobachtern Professor Mayer, Kendall, Willard, Phillips, und Mahoney, denen sich der als gewandter Spectroskopist bekannte Dr. C. A. Young, Professor am Dartmouth College (Hanover), und *Dr. B. A. Gould für die photographischen Aufnahmen hinzugestellen.*"

In the English translation, edited by Mr. Huggins, the above reads, "The first expedition, under the guidance of Professor Morton, selected stations in the State of Iowa as follows:—

"(1) Burlington, where its observers were Professor Mayer, and Messrs. Kendall, Willard, Phillips, and Mahoney, together with Dr. C. A. Young, Professor of Dartmouth College (Hanover), well known as an experienced spectroscopist, and Dr. B. A. Gould, to whose charge the photographic department was committed."

Dr. Gould had no connection with the photographic expedition, but placed himself under Professor Coffin's general organisation, so that he could have facilities for making observations on the corona, and in searching for the suspected intermercurial planet.

The Burlington station of the Philadelphia eclipse expedition was placed under the direction of Dr. Mayer, and the photographs pointing page 337 of Dr. Schellen's work are two of the five plates secured by him during totality.

Also the diagram on page 338 is from Dr. Mayer's report on the eclipse (published October 1869), an abstract of which, with accompanying copies on glass of the original negatives, was presented by M. Delaunay to the Institute of France. The Rev. T. W. Webb laid them before the Royal Astronomical Society, when the report and the photographs were discussed at length at the meeting of November 12, 1869.

HENRY MORTON, President

Stevens Institute of Technology, Hoboken, New Jersey

Mr. Spencer and the Dissipation of Energy

WILL you permit me to inquire, for the instruction of the many who are familiar with Mr. Herbet Spencer's "Doctrine of Evolution," and especially in regard to "First Principles," sec. 58, referred to by Mr. Spencer in his paper in your number for February 1, if the theory of the "Dissipation of Energy" does not upset a very considerable and significant portion of Mr. Spencer's "First Principles"?

WILLIAM SMYTH

Maidstone, February 12

THE AURORA OF FEBRUARY 4

ON Sunday, the 4th inst., was witnessed one of the most magnificent displays of aurora which have been seen in Europe within the past twenty or thirty years. To most observers in this country it appeared equal in magnificence to the two fine auroræ seen on Oct. 24 and 25, 1870, and which were especially grand in England; but foreign observers could only compare it with those seen in 1831 and 1836. But if we take all the attendant phenomena into consideration, it will appear that, whilst others may have equalled this one in grandeur and beauty, there is not one which can compare with it either as to the wide extent of country over which it was visible, or as to the strangeness of many of the phenomena by which it was accompanied. The numerous letters which have appeared in these columns the last two weeks show how universally it was noticed in England, Scotland, and Ireland; but in addition to these, the letters and telegrams which have appeared in the daily and weekly papers—both English and foreign—show that it excited attention over a still larger area. It is difficult to trace the exact limits of this area; but when we mention Paris, Cologne, Berlin, Malta, Constantinople, Egypt, and India, it will be seen what a large extent of country is embraced. So far we have seen no account of it as having been visible in the extreme north of Europe, as in Iceland, Norway, Sweden, St. Petersburg, &c., where most auroræ boreales are so well displayed; but, on the contrary, many of the cities in which it was noticed are those which are commonly supposed to be too far south for such phenomena to be seen. The importance of this point will appear later on.

To take England first. Mr. Allnatt sends to the *Times* a long description of the appearance of the aurora as seen by him at Frant, which shows that it was first noticed at 6 P.M. in the S.W., and that by 7 o'clock it had reached the zenith. It disappeared at 7.45, but reappeared for a short time at 10.50 in the N.; but "at 7.30 P.M. the whole heavens were pervaded by this abnormal southern aurora, that had now expanded universally and dipped its supplementary bands into the northern horizon." He also writes:—"The earth's electricity was so powerful, that the gold leaves of the electrometer remained diverged for a considerable time!" Other correspondents describe it as seen at Blackburn, in Lancashire, at 7, "embracing the whole southern sky from N.E. round to W.;" from Faversham, in Kent, as visible between 9 and 10 o'clock;" from Cambridge as having its maximum intensity about 10; at Swindon as commencing at 10 minutes past 7 and lasting till 10 o'clock, "and giving as much light as a full moon, every object being clearly visible." But many observers had noticed it at times considerably earlier than those just mentioned: thus, "J.S.H.," writing in NATURE last week from Gloucester, "observed it at 5.30, just in the twilight, but it was then confused with the rays of the setting sun; but as the darkness deepened the aurora came out alone, and was then extremely beautiful." But still earlier was it observed at Hartlepool, whence a correspondent writes, at 5 o'clock:—"The whole of the southern sky was tinged with a most beautiful rose colour, which, as darkness set in, extended towards the zenith, where it culminated in a brilliant corona." This very early manifestation of the aurora partakes very much of the nature of a "day aurora," the possibility of which has been so much discussed in these columns (*vide* NATURE, vols. iii. and iv.) To us there does not appear much difficulty in believing that these grand meteorological phenomena, whatever their cause may be, are independent of merely relative time, and that the reason why they are mostly observed at night is because the purely local circumstances are then most favourable to their observation. That an aurora should wait till night-time before it manifests itself hardly seems probable, whilst, on the other hand, that the more brilliant light of the sun should prevent auroral displays being seen in the day-time is not only probable but is borne out by what we know of the light of the stars and planets. No one believes that stars only shine at night-time, why then should there be a belief that auroral displays take place only at night-time, especially when it is remembered that the experiences of polar travellers in their sunless regions are distinctly against it? But this is a digression arising from the fact that in comparative daylight we have distinct and independent evidence of this aurora having been observed. In addition to those already given, from Worcester we learn that it was noticed "shortly before 6 o'clock in the twilight, when thin fleecy clouds of a bright rose colour were observed in the South and East," whilst correspondents of the *Kölnische Zeitung* state that it was first noticed at Cologne about 6 o'clock, and at Bonn about "half-past 5, gradually becoming more and more marked till 6 o'clock, when do doubt was left as to its true auroral character." While there is thus clear evidence tbat tbe phenomena had commenced some time before 6 o'clock, there is, as might be expected, great diversity as to the time when it was last visible. That this should be the case is only natural, and is entirely dependent on purely local circumstances—the state of the weather, the cloudiness of the sky, &c. Thus, whilst in some the aurora first appeared at 6 o'clock, to others it was not visible till between 7 and 8; and whilst in some places it disappeared about 8 or 9, it was then in others in its most brilliant state. But, taken as a whole, it appears to have lasted the whole evening until quite late at night; thus a correspondent, writing to the *Pall Mall Gazette* from Autun, states that "at midnight the East was crimson, and it was so light that I could tell the time easily, although my watch has gold fingers, and strong shadows were cast in rooms whose windows faced the East."

We have thus evidence of the aurora having commenced about 5 o'clock, and continuing at least till midnight, and probably later. But before proceeding to notice the other attendant phenomena, we would direct attention to a passage in the letter of the correspondent of the *Pall Mall Gazette*, before alluded to, which confirms the hypothesis that the accounts of "showers of blood," &c., mentioned in ancient chronicles were in reality only auroral displays. He writes, "all these signs and wonders produced a considerable effect upon the peasantry, who see in them warnings of a coming war; they always connect the idea of a red aurora with bloodshed." Comparing, then, all the varied accounts to which we have referred, we find very general agreement with regard to certain phenomena, some of which are of very remarkable cbaracter. The first of these is that when the aurora was noticed by those who observed it early in the evening, it appeared in the Southern and South-Western horizons, thence it seems gradually to spread, and finally appeared later on in the evening in the Northern and Eastern horizon. That this was the case is shown by the agreement of the accounts, some of which we have already quoted, and many more of which might be given. Thus at Bonn, "nothing remarkable was to be noticed on the northern horizon, whilst on the southern lay the dense, greyish bank of clouds, whence auroral streamers shortly ascended." There can also be little doubt that during the middle of the evening, and towards midnight the chief seat of the display was to the north and east, as shown in the letters of those who first observed the phenomena at about 7.30 to 9 o'clock, and continued to do so till towards midnight. The second well-marked phenomenon was that between 7 and 8. There appeared a brilliantly-coloured arch, extending across the heavens from S.W. towards the north and east. Thus at Autun we have described "a splendid and perfect arch, spanning the sky from a point on the south-eastern horizon to one on the south-western, and which lasted, more or less continuously, for two hours, whilst from 10 to 12 the sky became gradually less luminous in the south, and grew more and more splendid overhead. Till about 11 the two eastern and western auroras united in a vast arch overhead, with tongues of green flame darting through a suffused crimson." Similarly other accounts, with merely local variation. The third well-marked phenomenon appears to have been the formation of a "corona," nearly, if not quite, in the zenith, whence auroral rays streamed out in all directions. At some places this was more marked than at others, but is more or less universally noticed, both by English and foreign observers. Thus at Cardiff it is reported that "a corona, having rugged, sharply-defined edges, stood out prominently in the zenith, apparently on a parallel plane to the earth, and having its centre almost immediately over the head of the spectator, rays from which extended to the N.E. and N.W. horizons." If one may venture to say so, most auroræ visible in our latitudes appear to commence in general by an accumulation of cloud masses towards the magnetic north, then coloured masses slowly appear, and afterwards rays, or streamers, are sent up from this northern horizon towards the zenith. Sometimes the coloured masses themselves rise toward the zenith, and there the streamers pass in all directions. But in this aurora of the 4th of February, all the most marked phenomena are directly contrary to our ordinary experience, and should therefore be carefully noted. It is an extremely interesting inquiry to ascertain whether on the evening of the 3rd or 4th instant a brilliant Aurora Australis was visible in the southern hemisphere. If we consider the wide extent of country over which the aurora which we are describing was visible, the probability becomes very great that this will be found to be the case. The question then arises, Was the aurora of Feb. 4th, appearing as it did first in the southern horizon, an Aurora Australis or not? It is impossible to answer this question definitely; but we would throw out the following suggestion:—Knowing the ultimate connection that there is between northern and southern auroræ, and the fact that one of any magnitude rarely happens without the other, may we not have seen the last traces of a grand Aurora Australis, which gradually died away, whilst at the same time an Aurora Borealis was in process of formation, and which appeared in its full brilliancy in the northern and eastern horizon towards the latter part of the evening? We would make this suggestion with all due deference, but it seems to us to account in a fairly satisfactory manner for most of the very unusual and peculiar phenomena noticed, viz., the first appearance of the aurora in the south, the grand arch, the corona in the zenith, and the final disappearance in the north. We must also remember that in what is called the correspondence of northern and southern auroræ, there must be at least twelve hours difference as regards time. So that if there was an Aurora Australis on the same day, it would be dying out at the time our display was commencing.

In conclusion, the wide extent of country over which this aurora of the 4th February was visible, is easily shown. In Paris a "magnificent aurora" is reported, at Nancy and Chaumont there was a "brilliant display," while the Franco-German telegraph lines were greatly disturbed. At Constantinople one telegram states that "a splendid aurora, extending over half the heavens, was visible for several hours;" whilst another states that it was seen "from 10 till half-past 1." From Alexandria we hear that "a large space of the skies was illuminated for five hours." That it was visible at Malta, Suez, and Bombay, the following interesting account shows. It is supplied by Mr. George Draper, of the British Indian Submarine Telegraph Company, under date of Feb. 5th, and it also shows how powerful were the "earth currents" which were noticed in connection with this most brilliant aurora. He writes:—

"It may interest your readers to know that the brilliant aurora which was visible in London last night was also visible at Bombay, Suez, and Malta. Our electrician at Suez reports that the earth currents there were equal to 170 cells (Daniell's batteries), and that sparks came from the cable. These electrical disturbances lasted until midnight, and interrupted the working of both sections of the British Indian cable between Suez and Aden, and Aden and Bombay. Since Thursday last the signals on the British Indian cables have been very much interfered with by electrical and atmospheric disturbances, causing considerable delay in the transmission of messages, which all efforts failed entirely to overcome. Our superintendent at Malta also reports that there was a very severe storm there yesterday morning, so much so that they were compelled to join the cable to earth for several hours. He also reports the aurora as very large and brilliant. The electrical disturbances on the cables in the Mediterranean, and on those between Lisbon and Gibraltar, and Gibraltar and the Guadiana, were also very great. The signals on the land line between London and the Land's End were interrupted for several hours last night by atmospheric currents."

Taken, then, on the whole, this aurora of February 4th was one of the most brilliant, most interesting, and most widely visible which has been witnessed for many years past, and is probably one that will cause renewed attention to be paid to the still unsolved problem of their causes.

J. P. EARWAKER

[We have also received the following from J. W. Spengel of Berlin: — "At Berlin, the sky being covered by clouds, no one could see anything. But a young astronomer of our observatory told me that he had recognised the existence of a mighty aurora by means of the spectroscope. The magnets were also vehemently disturbed, and all the telegraphs failed for several hours. The following appears in the *Leipziger Allgemeine Zeitung* for Feb. 8:—'Freiberg, Feb. 6. The aurora observed by many on the evening of Sunday caused here a complete interruption of communication through the telegraph wires for some time. The intensity between 5·40 and 6·45 overcame the strength of the battery at this station, so that it was not possible to change the oscillations of the magnetic needle caused by the earth-stream. After the northern light had become fully developed the oscillations became stronger, and followed one another at short intervals until the phenomena entirely disappeared about 7 P.M.' At Warmbrunn in the Riesengebirge, the aurora was seen magnificently from 6 to 8·30. Towards 10 it had almost disappeared. The thermometer indicated 0° C., with a violent storm from the south-west. About 11 the storm suddenly subsided; the thermometer fell to − 1·5°, and the aurora appeared for the second time in the same manner and with the same uninterrupted play of colours as at 6. After 11·30 the storm recommenced, and the aurora disappeared soon after 12. The play of the aurora on the snow-covered mountains is described as one of the most magnificent sights that can be conceived."—ED.]

REFERENCE SPECTRUM FOR THE CHIEF AURORA LINE

WHILE Nature herself seems to delight in surrounding some questions with triple difficulties and mysteries almost inscrutable, there are other questions which she has made the easiest of the easy if men will only use the means which she has prepared. And amongst such easy questions, no more signal example can be quoted than the exact spectrum place, within very narrow limits indeed, of Angström's yellow-green aurora line, whenever any aurora at all appears.

This chief aurora line coinciding precisely (as I believe I may say from my own observations, though by means of the roughest of home-made apparatus) with the second line, at W.L. 5579, of the citron band of the blue base of flame, from any and every material used for artificial illumination by man, and having immediately on one side the 1st line, of the same strength with itself, at W.L. 5630, and on the other side the fainter 3rd line, at W.L. 5535, of the same citron band; the smallest variation of spectrum place in the aurora line can be instantly perceived by the eye on this chemical scale, without the aid of any mensuration apparatus.

And yet in your last impression a respectable spectroscopist, after much labour, informs the Academy of Sciences in Paris, on Feb. 5, that Angström's yellow-green aurora line is somewhere close to Fraunhofer's solar line E, *i.e.* W.L. 5269; and in your previous impression a returning Indian observer considers the same Angström line to be somewhere near F, or W.L. 4860. Now, not only are these statements in error to the extent of from 30 to 70 times what they need be, but they cruelly drag us backwards in what should be the always onward course of science, and cause men to flounder once again in that slough of confusion they were immersed in a couple of years ago, when the chief solar corona line, at W.L. 5316, and Angström's grand aurora line, at W.L. 5579, were stated to be one and the same line, in the same place.

Excuse may, indeed, be proffered for these two observers, that they did not know of such a convenient night reference-spectrum as that which I have now alluded to; and then comes the question as to whose fault was that.

A full description of the method (after extensive trial for several months) was sent by me to the Royal Astronomical Society on May 30, 1871, with the particular request that the paper might be read at their June meeting and printed in the June Monthly Notice. This was mainly with the hope of supplying some possibly useful hints to the intending eclipse-corona-observers of December. The paper, however, though taken in, was neither read at the June meeting (if I am rightly informed) nor did it appear in the June Monthly Notice; but was handed over to secret referees, who simply sat upon it during six long months—or until the eclipse was safely past, and then they began to hint about possible objections being likely to be taken against some parts of the paper.

Of course I could not allow so admirable a society to run any risks of which they were afraid on my account; so I withdrew the paper thereupon, and am now engaged in publishing it myself, sustained in so doing by the hope that, although the eclipse for which it was mainly intended is irretrievably gone, its pages may yet be useful to some spectroscopists of aurora; and, in fact, that through their influence certain of both French and English observers will cease to attempt comparing the faint aurora's chief line with a bright solar spectrum, which they can never see in combination therewith (and if they could it has no coincident lines), but with a cheaply-procured chemical spectrum, which only comes well into view under the darkness of night, and is gifted by Nature in the spectroscope with an easily recognisable line in apparently absolute coincidence with the cosmical line of Angström.

C. PIAZZI SMYTH

15, Royal Terrace, Edinburgh, Feb. 16

AMERICAN DEEP-SEA SOUNDINGS *

UNDER the title at foot a pamphlet of thirty-three pages, accompanied by a large chart, and illustrated by several diagrams and tables, has been issued. The school-ship *Mercury* is a vessel belonging to the commissioners having in charge the hospitals and prisons of New York city, and is employed for the purpose of training boys, committed by the magistrates for vagrancy and slight misdemeanours, to become thorough seamen. Instead of growing up to be a curse to the community, such boys are made into valuable men. The adventurous life has a special charm for them.

An essential feature of the discipline on this ship is to make long cruises, by which the boys are fitted quickly to enter into the service of the navy or mercantile marine. Of 258 boys carried out on this voyage, 100 were on the return of the ship, in the opinion of the captain, capable of discharging the duties of ordinary seamen.

The commissioners, in addition to the above object, desiring to advance the interests of science as far as lay in their power, instructed the captain, P. Giraud, to obtain a series of soundings on the line of or near the equator, from the coast of Africa to the mouth of the Amazon, to observe the set of the surface currents and the temperature of the water at various depths. He was also directed to bring home specimens of water and of the sea bottom.

The ship sailed on December 20, 1870, and arrived at Sierra Leone on February 14. On February 21 she left

* Cruise of the school-ship *Mercury* in the Tropical Atlantic, with a Report to the Commissioners of Public Charities and Correction of the City of New York on the chemical and physical facts collected from the deep-sea researches made during the voyage of the nautical school-ship *Mercury*, undertaken by their order in the Tropical Atlantic and Caribbean Sea, 1870–71. By Henry Draper, M.D., Professor of Analytical Chemistry and Physiology in the University of New York.

Sierra Leone, and the soundings and other observations were continued till she reached Havanah, April 13, 1871.

The papers, together with the various specimens, were placed in the hands of Professor Henry Draper, of the New York University, for examination. His report commences by stating "that much attention has recently been given to deep-sea researches in consequence of the investigations made by the United States government on its coast, and by Dr. Carpenter. Mr. Gwyn Jeffreys, and Prof. Wyville Thomson, in the North Atlantic and Mediterranean Sea. Not only have many of the facts so ascertained been corroborated by this voyage of the *Mercury*, but the commissioners, by authorising it, have added much that is new and interesting to our knowledge of the physical condition of the deep sea."

Then follows a discussion of the barometric variations, in which it is show that they were very small in crossing the ocean, the minimum being only $\frac{13}{100}$ below, and the maximum $\frac{8}{100}$ above the mean. In a general manner the pressure increased on nearing the American coast.

The currents varied from south near the African coast by south-west to west near the American coast, and their velocity was on an average above half a knot.

Some general remarks on the sounding apparatus (Brook's detaching apparatus) and water-collecting cylinder are next made, attention being more particularly directed to the incorrect conclusions that the latter is apt to lead to. "The constitution of the water as it exists at great depths is not correctly represented by the sample thus obtained. A considerable portion of the gases dissolved therein may escape under the relief of pressure as the cylinder is drawn to the surface, and hence examinations of such samples as regards their gaseous ingredients are liable to be deceptive. Even the saline ingredients will suffer disturbance when they are held in solution by gases that will thus escape; for instance, this is the case with carbonate of lime." Table iv. shows the specific gravities of the samples of sea water from the surface and at various depths to 420 fathoms; Table v., the air temperature between Sierra Leone and the Florida capes; Table vi., the temperature of the air, sea surface, and of the water at various depths. The thermometer was of Six's form, without index error when compared with a standard Kew instrument, but not protected on the Miller-Casella plan.

A diagram of the bed of the Atlantic Ocean at the twelfth parallel of latitude is introduced, based on fifteen soundings. It shows that "parting from the African coast the bed of the ocean sinks very rapidly. A couple of degrees west of the longitude of Cape Verde the soundings are 2,900 fathoms. From this point the mean depth across the ocean may be estimated at about 2,400 fathoms, but from this there are two striking departures—first a depression, the depth of which is 3,100 fathoms, and second, an elevation at which the soundings are only 1,900,—the general result of this being a wide and deep trough on the African side, and a narrower and shallower trough on the American. It may be that this peculiarity is a result of the river distribution on the two continents respectively, there being, with the exception of the Senegal and Gambia, no important streams on the African side, whilst on the American there are many, and among them pre-eminently the Orinoco and the Amazon, these vast rivers carrying their detritus far out to sea and helping to produce the configuration of the ocean bottom in question. However this may be, it is doubtless through these deep troughs that much of the cold water of the north polar current finds its way."

"In accordance with this we perceive, on examining the temperature of the water after the African verge of the greater or eastern sea trough is reached, that there is a difference in temperature between the surface and that at a depth of not more than 200 fathoms exceeding 25° in many cases. This decline of temperature increases as the depth increases, one observation giving an additional fall of 4° at an additional depth of 200 fathoms. It is not, however, intended to affirm that the mass of cold water is restricted to these deep troughs, since even in the West India seas at similar depths low temperatures are observed, and this though the heat of the surface water had become very much higher. In those seas while the surface temperature was 84° the thermometer at depths of 400 and 500 fathoms marked 48°; and these it must be remembered were the indications of an uncompensated instrument which was bearing a pressure of at least half a ton on each square inch of its surface, and hence registering degrees that were higher than the truth. This accords with the observation of Mr. Barrett that in the deepest parts of the sea near Jamaica there exists a temperature not far above that of the freezing point of fresh water." Accompanying these remarks is a diagram showing the curves representing the temperature of the air, surface of the water, and deep water during the voyage, and that is followed by a diagram of the specific gravity of surface and deep water.

"The general conclusion which may be drawn from these results as to temperature and specific gravities is that there exists all over the bottom of the tropical Atlantic and Caribbean Sea a stratum of cold water—cold since its temperature is below 50°. This is the conclusion to which Dr. Carpenter has come as respects the Atlantic in higher north latitudes; and in this important particular the cruise of the *Mercury* must be considered as offering confirmatory proof of the correctness of the deductions drawn from the cruises of the *Lightning* and *Porcupine*."

"There are reasons for supposing that, so far from this water being stagnant, its whole mass has a motion towards the Equator, whilst the surface waters in their turn have a general movement in the opposite direction."

An analysis of the gaseous ingredients was not attempted, because the specimens had been kept too long and for other reasons that are specified; but in relation to organic matter it is stated: "I made some examinations of the organic matter contained in these waters both by incinerating the solid residue and by the permanganate test. . . . . . It needed no especial proof that organic matter was present in every one of these samples, for the clearest of them contained shreddy and flocculent material, some of them quantities of sea-weed in various stages of decomposition. With these vegetable substances were the remains of minute marine animals. As bearing on this subject I found on incinerating the solid residue of a sample of water taken from 200 fathoms, that the organic and volatile material was not less than 11 per cent. of the whole. Though the quantity of organic substance diminished as the structure under examination was deeper, there still remained a visible amount in the water of 400 or 500 fathoms. It is probable therefore that even at the bottom of the ocean such organic substance may exist, not only in solution affording nutriment to animals inhabiting those dark abysses as Prof. Wyville Thomson has suggested, but also in the solid state. Plants of course cannot grow there on account of the absence of light."

"In order to determine whether any hitherto unknown element existed in these waters, I subjected the solid residue to examination with the spectroscope, volatilising the substances by the aid of a voltaic current and induction coil. A careful examination did not reveal the presence of any spectral lines other than those belonging to the well-known elementary substances in sea-water."

"The specimens of the bottom, obtained by attaching to the sounding line quills or wooden tubes, I have transmitted to Dr. Carpenter, who has kindly consented to examine them. In a letter recently received he says, 'As far as I can see they consist of the ordinary Atlantic mud, chalk in process of formation, with the ordinary types of deep-sea foraminifera.'"

THE RECENT AURORA, AND A NEW FORM OF DECLINOMETER

ON Sunday night, the 4th of February, we saw here the magnificent coloured Aurora Borealis, which has been described in NATURE, in the newspapers, and which, I see from telegrams, has been observed at very distant stations. Indications of the aurora were noticed here soon after sunset; but about 6.45 P.M. the whole eastern portion of the sky became illuminated with red light, at first faint, but rapidly becoming more and more intense, while yellow streamers began to shoot up from the north-eastern arc of the horizon nearly to the zenith. About the south-west there was also much red and yellow light; it was spread over a large apparent area, but was not so intensely bright or so strongly coloured as that which lay to the north-east. It too, however, possessed splendid broad, yellow streamers. The display lasted in full beauty till about 7.20, but long after that time much red and yellow light with occasional streamers was to be seen.

It is strange that the phenomena of the Aurora Borealis still remain so little understood. It would add much to our knowledge, if those who witness these displays would make sketches of the appearances at the time when very definite forms of the streamers are observed, noting also the time of the observation very carefully, and the position of well-known stars and constellations. A comparison of such sketches, and of notes that might accompany them, would give us most important data, and might lead to the determination of the locality of the discharge

Simultaneous observations, at widely different stations, of the disturbances of terrestrial magnetism that always accompanied the aurora might, if compared, give us useful information as to the direction and velocity of the electric discharge; and would probably at least help us to decide whether it is to the discharges themselves, or to earth-currents, or to both combined, that these disturbances are due.

I wish to describe an instrument planned by Sir William Thomson, which may be easily constructed, and with which the variations of the horizontal component of terrestrial magnetism can be determined with great accuracy.

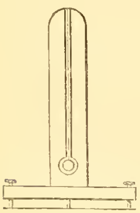


FIG. 1

A flat wooden support, seven or eight inches high, is fixed on a convenient foot furnished with levelling screws, and in the face of it a groove, rather more than four inches long and about $\frac{1}{16}$ of an inch deep, is cut. From a point at the top of this groove, a very light mirror with magnets attached—such as is used in Thomson's reflecting galvanometer—is suspended by a single silk fibre about four inches long; and in front of the groove there is fastened, if the mirror be concave, a slip of plate glass to keep off currents of air: or, if it be a plane mirror, a lens is fastened in front of it, and the remainder of the groove is covered up with a slip of glass or in some other way. A lamp is placed in front of the mirror, and the reflected image of it is received on a scale. The motions of the reflected light upon the scale indicate the deflections of the magnet.

Suitable mirrors and lenses are constructed by Mr. White, instrument maker, Glasgow. In making the mirrors, a large number of the lightest circular glasses used for covering objects on slides for the microscope are silvered; and from these those which give an image perfectly free from distortion are selected by trial. Many of the mirrors formed are much twisted and quite unfit for use; but mirrors are obtained by this plan of selection by trial far superior in lightness and in freedom from distortion to any that can be made by expending extreme care in the glass-work. To the back of each mirror four small magnets are attached; an arrangement which has been found by trial to give the best result. The object is to make the mirror with its magnets sufficiently light, and to give it at the same time the greatest possible magnetic moment. The mirror is three-eighths of an inch in diameter, and weighs not more than one-third of a grain.

Plane mirrors are generally used in Glasgow, and the lens is of such power that a lamp placed at a distance of one metre (about 40 inches) gives an image at the same distance from the mirror. The lamp is placed behind a screen, and in the screen an oval hole is cut and a vertical wire* is stretched across it. The image of this wire is received upon a scale. The scale may be set at a distance of 40 inches (one metre) from the mirror; that is to say it

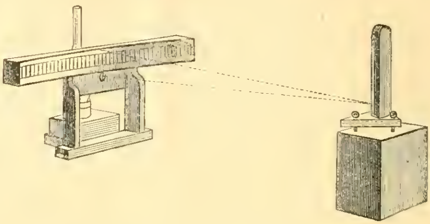


FIG. 2

may be attached to the screen between the mirror and the lamp; or it may be put much farther away, at, say two or three times that distance. The lamp and screen, with its slit and wire, must then be brought near enough to the mirror to throw back the conjugate focus sufficiently. This arrangement gives of course increased sensibility. We use for it a paraffin oil lamp, of which the reservoir is a very shallow rectangular vessel. The slit in the screen is slightly above the horizontal plane through the centre of the mirror, and the scale slightly below that plane. The reflected ray passes below the reservoir of the lamp to the scale beyond.

Our scales, which are also obtainable from Mr. White, are divided into fortieths of an inch, and are generally attached to a piece of wood, cut out so that its curvature corresponds to that of a circle described with the distance of the mirror as radius. Thus, by dividing the number of scale divisions by the distance of the mirror in fortieths of an inch from the scale, the angle is obtained to which that number of scale divisions correspond. At a distance of 60 inches we can easily read the position of the image of the wire on the scale to less than half a scale division, which, since the angle turned through by the reflected beam of light is twice that turned through by the mirror, corresponds to an angular deflection of about 20″.

The great advantage which the arrangement that I have just described possesses over any that are ordinarily used for observing rapid variation in magnetic declination lies in the lightness of the mass moved. The heavy declinometers employed in observatories are unable, through their great inertia, to follow accurately the sudden variations that occur during a magnetic storm.

JAMES THOMSON BOTTOMLEY

The College, Glasgow

* A simple vertical slit was formerly used, but the vertical wire in the middle of the slit, a suggestion of Prof. Tait, is a great improvement, as it enables us to use plenty of light, while it gives increased precision to the reading on the scale.

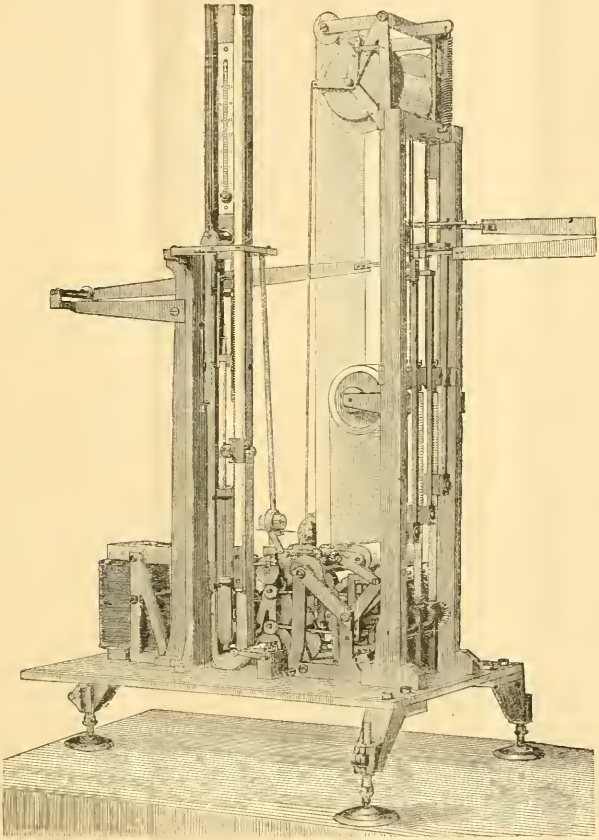
THEORELL'S PRINTING METEOROGRAPH

SOME time since brief mention was made of the above instrument (devised by Dr. A. G. Theorell, of Nybrogatan, Stockholm, Sweden—NATURE, vol. iv. p. 466)—with reference to its having been exhibited at the London International Exhibition, 1871.

Being in possession of a detailed description of the same, I have thought that a copy with additional remarks may be of interest in the pages of NATURE.

I have no hesitation in saying that this instrument does not occupy the prominent position to which it is justly entitled, neither do I think that it is so well known—whether taken in the light of a wonderful piece of mechanism, or of excellent workmanship—as it well deserves to be.

The following is a description of the instrument in accordance with the original, excepting only that I have made a few slight alterations in order to render it more intelligible, the original having been, as I suppose, translated from the Swedish language, and not well expressed :—



"Meteorological observations are by this instrument delivered in tables printed on a slip of paper. Of the four tabular columns the *first* gives the hours, the *second* the temperature, the *third* the degree of humidity according to August's method, and the *fourth* the atmospheric pressure; this last (atmospheric pressure) is given in millimetres, but the first figure, being always a 7, is suppressed. The degrees of the thermometer employed are those of the centigrade scale, and negative degrees are expressed by their complements to 100.

"The registration takes place by means of electrical currents, which are closed by contact between the mercury in the various meteorological instruments and steel wires that descend into their tubes. These steel wires are connected, by means of levers and three vertical screws, each with its respective system of brass wheels with numerical type engraved on the edges, in such a manner that the rotation of the wheels causes an upward or downward motion of the steel wires, so that the point of the scale on which the lower extremity of the wire is situated, is necessarily that indicated by the number appearing at the same moment uppermost on the corresponding wheels.

"The wheels containing the figures are governed by an electro-magnetic motor, which, for each observation, sets the three wheel systems successively in motion, until the corresponding wires have reached the mercury in their respective meteorological instruments, when the above-mentioned electrical current instantly arrests the motion, so that accordingly all three steel wires stop with their lower extremities each in contact with the surface of the mercury in its respective instrument. The numbers therefore that stand uppermost on the numbered wheels are just those which indicate the height of the barometer and of the two thermometers, and now the same electro-magnetic motor operates upon a printing apparatus which, after having deposited colour on the type, presses the slip of paper against them. This being done, the steel wires are drawn up again by the motor, which stops as soon as a certain distance from the mercury is attained, and all is ready for the next observation.

"The interval between the observations is a quarter of an hour."

Attention is then directed to the following considerations:—

"The instrument delivers the observations in a form in which they may immediately, and without further modification, be used by the meteorologist in his work.

"A very large number of very carefully made comparisons have shown that the observations registered by this method possess an accuracy equal to that which is generally attained by ocular observation.

"The zinc vessel, in which the upper ends of the thermometers are enclosed, is so air-tight that it is found possible, by means of chloride of lime and caustic potash, to keep the enclosed air always free from damp and carbonic acid, a precaution which it will be easily understood is necessary in every climate where the temperature is liable to sink below the freezing-point, but is still further necessary to protect both the mercury and the steel wires from oxidation, and thus preserve the galvanic contact.

"A meteorograph of this construction has for two years and three-quarters been in use at the Upsala Observatory, executing six observations every hour, without any perceptible alteration of the surface either of the mercury or the steel wires, that could in any way affect either the free efficiency of the instrument or its degree of accuracy, which throughout the whole time has been found to be that above named.

"As the clock which determines the time of the observations does not require winding up—the instrument itself restoring the tension of the mainspring every quarter of an hour—it continues to go as long as the driving force, *i.e.*, the electrical current, is maintained; and, as the slip of paper applied lasts fully three months, it is clear that that is the period for which the instrument may be left to itself. The work then requisite is little more than to take out, cut, and sew up in order the paper of observations, and replace it with another slip. We thus see that this instrument requires but very little time and labour of the person who takes charge of it.

"It is entirely for special reasons that the construction of the instrument has been limited to the registration of thermometrical, psychrometrical (hygrometrical), and barometrical observations, for the method may be applied advantageously to observations of the course of any phenomena whatever, provided they can be indicated by an index admitting of galvanic contact. It is, therefore, applicable for all the now usual kinds of meteorological observations, and nothing prevents the same instrument executing and printing them all in one and the same table."

The following is an extract (giving one hour's instrumental readings) of one of the printed forms referred to in the first paragraph:—

1	57	130	673
	57	1305	6725
	57	132	672
	57	133	673
2	57	1335	672

The width of the slip of paper used in these observations is 4·25 in.

In the Exhibition meteorograph, the timekeeper (referred to in the eighth paragraph) is merely a watch-movement of moderate size.* In the place of the ordinary minute-hand there are four, fitted on the same centre and projecting from each other at right angles in the form of a cross; in other words, the points (one of which resembles what is technically termed a *spade* hour hand, and indicates the time) are 15 min. apart. Every time one or other of the hands comes opposite the figure III. it depresses a small steel lever which, through suitable mechanism, completes the circuit.

I am indebted to Dr. Theorell for a very courteous letter, dated from Upsala, respecting the block used in the original description, also to Messrs. Norstedt and Son, printers to the Swedish Government, for supplying me with an electrotype copy of the same through the Swedish Consulate.

JOHN JAMES HALL

ON SLEEP†

PROFESSOR HUMPHRY commenced his lecture by giving a brief account of some of the changes that take place in the tissues when their function is active, and explained that during this time a slight deterioration of structure takes place, which, affecting the voluntary system, the muscles and hemispheres of the brain, causes the sense of tiring, and necessitates a period of rest for the restoration of the tissues to their former condition. In the case of the muscles this rest is provided for by periods, quickly alternating periods, of action and cessation of action. But in the case of the brain, the actions upon which consciousness, volition, &c., depend cannot be thus frequently suspended. Their continuance is needed for the safety of the body during long periods, through the whole day, for instance; and longer periods are therefore required for repair. These are the periods of sleep.

He next took a cursory glance at the different parts of the nervous system, explaining that the upper regions of the brain are those which minister to consciousness and volition, the intellectual operations, &c. He showed that the functions of these regions not only can long be suspended without interfering with the action of the lower parts of the brain, which are more immediately necessary to life; but that they are very easily suspended—slight causes, such as a jar, or a shock, or an alteration in the blood current, being sufficient to stop the action of these parts and deprive the person of consciousness. The spontaneous stopping of their action, consequent on the slight deterioration of their structure from the continuance of their functions during the day, is the proximate cause of sleep during the night; and the periodic recurrence of sleep is in accordance with the periodicity observed in several of the nutritive functions, and, indeed, witnessed in many of the other operations of nature.

After observations upon the condition of the brain during sleep, the circumstances that conduce to sleep, the time that should be allotted to it, and other points, the Professor entered at some length into the subject of dreams. These he regarded not, as has been supposed by some, to be a necessary attendant on, or feature of, sleep, but rather to be the result of an abnormal condition. In the natural state we should pass from wakefulness to complete uncon-

* On the other side of the instrument to that seen in the engraving.

† Abstract of a Lecture delivered at the Royal Institution, on Friday, February 9, by Prof. Humphry.

sciousness, and *vice versâ*, quickly, almost instantaneously, and many persons habitually do so. But the transition period is sometimes prolonged, and stages are observable. The first thing that occurs is the lowering, or cessation, of that control over the mental processes which is the highest of our powers, the one requiring the greatest effort, and the one most easily lost. In this condition the thoughts ramble unchecked, chase one another confusedly over the mental field, and give rise to all sorts of incongruities of the imagination. At the same time, being unrestrained, they are excited, and evince efforts of memory and even of combination, of which, in the regulated state of wakefulness, they are quite incapable. In this way the images of persons and places, events, and items of knowledge, long forgotten in the ordinary state, are recalled with distinctness, and we fancy that new information has been acquired when it is only forgotten facts that are recalled. He did not agree with the physiologists who conceive that dreaming depends upon an inequality in the condition of different parts of the brain, some being excited or wakeful, while others are quiescent or asleep. He rather took the view that all the parts of the cerebral hemispheres combine in each of the efforts of control, consciousness, memory, and other mental acts, that all suffer alike from those efforts, alike need the restoring changes which take place in sleep, and, together, *pari passu*, pass through the stages on the way to and from sleep, in which dreaming, sleep-walking, &c., occur.

NOTICE OF THE ADDRESS OF PROF. T. STERRY HUNT BEFORE THE AMERICAN ASSOCIATION AT INDIANAPOLIS *

IN a brief notice of the recent address of Prof. Hunt, it is stated that, while the discussions show learning and research, and his review of the progress of opinions with regard to the Taconic and associated rocks is an able presentation of the subject, its conclusions are throughout open to doubts and objections. Since it is fairer to an author to make special, rather than general, criticisms, I propose to state here a part of the objections referred to in that remark. They are as follows :—

1. That, while accepting the ordinary views with regard to most "pseudomorphs by alteration" (crystals chemically altered without a loss of form), he rejects them with respect to those that are silicates in composition ; that is, he denies that the crystals of serpentine having the form of chrysolite, pyroxene, dolomite, &c., are pseudomorphs ; and the same of those of steatite, having the form of hornblende, pyroxene, spinel, &c. ; of those of pinite having the form of nephelite, scapolite, &c. ; and so in other cases :—notwithstanding that (1) they bear positive evidence of change in having ordinarily no polarising properties, and no other interior features or qualities conforming to the external form ; that (2) the crystalline forms are just those presented by the species after which they are supposed to be pseudomorphs, and the idea of their being real forms of a single polymorphous species is wholly inadmissible, as pronounced by every crystallographer who has written on the subject ; that (3) the pseudomorphs show all stages in the process of change from incipient to complete alteration, in the latter case not a trace of the original mineral remaining.

In this assumption, for it is little better, he opposes the views of every writer on pseudomorphs, excepting one—Scheerer ; and Scheerer's chemical speculations, which are at the basis of his opinions, he rejects, like all other chemists.

This unwarranted assumption has a profound position in the system of views on metamorphism which Prof. Hunt holds, and gives shape and intensity to his opinions of the views of others.

2. That, in commencing a paragraph with the sentence, "The doctrine of pseudomorphism by alteration, as taught by Gustaf Rose, Haidinger, Blum, Volger, Rammelsberg, Dana, Bischof, and many others (meaning thereby other writers on pseudomorphism), leads them, however, to admit still greater and more remarkable changes than these, and to maintain the possibility of converting almost any silicate into any other"—he grossly misrepresents the views of at least Rose, Haidinger, Blum, Rammelsberg, Dana ; and that he completes the caricature in the closing sentence of the same paragraph, in which he says, "In this way we are led from gneiss or granite to limestone, from limestone to dolomite, and from dolomite to serpentine, or more directly from granite, granulite or diorite to serpentine at once, without passing through the intermediate stages of limestone and dolomite ;" part of which transformations, I, for one, had never conceived ; and Rose, Haidinger, Rammelsberg, and probably Blum and the "many others," would repudiate them as strongly as myself. Next follows a verse from Goethe, that is made to announce his personal vexation with their "sophistries ;" *alias* absurdities, as the context implies.

Prof. Hunt's rejection of established truth alluded to under sec. 1 here manifests its effects in leading him to misrepresent—although unintentionally—the views of writers on pseudomorphism ; and to add to his misrepresentation by means of the strange conclusion, that, because such writers hold that crystals may undergo certain alterations in composition, therefore they believe that rocks of the same constitution may undergo the same changes ; as if it were not possible that external or epigenic agencies might reach and alter crystals under some circumstances of position, when they could not gain access to great beds of rock. Haidinger, the eminent crystallographer, mineralogist, and physicist of Vienna, and one of the most prominent writers on pseudomorphism, never wrote upon the subject of the alteration of rocks at all, and this is true of others, against whom the above charge is made by Mr. Hunt.

With a little clearer judgment, part at least of that vexation of spirit which required the help of a great German poet, and the German language, adequately to express, might have been avoided.

3. That he charges me with the opinion of Bischof, that "regional metamorphism is pseudomorphism on a grand scale :" when I make no such remark, neither express the sentiment, in my Mineralogy of 1854, in which I give an abstract of Bischof's views and make my nearest approach to them ; and when, if there was any occasion for a notice of my opinions, a critic of 1871 should have referred to the formal expression of them in my "Manual of Geology," first published in 1863. The reader will there find the "diagenesis" of Gümbel, which Mr. Hunt takes occasion to commend, applied, as had been done by others, although Gümbel had not then announced it ; and also other points discussed, with but a brief allusion to pseudomorphism.

The above remark by Mr. Hunt is not made with special reference in his address to magnesian silicates, or any other particular class of siliceous minerals ; but, as the context shows, to rocks in general. I have held to views respecting the origin of serpentine which Prof. Hunt rejects, and have sustained them on the ground that the pseudomorphous crystals of serpentine show what transformations are chemically possible, and that hence they may possibly illustrate the changes which beds of rock have undergone. I have not applied this principle in accounting for the origin of ordinary metamorphic rocks, because, as above observed, crystals may often be reached by agencies which can never reach or affect rock-formations, and for various other reasons against it. But the case of serpentine has been regarded as somewhat

* Prof. Hunt's address has been published in the "American Naturalist" for September, 1871, and, since then, in part, in NATURE, Vol. v. Nos. 105, 106, 107. Prof. Dana's reply is reprinted from advance-sheets of *Silliman's Journal* forwarded to us by the author.

different; and I have believed, and still believe, that extended beds of rock have been turned into this mineral by a method analogous to that which takes place in pseudomorphism. Had Mr. Hunt's statement been made a special one, restricted to this case, I should have had little objection to it. I may add that the method of origin for serpentine which I have deemed most probable (though perhaps not the only method) is one which he once advocated—that of the alteration of beds of dolomite, or magnesian carbonate of lime, by waters containing alkaline silicates in solution; and it has appeared to me that the facts (1) that serpentine is commonly associated with beds of limestone or dolomite, (2) that chrysolite crystals are sometimes found in these rocks, and (3) that the forms of crystals of both dolomite and chrysolite occur among serpentine pseudomorphs, give strong support to this view.

Prof. Hunt's opinion on this point in 1857 he thus expressed in a letter to the writer, sent for insertion in "Silliman's Journal," where it appears in volume xxiii. (1857) at p. 437, as a conclusion to his brief statement.

"Suppose a solution of alkaline silicate, which will never be wanting among sediments where feldspar exists, to be diffused through a mixture of siliceous matter and earthy carbonate, and we have, with a temperature of 212° F., and perhaps less, all the conditions necessary for the conversion of the sedimentary mass into pyroxenite, diallage, serpentine, talc, rhodonite, all of which constitute beds in our metamorphic strata. Add to the above the presence of aluminous matter, and you have the elements of chlorite, garnet, and epidote. We have here an explanation of the metamorphism of the Silurian strata of the Green Mountain range, and I believe of rock metamorphism in general." Again, in a letter dated July 6th, published in volume xxiv., at page 272, he says:

"I have already in a previous note indicated the manner in which I suppose these siliceous and argillaceous magnesites and dolomites to have been in certain parts of the formation transformed by the intervention of solutions of alkaline carbonates into silicates, such as talc, serpentine, chlorite, pyroxenite, &c. A further development of my views of the metamorphism of sediments, with the results of the investigation of a great many altered rocks, will appear in the Report of Progress of the Geological Survey of Canada for the last three years—now in press."

It should be added, that Prof. Hunt acknowledges his change of opinion in his address. But, in view of it, some moderating of his positiveness of assertion would have been reasonable.

4. That he attributes the origin of *beds* of serpentine and steatite,—here following nearly Delesse,—to the alteration of beds of different hydrous magnesian silicates related to sepiolite (meerschaum), formed in the surface waters of an era—Palæozoic or earlier—while fossiliferous rocks were in progress:—when, as a matter of fact, no such sepiolite-like beds are known to occur anywhere in *unaltered* stratified formations of Palæozoic or pre-Silurian time, and they are found of limited extent only in some strata of comparatively recent origin. The hypothesis, although deserving of consideration, is therefore without any solid foundation. The doubts that have been recently thrown about the Eozoön affect unfavourably the hypothesis, since these supposed fossils have been made prominent in its support. The view, if true, would, as Prof. Hunt implies, bring the making of serpentine and steatite rocks under the kind of metamorphism styled by Gümbel diagenesis, instead of that of epigenesis; making them a result of change without an addition of ingredients from any external source, like most other metamorphism, instead of through the agency of outside ingredients. But it wants facts to rest upon.

5. That he attributes an origin similar to that for serpentine and talc to beds of chlorite and hornblende; notwithstanding the fact that chlorite schist and hornblende schist—the purest forms of any large beds of these minerals—are always more or less impure, and often graduate into clay slate on one side, and mica schist on the other; and that these schists are thus so involved with others, that if one is derived from ordinary sedimentary beds, all must be.

6. That he devotes some pages to a "theory of envelopment" as a method of accounting for the silicate pseudomorphs referred to, beginning a paragraph with the sentence:—

"By far the greater number of cases on which this general theory of pseudomorphism by a slow process of alteration in minerals has been based, are, as I shall endeavour to show, examples of the phenomenon of mineral envelopment, so well studied by Delesse in his essay on Pseudomorphs."

While, in fact, this theory has almost nothing to do with the subject, since pseudomorphs of serpentine, steatite, and other species, with regard to which there is the dispute, consist often of *pure* serpentine, steatite, &c., and therefore have no enveloper, and are not cases of envelopment. This theory supposes the material of the so-called pseudomorph to be an impurity taken up into a crystal in process of formation—a thing of common occurrence; and, if satisfactory, would account for the want of conformity between internal qualities and external form. It is unfortunate for it that, as just shown, it does not apply where it is wanted.

7. That he makes Delesse the author of the "theory of envelopment:"—when Delesse has not proposed any such theory for cases of ordinary pseudomorphism, but has simply commenced, and very judiciously, his work on Pseudomorphs (1859) by distinguishing the examples of mere impurity, or envelopment, in crystallisation, in order to clear the way for the actual facts; and then gives a long list of admitted pseudomorphs, including in it nearly all kinds so recognised by other authors, and all that affect the question discussed by Prof. Hunt; serpentine occurring in the list as forming pseudomorphs after chrysolite, hornblende, garnet; steatite after pyroxene, hornblende, epidote, scapolite, mica, topaz, magnesite, dolomite, &c. In his work on metamorphism (1861), Delesse takes back none of his views on pseudomorphism; and in his late "Reviews of the Progress of Geology," down to the last just out (1871), he reiterates the ordinary views with regard to pseudomorphism, and mentions the occurrence of other pseudomorphs consisting of talc, serpentine, &c.

8. That he cites Naumann as sustaining the "theory of envelopment:"—when this learned crystallographer and mineralogist has only commended Delesse's chapter on the envelopment of minerals in crystals, and presents in his "Mineralogy" (the last edition of which, that of 1871, is now before me) the subject of pseudomorphism in the usual way, with nothing whatever on the theory of envelopment; and, under the description of the species serpentine, he speaks of "large pseudomorphous crystals of serpentine from Snarum which still contain a nucleus of altered chrysolite."

There is hence no foundation for Mr. Hunt's statement that his views are "ably supported by Delesse," or any occasion for the "no small pleasure" he derived from Naumann's letter; or any warrant for the remark (p. 47) that Delesse and Naumann hold the "view" "that the so-called cases of pseudomorphism, on which the theory of metamorphism by alteration has been built, are, for the most part, examples of association and envelopment, and the result of a contemporaneous and original crystallisation." These men of science are not to be counted upon for aid, countenance, or comfort; though claimed as friends, it has not been their fault, as they have always avowed the opinions of Haidinger and the "many others." It is a strange fact that, neither these claimed friends, nor the many announced opponents, with one or two exceptions, hold the views which Prof. Hunt has attributed to them in his address. We are glad to know that this is not the usual American method of dealing with authorities.

Gümbel and Credner are the other two claimed supporters of his views. They have sustained Mr. Hunt's opinions as regards the Eozöon and the origin of the serpentine constituting it. But whether they disagree with Haidinger and all others as to pseudomorphs of serpentine, and of other hydrous silicates, I cannot say.

9. That while setting down the Taconic rocks, and rightly, as Lower Silurian in age, he denominates the micaceous gneisses, diorites, epidotic and chloritic, steatitic and serpentinous rocks, talcoid mica schists, quartzites, and clay-slates (which are always without staurolite or andalusite), in fact, the whole range of metamorphic rocks, with small exceptions, between the Connecticut river and the great limestone formation of the Green Mountains (admitted to be Lower Silurian), as the *Green Mountain Series*, and makes the whole "*pre-Cambrian*" in age, although the region has not been examined by any one stratigraphically with the care necessary for a positive opinion; and, although there are gneisses, mica schists, and chloritic talcoid (or mica) schists in the Taconic series, and therefore of admitted Lower Silurian origin, which are closely like those of his Green Mountain Series.

10. That he denominates, in like manner, the gneisses, mica schists (said to be richer in mica than those of the Green Mountain Series), hornblendic gneisses and schists, micaceous and clay-slates containing andalusite, cyanite, or staurolite, and certain limestones, existing east of the Connecticut river, as a *White Mountain Series*, and makes these a newer "pre-Cambrian" than the Green Mountain Series:—when there is the same want of stratigraphical evidence as to age as in the former; and when Prof. C. H. Hitchcock's discoveries of Helderberg corals (Lower Devonian, according to Billings, or else upper beds of the Upper Silurian), at Littleton, not far north of the western extremity of the White Mountain Series, makes it more probable that part of the White Mountain Series of beds are of Helderberg age rather than pre-Silurian; and his discovery of labradorite rocks on the south-western margin of the White Mountains, wholly unlike any of the so-called White Mountain Series, shows further that a vast amount of study in the field is needed before the dictum of any one respecting the age of New Hampshire rocks is worth much.

It is now proved that there are labradorite rocks in Waterville and Albany, N.H., on the borders of the White Mountain region, which are *probably* of *Laurentian* age; that on the other side of the White Mountain line, but 25 miles to the north-northwest, there are fossil-bearing, metamorphic rocks of the *Helderberg* (upper or lower) period; that 100 miles south-southwest, in Bernardston, Mass., or central New England, there are other fossil-bearing metamorphic *Helderberg* rocks, some of the well-preserved crinoidal stems (as the writer has seen, as well as read of in the account of Prof. Hitchcock) *an inch in diameter*. Who then knows whether all, or any, of the long intermediate periods of geological time, from the Laurentian to the Devonian, are represented in the New Hampshire metamorphic rocks lying between these limits? When observation has given positive knowledge, we may then have several "White Mountain Series."

11. That he has relied for his chronological arrangement of the crystalline rocks of New England and elsewhere, largely on lithological evidence, and commends this style of evidence, when such evidence means nothing until tested by thorough stratigraphical investigation. This evidence means something, or probably so, with respect to Laurentian rocks; but it did not until the age of the rocks, in their relations to others, was first stratigraphically ascertained. It may turn out to be worth something as regards later rocks when the facts have been carefully tested by stratigraphy. A fossil is proved, by careful observation, to be restricted to the rocks of a certain period, before it is used—and then cautiously—for identifying equivalent beds. Has anyone proved by careful observation that crystals of staurolite, cyanite, or andalusite, are restricted to rocks of a certain geological period? Assumptions and opinions, however strongly emphasised, are not proofs.

It is no objection to stratigraphical evidence that it is difficult to obtain; is very doubtful on account of the difficulties; may take scores of years in New England to reach any safe conclusions. It must be obtained, whatever labour and care it costs, before the real order and relations of the rocks can be known. Until then, lithology may give us guesses, but nothing more substantial.

Mr. Hunt's arguments with reference to the White Mountain Series, as urged by him in 1870, will be found in *Silliman's Journal*, ii. l. 83. Both there, and in his address, may be seen the kind of evidence with which he fortifies, or supplements, that based on the character of the rocks. Direct stratigraphical investigation over the region itself, in which all flexures, faults, and unconformabilities have been thoroughly investigated, is not among the foundations of opinion which he brings forward.

He endeavours to set aside the objections to his views suggested by the existence of Devonian or Helderberg rocks in central and northern New England; but he presents, for this purpose, only some general considerations of little weight, instead of definite facts as to the extent and variety of the metamorphic strata that are part of, because comformable to, these Helderberg beds. Had he studied up these stratigraphical relations with the care requisite to obtain the truth, and all the truth, perhaps he would no longer say—it is "contrary to my notions of the geological history of the continent to suppose that rocks of Devonian age could in that region have assumed such lithological characters." Notions often lead astray.

JAMES D. DANA

NOTES

THE Royal Horticultural Society has taken a step which may prove very advantageous to the interests of science, namely, the appointment of a botanical Professor, who, by lectures, answers to personal inquiries, and other means, shall assist in establishing a more correct knowledge of the principles of botany and horticulture, and of the names of plants, among those of the Fellows and their gardeners who are desirous to profit by the opportunity. Among the duties of the Professor of Botany will be to conduct the scientific business of the society, both horticultural and botanical; to enter into communication with horticultural and botanical establishments at home and abroad; to conduct the meetings and edit the publications of the society; to give courses of lectures on scientific botany to the gardeners and others; and to have a general superintendence of the gardens at Chiswick. The appointment to this office of Mr. W. T. Thiselton-Dyer, late Professor of Botany at the Royal College of Science, Dublin, is a guarantee that the cultivation of scientific botany will not be neglected.

DR. DAVID FERRIER has been appointed Professor of Forensic Medicine at King's College, London, *vice* W. A. Guy, M.B., resigned.

THE Secretary of State for India has appointed Mr. A. G. Greenhill, Fellow of St. John's College, Cambridge, Professor of Applied Mathematics at the Civil Engineering College, Cooper's Hill. Mr. Greenhill graduated as Second Wrangler in 1870, and was bracketed equal with the Senior Wrangler for the Smith's Prize; he also gained a Whitworth Scholarship while an undergraduate.

THE Radcliffe trustees at Oxford, anxious to aid one or more advanced students in the scientific study of preventative or curative medicine, offer 10*l.* a month, for three months, to a student of St. Bartholomew's, Guy's, or St. George's Hospitals, desirous of working for that time in Oxford. He will have opportunities of studying physics, chemistry, geology, the higher parts of biology, clinical and sanitary medicine. Candidates must be recommended on intimate personal knowledge by the Dean or secretary of their medical schools, and will not be submitted to an examination. The first election will be in the last week of February. There will be an election of another student in April.

A GENTLEMAN named Millard has bequeathed to the President and Fellows of Trinity College, Oxford, 8,000*l.* for the advancement of mathematical and general science.

THE University of St. Andrew's has conferred the degree of LL.D. on Mr. Archibald Cunningham Geikie, Professor of Mineralogy and Geology in Edinburgh University.

THE Royal Irish Academy have granted from the fund at their disposal for scientific research, the following:—50*l.* to C. R. C. Tichborne, for Researches on the Dissociation of Salts in hot solutions, and on the History of the Terebenes; 30*l.* to E. T. Hardman, for Chemico-Geological Researches; 25*l.* to Prof. R. S. Ball, for Researches in the Motion of Vortex Rings; 25*l.* to Prof. S. Downing, for Researches on the Motion of Water through Curved Tubes; 50*l.* to P. S. Abraham, for Biological Researches on the Coast of Madeira.

ROBERT PATTERSON, F.R.S., died at his residence, Belfast, on the 14th. He was born in April 1802. Educated at the Belfast Academy, in his early days he contemplated the Irish Bar as a profession, but finally devoted himself to mercantile pursuits. At a very early age he was an ardent student of Natural History, and in 1821 he joined with a few others to form the Natural History Society of Belfast. Among the first papers read before this Society were a series by Mr. Patterson on the insects mentioned by Shakespeare, which were afterwards published. His most important contribution to biological literature was, perhaps, his "Zoology for Schools," the first part of which appeared in 1846. This little work proved a great success. It was adopted by the Commissioners of Irish National Schools, and also by the Committee of Education in England, and most certainly gave a great impulse to the study of Zoology among the school classes of Great Britain. This led to the issue in 1853 of "Zoological Diagrams," large coloured plates which have proved of material assistance to both the teacher and the taught. He was a member of the British Association in its early days, and we believe that the daily printed "Journal of Proceedings" was an idea that originated with him. Of the different positions of honour and trust held by Mr. Patterson in his native town, we need not here speak. He was elected a member of the Royal Irish Academy in 1856, and a Fellow of the Royal Society in 1859. His genial and kindly presence will be missed by very many of his old and young friends.

Harper's Weekly notes the death of Mr. W. Harper Pease, at Honolulu, about the last of July, 1871. This gentleman was an American, born, we believe, in Pennsylvania, and was occupied for a long period in natural history pursuits. During the Mexican war he visited that country, under the protection of the American army, and made extensive collections of birds, which were deposited in the Academy of Natural Sciences at Philadelphia, among them some new species described by Mr. Cassin. About the year 1853 he visited the Sandwich Islands, and occupied himself for a time as a surveyor, and was sufficiently well pleased with the climate and country to remain there, marrying a native, and adapting himself to the customs of the people. During the whole of his residence in Polynesia he was engaged in studying the mollusca of the Sandwich Islands, and gradually extended his research to the species of all the Polynesian group, making collections either directly or through the medium of Mr. Garrett and others. Numerous communications from his pen upon Polynesian conchology have appeared in the *Journal de Conchologie* of Paris, the *Conchological Journal* of Philadelphia, the Proceedings of the Zoological Society of London, and elsewhere, and he has long been recognised as a thorough naturalist and reliable author. He had accumulated around him at Honolulu a very large library of conchological works, which, indeed, lacked few if any of the more important treatises. He enriched the principal cabinets of America and Europe by furnishing extensive collections, by which means he obtained, in part, the facilities for procuring the books needed for his investigations. He was for several years in ill health, and his death by consumption was not at all unexpected by his friends.

WE also learn from *Harper's Weekly* of the death in Reading, Pennsylvania, on December 26, 1871, of Mr. Charles Kessler, in the sixty-sixth year of his age. Mr. Kessler was known as an ardent and successful student of entomology, devoting himself to the lepidoptera, or butterflies, and bringing together a very large collection of insects of this order. We have not heard what disposition is to be made of this collection, but we presume it will ultimately come into the possession of some one of the natural history museums of the country.

PROF. WYVILLE THOMSON has been prevented from lecturing to his students for the past fourteen days, owing to a mild attack of continued fever. He hopes, however, to be able to begin again on Wednesday next. Dr. Christison has also been laid up for some days, owing to an attack of ephemeral fever.

WE learn from the *Academy* that the African traveller and botanist, Dr. Schweinfurth, has happily returned in safety to Europe, and though he has suffered the loss of the greater part of his invaluable collections and drawings, he has brought back a harvest of information and experience which places his journey among the most successful of modern times. After his great journey west of the Upper Nile, in the country of the Niam-Niam and Monbuttu, he made a short excursion from his headquarters, the Seriba Ghatta, westward to Kurkur and Danga, positions formerly visited by Petherick, and returning, planned a much more extended journey, when a fire broke out in the Seriba Ghatta on the 2nd of December, 1870, which not only destroyed the station, but with it the whole property of the traveller. Fortunately, a portion of his collection was at that time already on its way to Berlin. Provided with a few necessaries at Seriba Siber, the headquarters of the Egyptian troops, the indefatigable traveller made a tour in a part of Fertit hitherto unvisited by Europeans from December 1870 to February 1871, during which he found that the Bachr-el-Arab is unquestionably the main stream of the basin which mouths in the Nile at the Bachr-el-Ghazal. Having been deprived by the fire of every instrument by means of which any mechanical reckoning of the distances traversed during this journey could be made, the explorer, with an energy perhaps unexampled, set himself the task of counting each step taken, and in this way constructed a very satisfactory survey of his route.

Harper's Weekly announces the receipt of advices as late as the 5th of November from Mr. William H. Dall, whose return to Alaska under the auspices of the Coast Survey we have already chronicled. Mr. Dall is well known for the encyclopædic work published by him some time ago upon Alaska, the result of several years' residence in that region. His present position gives him unusual advantages for observation and research, and will doubtless be made the most of in gathering an important mass of information. He is now stationed at Iliuliuk, in Unalaska, and engaged in surveying harbours and taking soundings, and generally in gathering such information as to the shores and their adjacent waters, the tides and currents, as will be to the interest of commerce and navigation. He is also using his opportunities in dredging for marine animals, and in making collections of natural history, of which he has already accumulated quite a number.

THE first contribution to science from the *Hasler* expedition, under Prof. Agassiz, appears in the form of a letter addressed to Prof. Peirce, dated St. Thomas, December 15. In this it is stated that, in the course of the frequent examination of the floating Gulf-weeds made daily, for the purpose of collecting the marine animals that usually inhabit them, a mass of this weed was found, the branches and leaves of which were united together by fine threads, wrapping it in every direction into the form of a ball. The threads forming the connecting material were elastic, and beaded at intervals; the beads being sometimes close together, sometimes more remote, a bunch of them occasionally hanging from the same cluster of the threads. From the accounts of the professor it would appear as if a globular mass had been formed by wrapping up a small quantity in the thread, and then adding more, and continually wrapping it up, until a ball of considerable size was produced. A careful examination of these beads showed that they were in reality the eggs contained in the substance of the threads, and in some the embryo was sufficiently far advanced to prove that they belonged to a fish. The mass was preserved and watched until some became detached and were free in the water; and by a very interesting process of critical investigation, the fish itself being too small for identification, it was ascertained, mainly through the structure of the pigment-cells, that they belonged to a small species, quite common in the Gulf Stream, known as *Chironectes pictus*. In this genus the pectoral fins are supported on arm-like appendages, giving them the power of hands; a somewhat similar structure in some allied forms enabling them, when thrown on the shore, to walk or crawl back leisurely into the water. It is somewhat remarkable that these eggs should have been found in the month of December, when the great majority of species lay their eggs in early spring. It is possible that *Chironectes pictus* may be an exception to the general rule. A scarcely less interesting peculiarity is seen in regard to the eggs of the goose-fish, or the common fishing-frog, of the Atlantic coast. This is an extremely hideous-looking species, shaped like a much-depressed tadpole, with an enormous head and huge mouth, and sometimes weighing from fifty to one hundred pounds. It is known to naturalists as *Lophius americanus*. The eggs of this species are contained in an immense flat sheet of mucus, sometimes thirty or forty feet long, and twelve to fifteen wide, which, when floating along the surface of the ocean, resembles nothing so much as a lady's brown veil. The mucus is so tenacious as to admit of being wrapped around an oar and dragged on board a vessel, but is extremely slippery, and readily escapes from one's grasp. The eggs, or embryos, are disseminated throughout this sheet at the rate of ten to twenty to the square inch, and by their brownish colour tend to give the impression just referred to. The number of eggs in one of these sheets is enormous, in some instances exceeding a million.

THE *Gardener's Chronicle* inquires whether the physicians or the lecturers on Botany at St. Thomas's Hospital and King's College Hospital, London, respectively, have been consulted as to the planting that has been lately carried on in the enclosures facing the buildings we have mentioned. We can hardly suppose that these gentlemen can have had any voice in the matter, since they must be too good physiologists not to know what must be the inevitable result of such operations. At St. Thomas's the expenditure for evergreen shrubs must have been very considerable. There are scores of such things as *Libocedrus decurrens*, *Cupressus Lawsoniana*, *Thujopsis borealis*, *Wellingtonia*, and the like, which are certain to die. The selection of evergreen shrubs for the Thames Embankment (north) is sufficiently unfortunate, but for reckless planting commend us to the Hospital of St. Thomas. At King's College Hospital the planting has been more modest, the victims consisting merely of cherry laurels. Surely we might have looked for a little common sense in such establishments as we have alluded to.

AT the Wisbech District Chamber of Agriculture, held on February 1st, some very interesting remarks were made by Mr. S. H. Miller, advocating the establishment of a County Agricultural Laboratory in which chemistry, botany, and agricultural meteorology might be prosecuted, in which young farmers might get a scientific training, and to which soils and manures might be sent for analysis. The warmth with which the proposal was received by those present augurs well for the manner in which subjects of this kind are now taking hold of the agricultural and commercial mind. We heartily commend the subject to the attention not only of Chambers of Agriculture, but of Chambers of Commerce throughout the country.

EQUALLY satisfactory was the reference made at the half-yearly meeting of the Scottish Meteorological Society, held on January 25th, by Mr. Milne Home and Mr. Melvin, to the extent to which this country is lagging behind in its endeavours to increase our knowledge in scientific agriculture. The following resolution was passed at the meeting:—"This meeting having had explained to it a scheme proposed by Commodore Maury, of America, for obtaining reports from all countries of the state of growing crops, and also of the weather in the districts where these crops are growing, so as to warrant correct estimates of these crops as regard both quantity and quality; and having learnt that an influential agricultural society in America has approved of the scheme, and applied to the United States Government to carry it out, and to invite the co-operation of the Governments of other countries, agree to express a general approval of the scheme, and remit to the Council to make a favourable answer to Commodore Maury's communication."

AT the recent annual *conversazione* of the Sheffield Literary and Philosophical Society, the annual address was delivered by Mr. H. C. Sorby, F.R.S., as president. Among the remarkable inventions of the year he referred especially to the honour done to the town by Mr. Earnshaw's new method of integrating partial differential equations, and to the invention of the Moncrieff gun-carriage, where, by a simple application of mechanical principles, the force of the recoil is utilised, and made instrumental in protecting the men and the gun, and employed to raise it into a position for the next shot.

THE pages of the "Public Ledger Almanack" are filled with far more sensible matter than usually finds its way into similar publications. We find articles on the atmosphere, on the various descriptions of weather signals, and on the United States Coast Survey.

M. QUETELET reprints a eulogium on the late Sir John Herschel, spoken before the Academy of Science of Brussels, of which he was an associate.

MR. FAIRGREVE, the proprietor of Wombwell's No. 1 Menagerie, is retiring from business, and is going to dispose of the stock. The horses requisite for the dragging of the vans were sold the other day in Edinburgh, and realised over 1,400*l*. This sum gives a slight insight into the large capital invested by the owners of menageries. It is not known what he is going to do with the animals. There is some talk of another Zoological Garden being formed in Edinburgh, but nothing definite.

*AERIAL NAVIGATION IN FRANCE**

THERE has been a most interesting sitting at the Academy of Sciences, at which M. Dupuy de Lôme read a report on his newly tried and apparently successful system for steering air balloons. M. de Lôme is one of the most eminent—if he is not the most eminent—of living French engineers. He was the first to apply steam to ships of war, and he was one of the earliest designers of ironclad frigates. The piercing of a tunnel under the English Channel is another of M. Dupuy de Lôme's long-cherished projects, and he is one of the engineers who are about to commence that gigantic enterprise. During the siege of Paris by the Prussians, M. Dupuy de Lôme offered to construct a balloon which should have steering powers of its own, and so not be totally at the mercy of the winds. That some sort of guiding power was required for the balloons which were despatched from Paris during its investment by the Germans is shown by the fact that, out of sixty balloons sent out during that period, no less than fifteen failed to carry their contents to a place of safety, some falling into the sea and several into the hands of the Prussians. After much tiresome delay, M. Dupuy de Lôme's plans were accepted by the Government of National Defence, a credit of 40,000 francs (1,600*l.*) was opened for him, and he began to construct his balloon at the Palais de l'Industrie, in the Champs Elysées. So great was the difficulty, however, in constructing an immense balloon on a totally new system, in a city completely cut off from the rest of the civilised world, that M. Dupuy de Lôme's huge machine was not ready until just four days before the capitulation. When that event took place, the balloon had to be packed up and hidden away from the prying eyes of the Germans when they partially occupied Paris. Then came the Commune, and all the disorganisation which followed. It was only after much difficulty that M. Dupuy de Lôme obtained permission to make use of the buildings of the Fort Neuf at Vincennes, whence, on the 2nd inst., he started on his trial trip. Before proceeding to quote from M. Dupuy de Lôme's most interesting report, it may be as well to say a few words as to the end which the eminent aëronaut has proposed to himself. He does not pretend to be able to make independent progress in the teeth of the wind, but only to deviate from the direct set of the wind when running before it. He does not hope ever to be able to beat to windward, but only to tack to right or left with the wind. A sailor would say that M. Dupuy de Lôme wanted to be always running free with the wind on the quarter. So if the wind set straight from Paris to Brussels, an ordinary balloon could only land at some point between Paris and Brussels, or else beyond the Belgian capital. But with a balloon constructed on M. Dupuy de Lôme's system, the aeronaut might steer his course either on the port or starboard tack, and might descend at London or Cologne, as he saw fit.

Having said this much, let me try to describe the balloon which M. Dupuy de Lôme makes use of. Let your readers imagine a gigantic egg of inflated silk, the longer axis being horizontal; to this egg is attached an oblong car, something the shape of a punt. The motive of the inventor in choosing the ovoid form was at once to obtain greater stability for the car than could be hoped for with the old balloons, and at the same time to give the least possible hold to the wind. The diameter of the balloon is about two-fifths of its horizontal length from point to point. I take the following dimensions from M. Dupuy de Lôme's highly interesting report, read before the Academy of Sciences, only changing French metres into feet for the convenience of English readers.

Total length from end to end	118 ft. 6 in.
Diameter at the point of greatest circumference	49 ft. 2 in.
Diameter of the screw	29 ft. 6 in.
Number of blades	2
Number of turns of the screw in a minute, when the balloon is going eight kilometres (five miles) an hour faster than the wind	21

M. Dupuy de Lôme thus describes the rudder by which his balloon is steered :—"The rudder is a plain triangular surface. It is made of unvarnished calico, and is kept in its place by a horizontal yard six metres long at its lower extremity. It can turn easily on its forward extremity. The height of the rudder is five metres, and it has a superfices of fifteen metres." The car is next described—it is of wicker-work, and of sufficient size to contain comfortably the windlass for the screw, and eight men to work it ; the ventilator with which to manage the small balloon—we shall have to speak of this presently—and the man who attends to it. In all, fourteen persons can be carried in the car. The driving screw is directly carried by the car. The shaft of the screw is a hollow steel tube. This shaft is constructed so as to allow of the screw being easily dismounted when a landing is effected. The rudder is fixed to the balloon itself, and the screw, as we said, is below it, and immediately attached to the car. Two blades only are used in the screw instead of four, because when the ground is touched the two blades can be placed horizontally, so as to escape injury. Were there four blades, the screw would be almost certain to be broken whenever a landing was effected. The windlass which turns the screw is worked by four, or, if necessary, eight men, in a similar manner to the steering wheel of a ship—only the wheel is placed parallel to the axis of the car, instead of at right angles to it, in order to lessen the rolling occasioned by the movements of the men working the windlass. The material of which the envelope of the balloon is composed is white silk, weighing 52 grammes, not quite 2 oz. to the square metre, and a coarser lining weighing 40 grammes the square metre, and seven coats of india-rubber, which together weigh 180 grammes, a little over 6 oz. the square metre. Thus the whole weight of the external web of the balloon is 272 grammes, about 9 oz. to the square metre. In order to render the web of the balloon totally impermeable to the hydrogen gas with which it is inflated, the silk was painted over with a sort of gelatinous compound, invented by M. Dupuy de Lôme. The total weight of the two balloons when ready to start was 570 kilogrammes, or rather more than half a ton. The web of the balloon was reckoned to be capable of supporting a pressure of over 2,000 pounds to the square yard. I have mentioned the smaller balloon ; this is, more correctly speaking, only a division as it were of the larger balloon. It is formed by means of an inner skin, separating the bottom of the balloon from the rest. This compartment occupies about one-tenth of the whole cubic space of the balloon, and serves to keep it stiff, and of the required shape. By these means M. Dupuy de Lôme has attained the two ends he proposed to himself, viz., first, permanence in the shape of the balloon ; and, secondly, he has been able to give the whole apparatus an axis decidedly parallel to that of the force of propulsion.

Having thus endeavoured to give some account of the new aerial navigator—no easy matter without diagrams—it only remains for us to say a few words about M. Dupuy de Lôme's first experimental trip. There was half a gale of wind blowing at the time he started, and the screw had been slightly damaged. The spirited inventor did not hesitate, however, to make his contemplated ascent. The end justified his confidence ; for not only was he able to land near Noyon, in the Department of the Oise, some seventy miles north-east of Paris, but his balloon more than answered his expectations. The screw, when worked by four men, drove the balloon eight kilometres (about five miles) an hour quicker than the rate at which the wind was blowing ; so that M. Dupuy de Lôme not only "went like the wind," but actually went faster than the wind. By the use of the rudder the course of the balloon could be altered eleven degrees either way from the set of the wind, making a total deviation of twenty-two degrees. This is, of course, the greatest and most noteworthy result obtained by the new aerial machine. It may possibly be asked, What is the use of the screw when the wind carries your balloon at the rate of fifty-four kilometres, or nearly forty miles an hour? The answer is, that without the screw the rudder would be of little or no use. Every one knows that a ship without way on her steerage-way, as it is called, is nearly impossible to steer. And a balloon which has not, like a ship, a second element for the rudder to work on, is still more at the mercy of the wind. The next question is whether the screw cannot be turned by steam instead of by manual labour. But fire and hydrogen gas are bad neighbours, and the introduction of a steam-engine into the car—although it was hazarded some twenty years ago by one of our countrymen, Mr. Henry Giffard—would expose the aeronauts to the dangers of an explosion, followed by a descent to the earth, doubling in rapidity every sixteen feet, in accordance with the law of gravitation. Even with a steam-engine on board, there does not seem much cause to fear the "airy navies" of the inventor of ironclad ships just at present.

* Reprinted from the *Daily News*.

SOCIETIES AND ACADEMIES

London

Linnean Society, February 15.—Mr. G. Bentham, F.R.S., president, in the chair.—Prof. Wyville Thomson, F.R.S., Prof. Allman, F.R.S., and Prof. W. T. Thiselton-Dyer were elected Fellows.—"On the Habits, Structure, and Relations of the Three-banded Armadillo," by Dr. J. Murie. This animal is distinguished from the other members of the order Edentata by its habit of rolling itself into a ball like a hedgehog. The three bands act as hinges, by means of which this rolling-up is effected. It is also peculiar in walking on the points of its toes, instead of, like other armadillos, on the whole foot. It may be considered as a connecting link from the armadillo to the extinct glyptodon, and thence to the megatherium, and so on to the pachyderms.—"On a Chinese Artichoke-Gall," by A. Müller.—"Comparative Geographical Distribution of Butterflies and Birds," by W. F. Kirby. The total number of species of birds is stated by Dr. Sclater as 7,500, and that of butterflies is about 7,700, showing a remarkable closeness. If the surface of the globe is marked off into the divisions proposed by Dr. Sclater, we find in the Palæarctic region (Northern Europe and Asia), including about 14,000,000 square miles, 630 species of butterflies and 630 of birds; in the Indian region, including Asia south of the Himalayas, about 1,200 butterflies and 1,500 birds; in the Australian region 725 butterflies and 1,000 birds; in the Nearctic or North American region, 480 butterflies and 660 birds; in the Neotropical or South American region, 4,200 butterflies and 2,250 birds; thus, in five divisions there is a preponderance of birds, which is balanced by a very large excess of butterflies in the sixth region.—An interesting discussion followed, in which Mr. A. R. Wallace, Mr. Sharpe, Mr. Stainton, and others took part, and it was shown that if Dr. Gray's estimate of the number of species of birds is taken, viz., 10,000, which is no doubt more correct than Dr. Sclater's, the apparent parallelism vanishes; that in limited districts, as the British Isles, there is no resemblance between the number of butterflies and of birds; that in Mr. Kirby's paper no reference is made to the number of birds in each region that are migratory, a most important consideration; and that the conditions of the natural features of the country, as the prevalence of forests, may be favourable to the abundance of insects, and unfavourable to that of birds.

Chemical Society, February 15.—Dr. Frankland, F.R.S., president, in the chair.—Prof. Roscoe, F.R.S., gave an account of some of his recent researches on the element tungsten, under the title "On the study of some tungsten compounds." The author, after giving a short *resumé* of the labours of other chemists on those compounds of tungsten which he had been investigating, proceeded to describe their properties, and the methods of preparation he had employed to obtain them. As the result of his labours he has definitely settled that the metal tungsten is a monad element with the atomic weight 184, and has also showed the cause of the error of the French chemist Persoz, who assigned 153 as the atomic weight. A collection of very fine specimens of tungsten compounds was exhibited by the Professor.

Royal Geographical Society, February 12.—Sir H. C. Rawlinson, K.C.B., president, in the chair. The President announced that the expedition for the search and relief of Dr. Livingstone left England on Friday last, and was at that moment probably crossing the Bay of Biscay *en route* for Zanzibar. The subscriptions from all sources, including the balance of the Government grant lying at Zanzibar, amounted to nearly 5,000*l*. Of this sum about 2,800*l*. will have been expended by the time the expedition leaves Zanzibar for the interior; the remainder would be held in reserve for contingencies very likely to occur. He read also to the meeting a letter from Earl Granville to the Sultan of Zanzibar, stating the great interest the Government and people of England took in Dr. Livingstone, and recommending the expedition organised by the Royal Geographical Society of England to his Highness's good offices; and another to Dr. Kirk, Acting Consul at Zanzibar, authorising him to apply 654*l*., the balance of the Treasury grant of 1870, to the purposes of the expedition. So far everything connected with the expedition had been most satisfactorily and expeditiously carried out; and a message ordering the preparation of escort and porters at Zanzibar, sent as far as Aden by telegraph, would reach Zanzibar in the unprecedentedly quick space of fourteen days. Letters had been received from Dr. Kirk of so recent a date as Dec. 16, and they informed us that no news whatever had been received since September from the interior, but that the war between the Arabs and the people of Unyamwezi would be continued. This would necessitate the adoption of an entirely new route by the expedition now on its way.—Letters were then read concerning Sir Samuel Baker's expedition. The President stated that he had received from the Prince of Wales the original letters of Sir Samuel, copies of which his Royal Highness had sent to the *Times*. A letter, three days later in date, contained the news that a fertile portion of the Bari territory beyond Gondokoro had been acquired, and that Lieut. Baker would have charge of the steamer for the navigation of Lake Albert Nyanza.—A paper was then read by Sir Harry Parkes (British Minister at Japan), entitled "Captain Blakiston's Journey round the Island of Yezo." Sir Harry explained that his office with regard to the paper was that of reducing into readable bulk the voluminous journals which Captain Blakiston had communicated through him to the Society, and of adding some necessary explanations. Yezo was the northernmost island of Japan, larger by 3,000 square miles than Ireland, and rising in importance from its position and its great fertility and mineral wealth. Captain Blakiston, the well-known explorer of the Yang-tsze-Kiang, since resident in Hakodadi in the south of Yezo, had enjoyed the peculiar advantage of travelling with the privileges of a Japanese official. He went by sea to Akis Bay, on the south-east coast, and thence by land almost entirely along the sea coast (the interior being without roads or Japanese settlements) round the island to Hakodadi. The native inhabitants are the singular isolated people called Hairy-men, or "Ainos," a robust race, apparently of Aryan extraction, and nearest allied to certain sections of Sclavonians, distinguished by the thick growth of hair on the body, as well as head and beard.

Photographic Society, February 13. — The officers and council for the ensuing year were elected, and the accounts of the society explained by the treasurer, who reported the society free from debt and with a satisfactory balance in hand. The report of the council was read and adopted.—Dr. Anthony read a paper "On various modes of Plate-cleaning." He stated that his experience went to show that the employment of cyanide of potassium was better than any other agent for the purpose, the plates being treated for a very brief period in the cyanide solution and then washed in water. He found mechanical methods generally rendered the bath unclean, and for this reason also deprecated the application to the glass plate of an albumen substratum. The specimens of Niepce de St. Victor were exhibited.

Edinburgh

Royal Society, February 19.—Sir Alexander Grant, Bart., vice-president, in the chair.—1. "Remarks on Contact-Electricity," by Sir William Thomson. 2. "On the Curves of the Genital Passage as regulating the movements of the Fœtus under the influence of the Resultant of the Forces of Parturition," by Dr. J. Matthews Duncan. 3. "On a Method of Measuring the Explosive Power of Gaseous Combinations," by James Dewar. 4. "Note on Modification of Sprengel's Mercurial Air-Pump," by James Dewar. 5. Prof. Alexander Dickson exhibited a series of Abnormal Fir Cones, with remarks.

Paris

Academy of Sciences, February 12.—MM. Delaunay and Serret protested against the insertion in the *Comptes Rendus* of a note by M. Renou relating to asserted inaccuracies in the publications of the Paris Observatory.—The controversy on fermentation and heterogeny was continued by M. Pasteur reading a reply to M. Fremy, and M. Chevreul a communication on the history of ferments after Van Helmont. M. Engel also presented a morphological investigation of the various kinds of alcoholic ferments, which he describes as forming two genera, *Saccharomyces* (Meyen) with seven species, and *Carpozyma* (*gen. nov.*) with one species. The characters of these forms were illustrated with outline figures.—M. Bertrand presented the solution of an arithmetical question by M. Bougaev; M. Serret a note by M. E. Combescure on some points in the inverse differential calculus; and a note by M. A. Mannheim on the determination of the geometrical connection which exists between the elements of the curvature of the surface of the principal centres of curvature of a given surface.—M. de Saint-Venant presented an elaborate report upon a memoir by M. Kleitz, entitled, "Researches upon the molecular forces in liquids in motion, and their application to hydrodynamics."—M. de Pambour read a note on the theory of hydraulic wheels, relating to the reaction-wheel.—M. Saint-Venant communicated a note by M. Boussinesq on the equation of the partial derivatives of the velocities in a homogeneous and ductile solid undergoing deformation parallel to a plane.—M. Serret presented a note by M. de Tastes in reply to a recent note by M. Ciotti on the employment of vibrating elastic plates as a means of propulsion. M. de Tastes stated that the elastic plate propeller is his invention, communicated by him to M. E. Ciotti.—M. E. Dubois presented a reply to M. Ledieu's objections to the employment of the marine gyroscope.—M. Delaunay presented a note by M. C. Wolf on the reflecting power of mirrors of silvered glass, and their application to astronomical purposes.—A note by M. D. Genez on the absorption-bands produced in the spectrum by solutions of hyponitrous, hypochlorous, and chlorous acids, was communicated by M. H Sainte-Claire Deville.—A note by M. Baudrimont on the recent experiments of M. Poëy with regard to the influence of violet light upon vegetation was read, in which the author stated that he had arrived at totally different results, having found that violet light was fatal to vegetation.—A great number of communications from all parts of France, and also from Belgium, Switzerland, and Algeria, relating to the aurora of February 4, were laid before the Academy; they included notices of magnetic disturbances observed in the telegraphic lines.—M. Delaunay presented a paper by M. E. Stephan containing a list of nebulæ discovered and observed at the Observatory of Marseilles.—M. E. Vicaire read a reply to Father Secchi's observations on the temperature of the solar surface.—Some remarks were read by M. Harting on the saccharine matter observed by M. Boussingault on lime trees, which he ascribed to the action of aphides in accordance with the commonly received opinion. He stated that the saccharine secretion produced by those insects consists in great part of cane sugar. M. Boussingault in reply said that in the case observed by him the saccharine exudation appeared before the aphides, and that it contained cane-sugar, grape-sugar, and dextrine.—M. Le Verrier also read an extract of a letter from M. Follie on this subject.—M. Bussy presented a report upon a memoir by M. Louvel, describing a process for preserving grain *in vacuo*. The author suggested storing grain in air-tight granaries, in which a partial vacuum may be produced by a powerful air pump; he described the construction of the apparatus, and stated that a granary such as he proposed of the capacity of ten cubic metres (about 370 cubic feet) and containing 100 hectolitres of wheat would cost 750 francs. He stated that by this process the ravages of insects are effectually stopped.

BOOKS RECEIVED

English.—The Origin of Species, 6th edition: C. Darwin (Murray).—Transactions of the Society of Biblical Archæology, Vol. i., Part 1. (Longmans).—Index of Spectra: W. M. Watts (H. Gillman).—Recollections of Past Life: Sir H. Holland (Longmans).—New Theory of the Figure of the Earth: W. Ogilby (Longmans).

PAMPHLETS RECEIVED

English.—Eighth Annual Report of the Belfast Naturalists' Field Club for 1871.—Italy in England.—Five Speeches on the Liquor Traffic: G. O. Trevelyan.—Description of a new Anemometer: J. E. H. Gordon.—Psychic Force and Modern Spiritualism: W. Crookes.—On the Mechanism of Accommodation for Near and Distant Vision: Dr. R. E. Dudgeon.—Address of Thos. Hawkesley on his Election as President of the Institution of Civil Engineers.—The Reflecting Media of the Atmosphere a Natural Law: J. Shaw.—Preliminary Report of the Scientific Exploration of the Deep Sea in H.M. surveying vessel *Porcupine*.—Report of the Ladies' National Association for the Repeal of the Contagious Diseases Act.—Contributions to the Flora of Berkshire: Jas. Britten.—A Grave Question for Englishwomen.—What is the Shape of the Earth: Scævola.—On the Elevation of Mountains by Lateral Pressure: Rev. O. Fisher.—Meteorology of West Cornwall and Scilly, 1871.—Journal of the Iron and Steel Institute, Jan. 1872.—On Teaching Universities and Examining Boards.—Child's Public Ledger Almanac, 1872.—Every Saturday, No. 1.—Pauperism and Crime: Robert Hill.—The Mining Magazine and Review, No. 2.—The Quarterly Journal of Education, Jan. 1872.—Righthandedness: D. Wilson.—Address at the Anniversary Meeting of the Entomological Society: A. R. Wallace.—Proceedings of the Geologist's Association, Oct. 1871.—The National Church, No. 1.—The Scottish Naturalist, No. 5.

American and Colonial.—Lippincott's Magazine for Jan. 1872.—Australian Vertebrata, Fossil and Recent Mammals: G. Krefft.—Catalogue of the Meteoric Collection of C. U. Shepard.—Proceedings of the Asiatic Society of Bengal, 1871, Nos. 10, 11.—Appleton's Journal, No. 143.—Proceedings of the Academy of Natural Sciences of Philadelphia, April-Sept. 1871.—A Letter concerning the Deep-Sea Dredging, addressed to Prof. B. Peirce by L. Agassiz.—Annual Report of the Secretary of the Interior for the year ending Oct. 1871.—Report on the Geological Structure of Prince Edward's Island: Prof. Dawson.—Nitro-Glycerine, as used in the Construction of the Hoosac Tunnel: G. Mowbray.—Cruise of the School-ship *Mercury* in the tropical Atlantic Ocean.—Correspondence relative to Deep-Sea Dredging.—The Indian Antiquary, No 1: Edited by Jas. Burgess.—Monthly Notices of the Meteorological Society of Mauritius.—The School Laboratory of Physical Science, Nos. 3 and 4: G. Hinrichs.

Foreign.—La Belgique Horticole, Dec. 1871–Feb. 1872.—Bulletin de l'Académie Royale des Sciences de Belgique, No. 12, 1871.—Verhandlungen der k. k. geologischen Reichsanstalt zu Wien, No. 16, and No. 1, 1872.—Anzeigen der k. Akademie der Wiss. math.-naturforsch. Classe, 1871, No. 1–29.—Bulletin de la Société d'Anthropologie de Paris, June and July, 1870.—Sitzungsberichte Isis in Dresden, July-Sept. 1871.—Die geographischen Verbreitung der Coniferen u. Gnetaceen: R. Brown.—Zeitschrift für Ethnologie, Heft 2.—Journal général de l'imprimerie.—Notice sur Sir J. F. W. Herschel: Ad. Quetelet.—Jahrbuch der k. k. geologischen Reichsanstalt zu Wien.—Memoire della Societa dei spectroscopisti Italiani, No. 1. Un expérience relative à la question de vapeur vésiculaire: F. Plateau.—Recherches expérimentales sur la position du centre de gravité chez les insectes: F. Plateau.—Annali di Chimica, No. 1, 1872.

DIARY

THURSDAY, February 22.

Royal Society, at 8.30.—On a New Hygrometer: W. Whitehouse.—On the Contact of Surfaces: W. Spottiswoode.

Society of Antiquaries, at 8.30.—The Roman Villa at Holcombe: Capt. Swann, F.S.A.—The Kirkham Chantry, Paignton, Devon: Sir W. Tite.

London Institution, at 7.30.—On South Africa and its Diamond Fields: Prof. T. R. Jones, F.G.S.

FRIDAY, February 23.

Royal Institution, at 9.—On Social Influence of Music: Mr. H. Leslie.

Quekett Microscopical Club, at 8.

SATURDAY, February 24.

Royal Institution, at 3.—On the Theatre in Shakespeare's Time: Wm. B. Donne.

SUNDAY, February 25.

Sunday Lecture Society, at 4.—On the Education of Women: Mrs. Fawcett.

MONDAY, February 26.

Geographical Society, at 8.30.

London Institution, at 4.—Elementary Chemistry: Prof. Odling, F.R.S.

TUESDAY, February 27.

Royal Institution, at 3.—On the Circulatory and Nervous Systems: Dr. Rutherford.

WEDNESDAY, February 28.

Society of Arts, at 8.30.

THURSDAY, February 29

Royal Society, at 8.30.

Society of Antiquaries, at 8.30.

Royal Institution, at 3.—On the Chemistry of Alkalies and Alkali Manufacture; Prof. Odling, F.R.S.

CONTENTS

NOTICE

We have received a letter signed "M.," which we hold over till informed (in confidence) of the name and address of the writer. Anonymous communications can in no case receive attention.

THURSDAY, FEBRUARY 29, 1872

SCIENCE STATIONS

WE shall not be far wrong, we imagine, in supposing that the article by Dr. Dohrn in a recent number of NATURE on "Zoological Stations" has attracted considerable attention among thoughtful men. We may, indeed, congratulate zoologists that so important a task has been taken in hand by one in every way so well fitted to accomplish it; and it will gratify our readers to learn that the cheery energy and bright enthusiasm of the German anatomist is fast overcoming the obstacles which his scheme naturally met with in the indolent city of the South, whose lands are so rich in classic ruins, and seas so full of Darwin-speaking embryos. At the risk of spoiling a good work we venture to add to his remarks some further suggestions, confining ourselves, however, to one or two points.

In the first place, we will be bold enough to express the doubt whether it will be advisable to separate so entirely, as Dr. Dohrn recommends, the stations in England from the work of teaching. The establishment of such stations will be rendered infinitely easier if they can in any way be made self-supporting. Dr. Dohrn hopes, if we understand him rightly, to pay the expenses of the Naples station out of the fees of the Gentile sightseers, who will be allowed to stroll about in the outer court of his embryological temple. There can be no such hope for any like English temple. Yet a very considerable share of the necessary funds might without difficulty be raised, and a Philistine British public might be made to believe that it was getting its money's worth for its money, if the work of teaching, which is palpable, which may be measured and valued, and for which a receipt in full may be given, were to go on hand in hand with the immeasurable and invaluable work of original inquiry. There would thus naturally grow up around the station a school of sound zoology; otherwise there would be great danger of its becoming a resort of ambitious *privat-docents* anxious chiefly to find a notochord where nobody had found it before, or a home of some narrow zoological clique.

Much might be said for the establishment somewhere on our British coasts of such a school of zoology on the theory of a geographical distribution of scholarship, and the existence of particular habitats best suited for particular branches of learning. Sufficient foundations for such a theory are at hand. It is easy to understand why Edinburgh, with her sea close by, has raised so many brilliant zoologists. We can see why Manchester in the past and in the present has done so much for chemistry. And, to look at the matter from another point of view, one gets a glimpse of the reason why high mathematics flourish at Cambridge, when one gazes at her fenny flats, where, if the conception of three dimensions be once reached, that of four is soon gained, and feels the fogs and mists which wash out of the mind everything that is not held fast by formulæ. The natural habitat for an English school of zoology is surely some bright spot on our southern coast.

Nor need such an institution necessarily have an independent isolated existence. There is too great a want of community in our English Universities and Colleges especially in matters of natural science. There is one zoology at Oxford, another at Cambridge, another at Jermyn Street, and these three have miserably little dealings with one another. What immeasurable good would a place of higher teaching do, where for a season, or for a term, the zoological students of all the Universities might mingle together with mutual diffusion of ideas! * The mere opportunity of material would be a great thing: the Cambridge student would lift his ideas above the line of beautifully prepared vertebrate skeletons, the Oxford man would benefit by the change of diet from Anodon and Astacus, and the London man would learn to see actual things instead of reading about them in books. But the greatest thing of all would be the catholic enthusiasm for biological learning, which such an institution could not fail to generate and foster.

Another remark which we would wish to make takes on somewhat the shape of a complaint against Dr. Dohrn, that he has confined to one science ideas which should properly belong to all the sciences of observation. It is well to have a Biological station, but it is far better to have a station at once Biological, Astronomical, and Meteorological. Let us imitate Dr. Dohrn in giving our views a concrete form. The eclipse party on their outward, and even on their homeward voyage, cannot fail to have been struck with the bright clear air of the North Red Sea. There is the very land of observation. It is impossible for any one with a fragment of a mind within him to sojourn on those delightful shores, where the eye rejoices in its power, where the air helps vision instead of hindering it, where the water is as clear and transparent as the air elsewhere, without the desire springing up to be a naturalist by day and an astronomer by night. And this blessed region is now little better than a week's journey from the fogs of London. Nothing could be easier than to establish at no great expense a Science Station at some spot on the shores of the Red Sea, a little south of Suez. Suez itself is for many reasons undesirable, but the little village of Tor suggests itself as being a very suitable neighbourhood. There would be comparatively little difficulty in getting supplies, or in going and coming to and fro. The naturalist, the astronomer, the meteorologist, with the Palestine explorer as an occasional helpmeet, might spend here a winter, or rather many winters, in which pleasure and profit would be running a hard race together.

We cannot help thinking that such an idea has only to be mooted to be at once caught up and set in action. The outlay of the initial building and arrangements need not be heavy, while the yearly expenditure might be kept within comparatively narrow limits. Such an undertaking is one which Government might justly take in hand, but it is also one which private liberality might largely aid, and to which contributions might come from the funds of our ancient seats of learning. In any case we fairly think it is matter deserving serious attention, and as such we leave it to our readers.

* It is impossible in a short article to develope a complete scheme; we might indicate our ideas, however, by suggesting that the right to study for one or more terms in the station might be granted as a sort of scholarship to promising biological students selected from all our great teaching institutions.

BURTON'S ZANZIBAR

Zanzibar: City, Island, and Coast. By Richard F. Burton. In 2 vols. (London: J. Murray, 1872.)

IN these two bulky volumes Captain Burton gives us, after a lapse of thirteen to sixteen years, a narrative of his adventures and explorations in the island of Zanzibar, the neighbouring smaller islands, the adjacent coast of the mainland, and the Highlands of Eastern Africa intervening between the coast and the great Victoria N'yanza, the publication having been delayed by a series of remarkable accidents. As in everything else that Captain Burton has written, the volumes are full of graphic delineations of the natural features and inhabitants of the country, combined with not a few details of a personal character which have not the same interest for the general reader.

In 1856 Captain Burton laid before the Royal Geographical Society his desire once more to explore Equatorial Africa; a committee was formed to assist him in his undertaking, a grant of 1,000*l.* was obtained from Lord Clarendon, then Secretary of State for Foreign Affairs, and on September 16th the enterprising traveller received formal permission, "in compliance with the request of the Royal Geographical Society, to be absent from duty as a regimental officer under the patronage of Her Majesty's Government, to be despatched into Equatorial Africa, for a period not exceeding two years, calculated from the date of departure from Bombay, upon the pay and allowances of his rank." On December 26th in that year he landed at Zanzibar, the first view of which is thus attractively described:—

"Earth, sea, and sky, all seemed wrapped in a soft and sensuous repose, in the tranquil life of the Lotos-eaters, in the swoon-like slumber of the Seven Sleepers, in the dreams of the Castle of Indolence. The sea of purest sapphire, which had not parted with its blue rays to the atmosphere—a frequent appearance near the equator—lay basking, lazy as the tropical man, under a blaze of sunshine which touched every object with a dull burnish of gold. The wave had hardly energy enough to dandle us, or to cream with snowy foam the yellow sandstrip which separated it from the underwood of dark metallic green. The breath of the ocean would hardly take the trouble to ruffle the fronds of the palm, which sprang like a living column, graceful and luxuriant, high above its subject growths. The bell-shaped convolvulus (*Ipomæa maritima*), supported by its juicy bed of greenery, had opened its pink eyes to the light of day, but was languidly closing them, as though gazing on the face of heaven were too much exertion. The island itself seemed over-indolent and unwilling to rise; it showed no trace of mountain or crag, but all was voluptuous with gentle swellings, with the rounded contours of the girl-negress, and the brown-red tintage of its warm skin showed through its gauzy attire of green. And over all bent lovingly a dome of glowing azure, reflecting its splendours upon the nether world, whilst every feature was hazy and mellow, as if viewed through 'woven air,' and not through vulgar atmosphere."

A residence, however, of some months in the island by no means established the impression which its first appearance might convey, of its being a terrestrial paradise. The city of Zanzibar itself is a miserable, ill-built place, fœtid and unhealthy; while the personal appearance and habits of the natives are repulsive in the extreme. The climate is remarkably uniform as to temperature, the result of nine months' observation showing a range of 18°—19° F. only. The medium temperature of January is 83·5°; of February, the hottest month in the year, about 85°; and the mean gradually declines till July, the coolest month, 77°. The mean average of the year is between 79° and 80°. The barometer is almost uniformly sluggish and quiescent, a few tenths above or below 30 in. representing the maximum variation, even under the influence of a tornado. Uniform, however, as is the temperature, the degree of humidity of the atmosphere varies excessively. At certain seasons the amount of moisture exceeds that of the dampest parts of India, and the annual rain-fall is in some years double that of Bombay, varying from 100 to 167 inches. The Msika, or principal rainy season, lasts from April to June; the island is enveloped in a blue mist, and the interior becomes a hot-bed of disease; the hair and skin are dank and sodden; shoes exposed to the air soon fall to pieces; paper runs and furniture sweats; the houses leak; books and papers are pasted together; ink is covered with green fur; linens and cottons grow mouldy; and broadcloths stiffen and become boardy. This excess of damp is occasionally varied by the extreme of dryness. During the prevalence of the dry wind cotton cloth feels hard and crisp, books and papers curl up and crack, and even the water is cooled by the excessive evaporation. Earthquakes are all but unknown in Zanzibar, a single shock being recorded as having been felt in 1846. Tornadoes are frequent, but the cyclones and hurricanes of the East Indian islands rarely extend to this coast. During fourteen years there was but one tourbillon strong enough to uproot a cocoa-nut tree.

The prosperity of Zanzibar depends almost entirely on its vegetable productions, and chiefly on the cocoa-nut and the clove. The former supplies the natives with nearly all their wants—food, wine, spirit, cords, mats, strainers, tinder, firewood, timber for houses and palings, boats and sails; and Captain Burton calculates that in 1856 12,000,000 nuts were exported for the soap and candle trades. The sugar-cane might be grown to great advantage, but for the constitutional indolence of the inhabitants. Cotton has been tried, but does not thrive; and coffee has not been cultivated to any extent. The fruits in greatest request by the islanders are the mango, the orange, the banana or plantain, the pine-apple, and the bread-fruit—all, however, with the exception of the banana and an inferior kind of orange, being introduced exotics; the pine-apple has become perfectly naturalised. The most important production of the island is the clove, which does not, however, produce crops comparable to those of the East Indies either in quantity or quality, owing to want of skill and intelligence in its cultivation. The copal of commerce is obtained chiefly from the neighbourhood of Saadani, on the opposite coast of the mainland; and Captain Burton entirely confirms the account of its production already communicated to the Linnean Society by Dr. Kirk, that it is a gum, or resin, exuding from wounds in the stem of a small tree or large shrub (*Hymenæa verrucosa*) belonging to the order Leguminosæ.

Captain Burton's first expedition from Zanzibar was to the smaller island of Pemba, lying to the north, and thence to Mombasah, on the coast 4° south of the line. the capital of Northern Zanzibar, the best harbour on the Zanzibar coast, land-locked by coral islands. The town itself is built on the largest of these islands, where the climate is hotter, drier, and healthier than that of Zanzibar. Here he did not attempt to strike inland, the weather and the hostility of the native tribes being unfavourable, but returned along the coast southwards to Pangani, and thence inland to Fuga, the capital "city" of Usambara, in the Highlands of Eastern Africa. In order to gain a complete knowledge of the Zanzibar coast, he also paid a visit to the island and port of Kilwa, situated beneath the ninth degree of south latitude. Here are the remains of an ancient town of considerable size, with respect to which many legends are current among the natives; but the gradual sinking of the coast has rendered the ancient site uninhabitable. Although at the present time a miserable and fœtid collection of squalid huts, Kilwa was found in 1500 by the Portuguese a town of great prosperity, the capital of Southern Zanzibar, and ruling the coast as far as Mozambique and Sofala; but the curses of European wars and the slave-trade have desolated the once thriving country. Captain Burton does not think very highly of the so-called "free labour" system, which he terms "the latest and most civilised form of slavery in East and West Africa."

The most important expedition made by Captain Burton was, however, that undertaken between 1857 and 1859 to Kazeh in the Ukimbu district, upwards of 500 miles from the coast, and about 2° south of the southern shore of the great Victoria N'yanza, in company with Captain Speke. But as this journey has already been illustrated in his own "Lake Regions of Central Africa," and the country has been further described by Colonel Grant and Captain Speke, he does not again enter into details respecting it; but thus sums up what he considers its geographical results:—"That the Boringo is a lake distinct from the 'Victoria N'yanza' with a northern effluent the Nyarus, and therefore it is fresh water; that the N'yanza, Ukara, Ukerewe, Garawa, or Bahari y a Pili, is a long narrow formation, perhaps thirty miles broad, and 240 miles in circumference, and possibly drained to the Nile by a navigable channel; that the N'yanza is a water, possibly a swamp, but evidently distinct from the two mentioned above, flooding the lands to the south, showing no signs of depth, and swelling during the low season of the Nile, and *vice versâ*; and that the northern and north-western portions of the so-called 'Victoria N'yanza' must be divided into three independent broads or lakes, one of them marshy, reed-margined, and probably shallow, in order to account for the three effluents within a little more than sixty miles."

The botanical results of this journey are about to be illustrated by Colonel Grant, in a magnificent volume, to be published by the Linnean Society, which it is understood will be illustrated by 600 plates, the cost of which will be defrayed entirely by the gallant author.

One chapter is devoted to a sketch of the labours of Captain Burton's old comrade, Captain Speke. Though tribute is here paid to his many excellent qualities, we regret to be again introduced to the details of the estrangement which grew up between the explorers, culminating at the meeting of the British Association at Bath, when the two companions in arms met as strangers, advocates of two rival "Nile-theories," as to the origin of the Father of rivers.

In the Appendices, Captain Burton gives some useful details of the meteorology, commerce, &c., of Zanzibar. A well-executed map helps to illustrate the author's journeys, without a constant reference to which the narrative is by no means clear; but we cannot commend the style in which the woodcuts interspersed here and there are executed.

OUR BOOK SHELF

Deschanel's Natural Philosophy. By Prof. Everett. Part III., Electricity and Magnetism. (London and Edinburgh: Blackie and Son.)

IN the Preface by the translator of the present volume, it is said, with much truth, that "the accurate method of treating electrical subjects, which has been established in this country by Sir W. Thomson and his coadjutors, has not yet been adopted in France; and some of Faraday's electromagnetic work appears still to be very imperfectly appreciated by French writers." Accordingly we find that the translator has added a considerable amount of matter, and more especially two important chapters, one on the electrical potential and lines of electric force, and the other on electrometers, together with an appendix on electrical and magnetic units. Dr. Everett has thus considerably improved a book, which, in its original form, was already a good one. The ordinary branches of the subject are unfolded, the plates are good, and the explanations are full and clear. The portion devoted to magnetism is in this, as apparently in all such general treatises on natural philosophy, considerably the most defective part, and especially in the sections which relate to terrestrial magnetism. The whole of that question is most insufficiently dealt with. The treatment of the secular changes in the magnetic elements is confined to twelve lines, where it is said that "declination and dip vary greatly, not only from place to place, but from time to time;" but from which we should expect that the unlearned reader would be led into the error that intensity is uniform. Then, again, the vast subject of changes in the elements, such as are not secular, is confined to one short paragraph, headed "Magnetic Storms"! The intrinsic importance of the subject of terrestrial magnetism, and the great and increasing interest attaching to it, no less than the extreme beauty of many of its investigations and results, entitle it to a much larger notice than the very imperfect one in this volume. The chapter on the Telegraph contains useful matter, and especially a description of an autographic telegraph, an instrument which, while interesting and ingenious, has not often found its way into such treatises. We miss such points as how to find the locality of a fault in a telegraph wire, which we might the more expect to see treated of when we consider the full explanation which is given of Ohm's laws, and when we see such elaborate details as to some telegraphic instruments as are entered into in the chapter in question. The chapters on the heating effects of currents, and on electrolysis, are clear. The question of electromotive force, and of the means of determining it, might have been entered into more fully; and, generally, from the character of the chapter on the potential, we might have expected to see a little more introduced concerning points which may be elucidated by the application of the principle of the conservation of energy. JAMES STUART

Medizinische Jahrbücher, herausgegeben von der k. k. Gesellschaft der Ärzte, redigirt von S. Stricker. Jahrgang 1871. Heft iv. Mit 4 Holzschnitten. (Wien: 1871.)

THIS part, which concludes the first volume of Stricker's Jahrbuch, contains: (1) Researches on the Inorganic Constituents of the Blood, by Adolph Jarisch. Jarisch gives the details of an improved method by which blood can be collected from the vessels of a dog without the loss of any of the water by evaporation, whilst at the same time, being frozen, it loses its disposition to coagulate, and when subsequently thawed can be readily manipulated. The mean of four analyses gave the following results:—

Phosphoric acid anhydride	0·1103
Sulphuric acid anhydride	0·0358
Chlorine	0·2805
Potash	0·0342
Soda	0·3748
Lime	0·0112
Magnesia	0·0058
Oxide of iron	0·0948
Total ash found	0·8922
Calculated	0·8640

In Verdeil's treatise, the amount of ashes of fresh blood is stated to be on the average 6·45 per cent. Jarisch points out that this must be an error of the press, his own results giving only 0·864 per cent., a difference that is too great to be regarded as an error of analysis. 2. An essay on the Centres of Vascular Nerves, by Dr. Soboroff. In this paper Dr. Soboroff shows from the results of experiments performed on frogs that the nerves supplying the vessels of the web of the foot proceed from the spinal cord, and run into the sciatic nerve. 3. On the presence of Fungi in the Blood of Healthy Men, by Adolph Lorstorfer. Lorstorfer drew blood from the fingers of eleven people who considered themselves in perfect health with every precaution to avoid contamination with dirt, and examined the specimens daily with a Hartnack microscope, ocular 3, objective 10. During the first two days he observed nothing remarkable, except in some cases a few scattered groups of small granules. On the third day similar groups were always found, though still scattered. The granules were of equal size, considerably larger than those of the colourless blood corpuscles, but without any definite arrangement. On the fourth day they had increased in size, and were arranged in groups of four, so as to resemble the well-known *Sarcina ventriculi*. On the fifth day the granules had slightly increased in number and size, but after this date no change was observable up to the tenth day, when the preparations became unserviceable. Lorstorfer thinks his experiments render it probable that the germs of *Sarcina ventriculi* exist in the blood as a natural condition. There are three other papers, but they are all of a purely professional nature. One being by Hofmokl on Resection of the Upper and Lower Jaw: one by Bresslauer on Typhus: and one by Popoff on Pneumonia.

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.*]

Development of Barometric Depressions

I have only just had my attention called to the critique on "The Laws of the Winds prevailing in Western Europe," in Nature of Jan. 11, which I have seen to-day for the first time. Though it is now rather late to do so, I may perhaps be permitted to point out some unintentional misrepresentations of my views into which the writer appears to me to have fallen.

He considers it improbable in the extreme that the course of baric depressions should be regulated "by one law" in intra-tropical, and by "a totally distinct law" in extra-tropical regions of the globe. I pointed out (pp. 40, 41) that in temperate latitudes the general distribution of atmospheric pressure commonly tends to transfer local depressions in an eastward direction; while the influence of precipitation resulting from the mean distribution of solar heat propagates them in the same direction. Since the reversal of pressure-distribution which accompanies polar periods only retards the eastward progression, I drew the conclusion that, in temperate latitudes, the most important of the two factors of the progression is the influence of precipitation, and accordingly I devoted the first part of my work to this, with the promise (which I hope shortly to redeem) that the motive effect of the general pressure-distribution shall be described in Part II. All this your reviewer ignores. Had I been engaged in a discussion of the tropical cyclones, I should have proceeded in an inverse order; since the most important factor of their westward progression appears to be the mechanical influence of the distribution of surrounding pressures. It is, however, important to observe that as in temperate, so in tropical latitudes, these two influences are commonly coincident in direction. In the West Indies, *e.g.*—at those periods when cyclones prevail—mean temperatures are lower on the south, or left, than on the north, or right, of their course; and a similar remark applies, *mutatis mutandis*, to the typhoons of the Indian and China seas.

Briefly, my position is this. The influence of the general distribution of temperatures, and that of the general distribution of pressures, may be practically regarded as two forces, A and B, from which the progression of local depressions results. Both of these commonly act in the same direction—in temperate latitudes producing eastward, and in tropical westward, progression. But of these A is the preponderating influence in temperate, B in tropical latitudes; partly because the influence of precipitation on the surface-currents increases with diminution of temperature, and partly because the currents resulting from the general distribution of pressures are far more constant and of vastly greater extent, in proportion to the extent of the cyclones, in tropical than in temperate latitudes. I am convinced that the attempt to simplify the rules which regulate the progression of depressions by striking out either of these factors, or by the substitution of J. K. L.'s single law, will meet, as it has hitherto met, with failure.

Your reviewer also ignores what I have said (pp. 28, 29) as to the occurrence of heavy precipitations unproductive of baric depression, and thinks it necessary to travel to Khasia or to the Himalayas to find illustrations of a truth which it was never intended to deny. Every one conversant, as he considers me to be, with the meteorology of Western Europe alone, is aware that heavy and extensive precipitation not uncommonly occurs without producing retrograde circulation (and sometimes with generally increasing pressures), where antecedent atmospheric conditions do not favour such developments. The reviewer concedes that the immense precipitation in the Himalayas "probably causes a very great barometric depression;" a concession which is not to be accepted, both because such a reference to antecedent probabilities is inapplicable to empirical science, and because the fact itself may be denied. But supposing this great Himalayan depression to exist, and no retrograde circulation (as J. K. L. maintains) to be developed around it, his discovery of a region in which "Ballot's rules" are contravened, is indeed one of no small importance.

Into the wide question of the influence of the earth's rotation I will not here enter, further than to remark that the hitherto admitted universality of the rules connecting the direction of all atmospheric currents with the distribution of surrounding pressures, and the variation of these rules in the two hemispheres, appears to have been satisfactorily accounted for by attributing it to the earth's rotation; while it has never been, with much plausibility, traced to any other cause or combination of causes.

Hereford, Feb. 17

W. Clement Ley

Zoological Nomenclature

In the President's address to the Entomological Society of London recently given by Mr. Wallace, one of the points most fully discussed is the rules of zoological nomenclature. These rules are undoubtedly of very considerable, though indirect, importance to science, and it is not very satisfactory to find that great divergence of opinion as to what these rules are, or should be, still prevails amongst recent describers and cataloguers.

Some years ago I was entrusted by the Entomological Society with the task of preparing a synonymical catalogue of the Coleoptera of our islands, to be published under the auspices of the Society; my attention, therefore, has necessarily been directed to the questions under discussion in this matter, and I will here state the conclusions to which I have come.

1st. That a committee to frame and publish laws on zoological nomenclature is not to be desired. Such committee would have no power whatever to enforce the laws it might make, and could not be expected to put an end to discussion on these points. The knot must be untied, not cut.

2nd. That the binomial system of nomenclature should not be arbitrarily considered to have commenced at any given date; but that recognisable names in all works in which this system is methodically employed should be used according to the rule of priority.

3rd. That it is not necessary to suppress a generic name in zoology because it has been previously used in botany (or *vice versa*); but that it is much to be regretted that any generic name should thus be in double use, and it should always be made matter of reproach to an author that he has committed an act of this nature.

4th. That names must be Latin to the extent that renders them capable of being written or used in scientific Latin; but that classical emendations beyond this are entirely inadmissible; no line except this can be drawn between emendation, alteration, and total suppression. The laws of classical languages have, *per se*, no more right over scientific nomenclature than has the Hindoo language. As regards the much talked-of "Amphionycha knownothing," it should be latinised in the simplest manner, as *Amphionycha knownothinga*; and I would further suggest that its barbarian author be well hissed whenever he ventures to show his face in a scientific assembly.

5th. That as regards placing an author's name after a species, the name so placed should always be that of the first describer of the species; not because he has any right in the matter, but as an additional means of certainty, and as a security against change.

6th. That the specific name is the name of an object, and therefore a noun, and should be changed in gender, or any other manner, when removed from one genus to another.

7th. That it is very undesirable to use the same specific name in two closely-allied genera; but that where this has been done already no alteration should be made till the two names actually come into collision on account of the two genera being united as one genus. Surely to act otherwise is like cutting one's throat for fear somebody else should do it.

8th. That as regards placing an author's name after a genus, the name so placed should be that of the author who established the genus in the sense in which it is actually used. *Carabus* of Linnæus included all the insects now comprised in the family *Carabidæ*, at present divided into several hundreds of genera. To write, therefore, *Carabus* Linn., when we mean something entirely different, may be usual but is not desirable.

I may add, that I consider it useless to expect a perfectly stable zoological nomenclature, until zoology itself is complete and perfect; but that in order to reduce changes to a minimum, classical and other secondary claims must not be allowed any great importance.

D. SHARP

Thornhill, Dumfriesshir

Deep-Sea Soundings

IN reference to the very interesting article in NATURE for February 22, "American Deep-Sea Soundings," may I be permitted to make the following remarks:—It is there stated that the water-collecting cylinder is apt to lead to incorrect conclusions in regard to the gaseous ingredients of sea water obtained by its means from great depths, owing to the escape of a portion of the gases when the pressure is relieved by the cylinder being drawn to the surface. As a member of the *Porcupine* expeditions of 1869 and 1870, I had nearly eight weeks' constant daily experience in the examination of samples of abyssal water thus obtained, and I believe that I was the first to adapt the gas analysis apparatus of the late Prof. W. A. Miller to the exigencies of a laboratory on board ship. The general result of these experiments for 1869 will be found as an appendix in No. 121 of the Proceedings of the Royal Society. My object in writing now is to point out that if there were such an escape of gaseous ingredients as is indicated above, the abyssal water would be so saturated with them at the ordinary atmospheric pressure (*i.e.* after the sample was removed from the water cylinder in the laboratory), that the least elevation of temperature would be sufficient to cause a further quantity to be given off. This, however, never was the case, since I invariably noticed that there was no appearance of bubbles of gas, until the water had been heated above 120° Fahr., and frequently still hotter. I may add that the only samples of water which appeared saturated with gaseous ingredients were those taken at the surface, afte several hours of strong wind. I must confess that after giving a good deal of thought to the subject, and conversing with friends whose knowledge of physics is far greater than mine, who agree with my view of the matter, I am unable to see any reason why we should expect to find any greater quantity of gaseous ingredients in abyssal than in surface water. No doubt, if the excess were there the enormous pressure would retain it, but where is the source of the supply of the supposed excess? I have never seen a satisfactory answer to this question. The solvent is exposed to excessive pressure, but the gases to be dissolved in it are not, unless there is any evolution of gas at those depths. It is probable that this abyssal water was at some point in its circulation near the surface, when an interchange would take place between some of its dissolved carbonic acid and the oxygen of the atmosphere. And it appears to me that it is only when the particles of sea water are near the surface, and exposed to no excess of pressure, that they dissolve their gaseous ingredients, which are afterwards modified in their composition by the animal life on the sea bottom.

WILLIAM LANT CARPENTER

Clifton, Bristol, February 26

Snow at the Mouth of a Fiery Furnace

IT would be interesting to ascertain the temperature of the saltatory drops noticed by Mr. H. W. Preece. Sudden and excessive evaporation may have produced actual congelation.

HENRY H. HIGGINS

ON THE SPECTRUM OF THE ATMOSPHERE

DURING the voyage out to India of the Eclipse Expedition, I took every opportunity of observing carefully the spectrum given at sunrise, compared with that at sun-high, and obtained the following results, which, though poor in themselves, will show the wide field open for further research.

When leaving England, and for some way into the Mediterranean, the length of the spectrum as seen at sunrise extended generally from about B in the red to near G in the violet. Great differences were, however, presented in the absorption-lines according to the state of the weather, or perhaps rather according to the state of the sky when the sun rose.

If the sun rose among yellow tinted clouds, the absorption bands about B, C, between C and D, and near D, were exceedingly well defined; at the same time the blue end did not extend so far as usual, showing that there was more absorption of the blue, while probably the greater quantity of aqueous vapour in the air reflected the red and yellow rays. In these cases the tint of the clouds generally changed to a rosy red shortly after sunrise.

A clear sunrise, on the contrary, showed an extension of the violet end, whilst the aqueous bands at B, C, and D were less defined, as if the red and yellow light were not so strong to show them out by contrast.

On passing through the Suez Canal and down the Red Sea the spectrum was shortened at both ends, leaving from little beyond C to a third from F to G; this would seem to show a general absorption going on in the atmosphere from some cause, probably light dust in the air. This idea is strengthened by the beautiful purple colour of the distant mountains, as if, though the violet rays were greatly absorbed, the red rays were so to a less degree, whilst the want of aqueous vapour allowed nearly all the yellow rays to be transmitted.

When clear of the Red Sea in the Indian Ocean, the blue became greatly reduced, and the red end extended to A; the aqueous bands were very strong indeed, so much so that on two mornings D_1 and D_2 could hardly be distinguished amid the black mass that surrounded them; the lines near C and C' or γ of Brewster were sharp and clear.

On nearing India another change took place; the blue continued to be absorbed, till at sunrise the spectrum could hardly be seen beyond F, but the blue green became very bright, and the dark bands between *b* and F very distinct, the lines commencing at 1825 Kirchhoff especially attracted notice, standing out sharp and distinct, so as at first to be mistaken for F: those nearer F at 1890 K showed as a clear broad band, but not nearly so black as 1825. I am not prepared to give an explanation of this phenomenon, but will remark that when the sun rose clear and free from clouds the aqueous bands to D were less distinct, while the atmospheric bands from D to E were clear and sharp, and those beyond *b* remarkably so. But if the sun rose among clouds, these were generally tinted with a golden yellow, changing afterwards to a rose or red colour, and, as might be expected, the lines from B to D and just beyond D_1 were well defined, whilst from E to near F the spectrum was not so clear.

After this the duties of preparing the instruments for the eclipse prevented my taking any observations, as most of our work was done in the early morning. But after the eclipse, whilst on the Neilgherry Hills, 6,000 feet above the sea, I had an opportunity of finding that the strong line at 1825 had nearly faded away. The weather was then fine, but misty. A few days after, on going down the Ghauts to Bombay, I was struck with the blue colour of the mist that was hanging about the valleys, and I examined it with the spectroscope; the blue extended much farther than usual, and the lines between *b* and F were again distinct.

On the passage home the same results were obtained as on going out; but as I had a much smaller spectroscope I could not make the observations with the same accuracy as before. When passing up the Red Sea the absorption was evident at both ends of the spectrum, and the mountains were of the same beautiful purple colour that I had noticed before.

From Alexandria to Southampton we had very bad weather, constant gales, making it difficult to observe. But I got the following results: With a cloudy sky at sunrise, and appearance of wet weather, the bands from B to beyond D (δ of Brewster) were strong, whilst the blue end of the spectrum was greatly absorbed, and the lines from *b* to F were less distinct; this was reversed with clear weather. As we gained higher latitudes, the blue end of the spectrum lengthened out, and the bands beyond F, particularly about 2330 K, became distinct, while the bands 1825 K and 1890 K gradually faded, and now their intensity is not one-fourth of what I observed it in the Indian Ocean.

These observations are very imperfect, but I hope, if I can get the instruments, to carry out a more perfect system of observation, feeling sure that it is a subject worthy of great consideration in meteorology, especially when taken in connection with the temperature and pressure of the atmosphere and the state of the weather.

Shanklin, Feb. 5 J. P. MACLEAR

PROF. AGASSIZ'S EXPEDITION

IT is probable that I may have been anticipated, as regards part of the present communication. If not, I believe that many of your readers will be glad to learn the objects with which Prof. Agassiz has started, with Count Pourtales and a distinguished band of skilled observers, on a scientific expedition in the United States' surveying ship *Hassler*, and to receive a brief account of what he has already done at St. Thomas and Barbados, at which places he was obliged to touch, in consequence of defects in the vessel or her machinery.

The Professor's chief objects are stated in a letter from himself to Prof. Peirce, the Superintendent of the U.S. Coast Survey. (See NATURE, vol. V., p. 194.)

The Expedition was detained some days at St. Thomas, and the time of the Professor and his assistants was devoted chiefly to the collection and preparation of fishes, with a view to the study of the brain, and the breathing and digestive organs. Several boxes full, preserved in alcohol, were at once shipped to the United States, as the first-fruits of the Expedition.

The party arrived at Barbados on December 26, and spent four days there. The first two were devoted by the Professor to examining and studying the large collection of West Indian shells, marine and terrestrial, of corals, sponges, crustacea, and semi-fossil shells of the island, made by the Governor, Mr. Rawson. Of the marine series he wrote in the following terms to Mr. J. G. Anthony, the Curator of the Harvard Museum:—"I am having high carnival. I have found here what I did not expect to find anywhere in the world—a collection of shells in which the young are put up with as much care as the adult, and extensive series of specimens show the whole range of changes of the species, from the formation of the nucleus to the adult." He was particularly struck with the now unique specimen of *Holopus*, lately procured by Mr. Rawson, which was described by Dr. J. E. Gray in the December number of the "Annals of Natural History," and named by him, from a drawing, *H. Rawsoni*, but which Agassiz, who had seen the specimen of D'Orbigny in Paris, before it disappeared, considers to be a normal specimen of *H. Ranzii*, which had only four, instead of five arms. Count Pourtales recognised among the corals several similar to those which he had obtained by dredging in or near the Gulf Stream, and described in the latest No. (4) of the "Illustrated Catalogue of the Museum of Comparative Zoology at Harvard College," the presence of which on the coast of Barbados serves to indicate the close similarity of submarine life in those two distant localities.

The next two days, or rather the night of the next, and the greater part of the following day, were spent in dredging in the neighbourhood, in a depth of 60 to 120 fathoms, about a mile from the shore, whence Mr. Rawson has procured his fine specimens of *Pentacrinus Mülleri*. The *Holopus* was found on the opposite side of the island. The results were beyond the expectations, or even the hopes, of the most sanguine of the party. Only dead fragments of the *Pentacrinus* were obtained, but among the abundant spoils were four specimens of a new genus of Crinoid, without arms on the stem, (like *Rhizocrinus?*) which remained alive, with the arms in motion, until noon on the following day, under the excited observation of the party. A number of deep-sea corals, alive, crustacea, sea urchins of new species, star fish, sponges, crystalline, jurassic, and corallines, &c., and a rich harvest of shells, were obtained. Among these was a splendid live specimen of *Pleurotomaria Quoyana*, F and B, of which genus Chenu writes that only one living species, and of that only one specimen, is known. The animal exhibited remarkable affinities, and the artist accompanying the expedition was able to take several sketches of it. A large *Oniscia*, shaped like *O. cancellata* Sow. but with an orange inner lip (*O. Dennisoni?*), some specimens of *Phorus Indicus* Gmel., a magnificent new species of *Latiaxis*, with many exquisite specimens of *Pleurotoma*, *Fusus*, *Murex*, *Scalaria*, and three or four of *Pedicularia sicula* Sw., with innumerable Pteropods and Terebratulinæ, rewarded these "burglars of the deep." The Professor was delighted, and it was with reluctance he abandoned so rich a field in order to secure his passing through the Straits of Magellan at a right season.

Barbados, January 26 R. W. R.

ETHNOLOGY AND SPIRITUALISM

THE *Academy* of February 15 contains a review by Mr. A. R. Wallace, of my "Primitive Culture," where he raises a point on which I wish to make some further observations; but inasmuch as the form of publication of that journal adapts it rather to criticism than to correspondence, I ask leave to change the *venue*, and make my remarks in the columns of NATURE.

In "Primitive Culture" (Vol. i., pp. 279-84), I have given an account of the widespread popular belief in "were-wolves," including under this heading the analogous belief in man-hyænas, man-tigers, &c. According to this superstition, certain human beings are considered to be temporarily transformed into wolves, hyænas, or tigers, and in these shapes to go about preying on mankind. While expressing an opinion that "the origin of this idea is by no means sufficiently explained," I have offered two suggestions as bearing on its prevalence in the world: first, that such notions are consistent with the familiar doctrines of the lower culture as to transmigration of souls and transformation of bodies; second, that certain insane persons do actually suffer under the delusion that this transformation (the idea of which popular belief has put into their minds) has really happened to themselves, and they prowl about like wild beasts accordingly. Mr. Wallace disapproves of this treatment of the subject, and propounds a view of his own, as follows: "A recognition of the now well-established phenomena of mesmerism would have enabled Mr. Tylor to give a far more rational explanation of were-wolves and analogous beliefs than he offers us. Were-wolves were probably men who had exceptional power of acting upon certain sensitive individuals, and could make them, when so acted upon, believe they saw what the mesmeriser pleased; and who used this power for bad purposes. This will explain most of the alleged facts, without resorting to the short and easy method of rejecting them as the results of mere morbid imagination and gross credulity."

Let me now first observe that Mr. Wallace's explanation does not supersede my suggestions; indeed, he meets neither of the points which I endeavour, however tentatively, to deal with. He offers nothing like a reason why knavish sorcerers in districts of Europe, Asia, Africa, and America should have all hit upon the device of imposing the same peculiar delusion upon their dupes; nor does he account for the fact, vouched for by satisfactory evidence, that in certain cases the supposed were-wolf is himself utterly persuaded of the reality of his own transformation, and goes to execution believing in his offence. The proofs are, I think, convincing, here as elsewhere in the history of magic, that sorcerers were originally and still are usually more or less believers in their own magical pretensions—though very many used and use fraudulent means to enhance their supposed powers; and some, who may be reckoned among the vilest of the human race, are simply professional impostors. Yet Mr. Wallace's suggestion, though it does not do away with the need of mine, seems to me valuable as a well-directed attempt to explain a part of the matter left untouched by me. His theory that a were-wolf may be a person possessed of the peculiar faculty exerted by mesmerists, of making others delusively imagine that they see and hear what in fact does not happen, is a theory at any rate plausible, and possibly on the track of explaining much of the power belonging to sorcerers, savage and other. (I may remark incidentally that the power of mesmerists in producing anæsthesia and working on the imagination of their patients has never been contradicted by me.) Now, without committing myself to Mr. Wallace's idea, beyond saying that it is plausible and worth pursuing, I proceed to apply it somewhat farther. Granting that a were-wolf, in virtue of being a person capable of exerting mesmeric influence, can delude people, and even assemblies of people, into fancying that they perceive monstrous unrealities, the question arises, Was any one with this were-wolf-faculty present in the room when Mrs. Guppy made her celebrated aërostatic entrance? Is Mr. D. D. Home a were-wolf? Is a professional "medium" usually or ever a person who has the power of acting on the minds of sensitive spectators, so as to make them believe they see what he pleases? Pursuing this subject yet a step farther, I have now to call Mr. Wallace's attention to an interesting fact. The sorcerers of the Abipones of South America, who by mere roaring within their tents threw the credulous savages into agonies of panic terror, caused by vivid belief that tiger-spots were in the act of coming on their (the sorcerers') bodies, that their nails were growing into claws, that they were actually transforming themselves into tigers, deadly though invisible—these sorcerers were actually the professional spiritualistic mediums of the tribe, part of whose business it was to hold intercourse with the spirits of the dead, causing them to appear visibly, or carrying on audible dialogues with them behind a curtain. Mr. Wallace, as the most eminent scientific man who has taken up what are known as modern "spiritualistic doctrines," no doubt has the ear of all who hold these doctrines. I think it may bring about investigations leading to valuable results if Mr. Wallace will inform spiritualists with the weight of his authority that he believes in the existence of a class of men who, in his words, have exceptional power of acting upon certain sensitive individuals, and can make them, when so acted upon, believe they see what the mesmeriser pleases, and who use this power for bad purposes.

With reference to other parts of Mr. Wallace's review of my work, I have to thank him for several valuable comments, while, at the same time, I venture to express an opinion that some of his objections to my ethnological treatment of spiritualism are unreasonable, and especially I wonder that so serious a student of natural science should make it a ground of complaint against me that in treating of difficult and important problems I consider it necessary to bring forward copious and widely distributed evidence. But rejoinders to reviews are seldom desirable in themselves, and my justification for the present note lies in the importance of drawing attention to a matter worth considering by persons on both sides of the spiritualistic controversy. E. B. TYLOR

DREDGING EXPEDITIONS

THE occasion of an American Dredging Expedition recently starting, leads us to make the following remarks on such Expeditions in general, more especially upon one whose programme has lately come to our ears.

England has perhaps of all countries done the most for dredging. We have only to point to such names as Forbes, Ball, McAndrew, Wallich, Jeffreys, Wyville Thomson, and Carpenter, as among the landmarks in the cause. Indeed, for many years coast dredging has been a popular amusement with the marine naturalist and collector, and many a prize has been in this manner turned up.

In 1868 Messrs. Carpenter, Thomson, and Jeffreys were fortunate enough to obtain the use, free of expense, of a Government steamer, and, armed with a substantial grant from the Royal Society, tried their luck in the deep sea. The following year the Government again gave them the use of a vessel, and the Royal Society a further grant of 200*l*. Again in 1870 they went out at the country's expense. The great and important results obtained during these cruises are pretty well known to the scientific world, and it is unnecessary to repeat them here.

In the year last mentioned an unheard-of circumstance took place. An English yachtsman, Mr. Marshall

Hall, not only gave up the use of his yacht for the summer in the cause of Science, but bore nearly the whole expense of the cruise himself. The naturalist who accompanied them was Mr. Kent, of the British Museum, a man comparatively unknown before that time; and this was, perhaps, the reason why the Royal Society could only afford to give £50 towards the expense of apparatus, &c. As a natural consequence, the expedition was considerably crippled for want of proper gear, and they were unable to attempt deep-sea work. It is too rare for persons who are blessed with means to assist Science in any way, and when such an act of generosity does take place, it ought not to be forgotten on the part of the scientific public. Yet it is rumoured that a similar expedition to Morocco and Madeira, which Mr. Marshall Hall is arranging for the spring, is likely to be received with some coldness by some influential members of the scientific brotherhood. We sincerely hope that the rumour is incorrect.

It appears that Mr. Marshall Hall proposes to be absent from England for between three and four months; and, besides the natural history, to investigate, as far as possible, certain chemical and physical questions concerning the deep sea and its currents in the neighbourhood of the above-mentioned places. He is taking with him a young naturalist, Mr. P. T. Abraham, B.A., B.Sc., lately from Dublin, at which University he came out first in natural science honours, and where he has gained a high reputation for zoological knowledge. It is also probable that another naturalist will make up the staff. These gentlemen intend to give, besides the use of the yacht, 150*l*. or so—as much as they are able. The remaining 250*l*.—for the total cost of the expedition could not amount to much less than 400*l*., when the items of gear, apparatus, outfit, and maintenance for such a time are taken into consideration—they hope to obtain in the form of grants from the learned societies. We feel sure that the Royal Society will be among the first to endow the work out of the fund placed at their disposal by the Government, and the best friends of Biology may wish that they had more frequent opportunities afforded them of assisting in researches in which it is fitting that in the first instance a private individual should come forward.

It is possible even that other societies may be induced to help if they have funds at their disposal. Among such societies we may mention the Zoological Society, which contains on its roll the names of men of the first rank in every department of zoology. It is true that a great portion of the funds are expended in the direction of the higher vertebrates, and that the lower animals do not receive the attention they may deserve; but still, it must be remembered that the great object of the society is the popularisation of natural history.

We hope that the *Norna's* will not be the only dredging excursion starting from British waters this year. The field that has been so ably opened up by Dr. Carpenter and his colleagues ought not to be allowed to slip away altogether from the hands of Englishmen. We know too well that other nations are not backward in following up and eclipsing the work that British pluck and genius have been the first to venture upon. The Americans are on the track, and our Continental neighbours will not be far behind.

We are glad that the extended circumnavigation expedition is in process, and we believe that if nothing unforseen occurs, Prof. Wyville Thomson, with a staff of competent aids, will sail in the autumn on their long journey, which cannot fail to have the most important bearing on our future advance in such studies. Such a journey as this, however, instead of making more modest dredging operations of no avail, vastly increases their importance; and it is not too much to hope that the time is not far distant when men of money and leisure will more generally occupy their time in such pursuits.

SOLAR HEAT

THE calculations presented by Père Secchi, in his work "Le Soleil," relative to solar temperature and solar radiation, tending to discredit the result of recent investigations on the subject, I have carefully examined the "solar intensity apparatus," the indications of which form the basis of those calculations. This unique device will be found delineated on p. 267 of the work referred to, the accompanying illustration (Fig. 1) being a fac-simile of the same. It represents a longitudinal section through the centre-line, thus described:—A B and C D are two concentric cylinders soldered one to the other; they form a kind of boiler, the annular space being filled with water or oil at any temperature. A thermometer, *t*, passes through a tube, across the annular space, to the axis of the cylinder; it receives the solar rays introduced through a diaphragm, *m n*, the opening, *o*, of which is very little larger than the bulb of the thermometer. A thick glass, V, closes the back part of the instrument, and admits of ascertaining whether the thermometer is placed in a direct line with the pencil of rays. The interior cylinder and the thermometer *t* are coated with lamp black. A second thermometer, *t'*, shows the temperature of the annular space, and consequently that of the inclosure. The whole apparatus is mounted on a support having a parallactic movement, to facilitate following the diurnal motion of the sun. The apparatus being exposed to the sun, it will be found, on observing the two thermometers, that their difference of temperature increases gradually, and that in a short time it ends by being constant.

Before pointing out the peculiarities of the contrivance thus described by Père Secchi, it will be instructive to examine his "solar intensity apparatus," manufactured by Casella, represented in Fig. 2. The manufacturer publishes the following statement regarding this instrument:—"Two thermometers are here kept immersed in a fluid at any temperature, and a third surrounded by the same conditions, but not immersed, is exposed to the rays of the sun. The increase of temperature thus obtained is found to be the same, irrespective of the temperature of the fluid which surrounds it." No one acquainted with the principles which govern the transmission of heat within circulating fluids can fail to observe that the thermometers applied above the central tube will not furnish a reliable indication of the temperature of the fluid below the same, nor of any portion of the contents of the annular space towards the bottom. Apart from this defect, it will be perceived that an upward current of atmospheric air will sweep the underside of the external cylinder, causing a reduction of temperature of the fluid confined in the lower half of the annular space. Again, the heat radiated by the bulb of the thermometer exposed to the sun will elevate the temperature of the air within the central tube, and consequently produce an internal circulation tending to heat the upper part of the fluid contained in the annular space. The effect of the irregular heating and cooling thus adverted to will be considered after an examination of the result of some observations recorded in Table A conducted at different times during the month of September 1871. In order to insure an accurate position, the instrument during these observations was mounted in a revolving observatory upon a table turning on declination axes provided with appropriate mechanism and declination circle. An actinometer being attached to the same table, the true intensity of the radiant heat, as well as the sun's zenith distance, were recorded simultaneously with the indications of the Secchi instrument furnished by Casella. Let us first consider the tabulated observations of September 2 recorded at equal intervals of three minutes. The indication of the two thermometers immersed in the fluid contained in the annular space first claims our attention, since the temperature of this fluid is the principal element in Père Secchi's computations of solar temperature. It will be seen on referring to the second and third columns of the table that, while the upper thermometer indicates a mean temperature of 86·9,° the lower one shows only 79·5°, difference = 7·4°. This great discrepancy of temperature at different points of the upper portions of the annular space at which, owing to the inclined position of the concentric tubes, something like uniformity ought to exist, suggests a still greater discrepancy of temperature at the underside towards the lower termination of the tubes. In addition therefore to the observed irregularity of temperature at the upper part, shown by the table, no indication whatever is furnished of the temperature of the fluid in the annular space below the central tube, nor towards the termination at either side. Obviously, then, no accurate computation can be made of the degree of refrigeration to which the central thermometer is exposed by the radiation from the cold blackened surface of the internal tube, every part of which, as we have seen, possesses a different temperature compared with the rest, consequently transmitting radiant energy of different intensity. It will be found practically impossible, therefore, to determine the true differential temperature of the contents of the bulb exposed to the sun's rays and the fluid contained in the annular space. Hence, the differential temperature entered in the table, the result of comparing the indications of the thermometers, is manifestly incorrect. It will be found also by reference to the table that while the mean temperature imparted to the central thermometer by the sun's rays is 93·1°, the mean temperature of the fluid in the annular space is 83·3°. Consequently, the intensity of solar radiation established by the instrument is only 93·1° – 83·3° = 9·79° Fah. Now, the sun during the recorded experiment of September 2 was exceptionally clear, the mean indication of the actinometer while the experiment lasted being 60·05°, thus showing that the energy developed was only $\frac{9·79}{60·05}$ = 0·16 of the true radiant intensity. The mean zenith distance, it may be mentioned, was only 33° 24′ during the experiment. Agreeable to the table of temperatures previously published, the maximum solar intensity for the stated zenith distance is 63·35°; thus we find that the sun, as stated, was exceptionally clear while the trial took place, which resulted in developing the trifling intensity of 9·79° Fah. The result of the experiments conducted September 6th, recorded in the table, it will be seen was nearly the same as that just related, the mean temperature indicated by the thermometer exposed to the sun being 98·2°, while the mean of the two thermometers immersed in the fluid was 87·8°, hence the differential temperature 98·2° – 87·8° = 10·4°. The mean temperature of solar radiation during the experiment, ascertained by the actinometer, was 59·75°, the zenith distance being 35° 33′. Consequently, the intensity indicated September 6th was only $\frac{10·45}{59·75}$ = 0·17 of the true energy of the sun's radiant heat, against 0·16 during the previous experiment. It will be observed that the fluctuation of the differential temperature was much greater September 2nd than during the succeeding experiment, owing, no doubt, to the influence of currents of air produced by a strong breeze on the first occasion, the revolving observatory being partially open on the side presented to the sun during observations.

With reference to the small differential temperature indicated by the Secchi instrument manufactured by Casella, it may be urged that it is not intended to show the true intensity of solar radiation on the earth's surface, but simply a means of determining solar temperature. Granted that such is the object, yet the extreme irregularity of the temperature of the fluid within the annular space shows that the instrument is unreliable, a fact established beyond contradiction by an experiment instituted September 27, 1871. On this occasion water of a uniform temperature was circulated through the annular space. This was effected by gradually charging this space from the top, and carrying off the waste at the bottom, holes having been drilled in the external casing for that purpose. The result of this conclusive experiment is recorded at the foot of Table A. It will be found on reference to the figures, that the mean difference of the two thermometers immersed in the fluid was only 64·9° – 64·4 = 0·5°, while the mean differential temperature was augmented to 79·1° – 64·45 = 14·65° against 9·79° on the 2nd of September, although the zenith distance was greater, and the solar intensity less; circumstances which ought to have *diminished* the indicated intensity. It is needless to enter into any further discussion of the demerits of the instrument represented in Fig. 2. We may now return to the consideration of the device delineated in Fig. 1, copied from "Le Soleil." It will be seen that the material difference of construction is that of applying only one thermometer for ascertaining the temperature of the fluid in the annular space. Possibly this single thermometer may indicate approximately the mean temperature of the upper and lower portions of the fluid above the central tube; but it furnishes no indication of the temperature below, nor at either extremity of the annular space. The inadequacy of the means adopted for ascertaining the temperature of the internal surface which radiates towards the bulb of the central thermometer having thus been pointed out, it will be well to consider whether the expedient of passing a stream of water of nearly uniform temperature through the annular space, will insure trustworthy indication. In order to determine this question, I have constructed two instruments, in strict accordance with the delineation in Fig. 1, excepting that in one of these the concentric cylinders are considerably enlarged, the annular space, however, remaining unchanged. Experiments with the two instruments prove that the enlargement does not materially influence the indications, provided water of a uniform temperature be circulated through the annular space. But these experiments have demonstrated that the size of the bulb of the thermometer exposed to the sun cannot be changed without influencing the differential temperature most materially. This will be seen by reference to Table B, which records the result of experiments with different thermometers, and tubes of different diameter, conducted October 17, 1871. As on previous occasions, the instruments, in order to insure accurate position, were attached to the declination table arranged within the revolving observatory. The bulbs of the thermometers employed were very nearly spherical, their diameters being respectively 0·30 and 0·58 ins. The upper division of Table B which records the experiment with the *small* bulb exposed to the sun, establishes, it will be seen, a differential temperature of 14·4° for the instrument having the 1¼-in. central tube, and 16° for the one having the 3-in. central tube. Referring to the lower division of the same table, it will be seen that when the thermometer with the *large* bulb is exposed to the sun, the differential temperature reaches 22·5° in the instrument containing the 1¼in. central tube, and 21·1° in the one having the 3in. tube. We thus find that, by doubling the diameter of the bulb of the thermometer exposed to the sun, all other things remaining unchanged, an augmentation of the differential temperature amounting to nearly one-third takes place. This fact proves the existence of inherent defects fatal to the device delineated in Fig. 1, rendering the same wholly unreliable.

Agreeably to the doctrine of exchanges, the diameter of the bulb is an element of no moment, since the internal radiation towards the same—*provided its temperature be uniform*—depends solely on the temperature and angular distances of the radiating points of the enclosure. Infallibility of the "solar intensity apparatus" has evidently been taken for granted on the strength of the soundness of this doctrine, as we find no allusion to the size of the bulb in M. Soret's account of his observations of solar intensity on Mont Blanc; nor does Mr. Waterston, who employed a similar instrument during his observations in India, advert to the dimensions of the bulb of the thermometer exposed to the sun. These physicists apparently overlook the fact that, while the entire convex area of the bulb is exposed to what may be considered the cold radiation from the enclosure, only one half receives radiant heat from the sun. This circumstance would be unimportant if the heat thus received were instantly transmitted to every part; but the bulb and its contents are slow conductors, while the conducting power diminishes nearly in the inverse ratio of the square of the depth. Consequently, by increasing the diameter, the parts of the bulb opposite to the sun will receive considerably less heat in a given time than if the diameter be diminished.

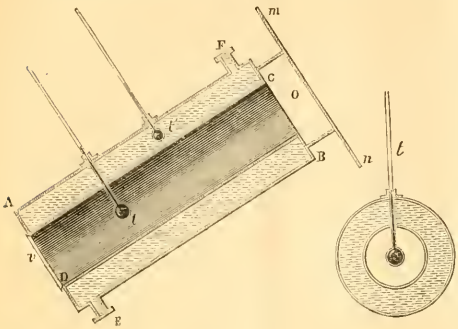


FIG. 1.

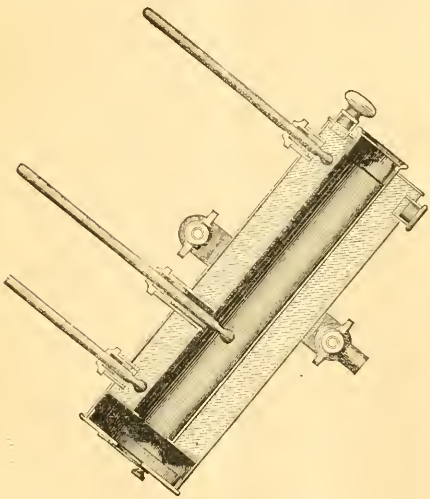


FIG. 2.

TABLE A, showing the result of observations made with Secchi's "Solar Intensity Apparatus," manufactured by Casella.

Thermometer exposed to the Sun.	External Casing. Upper Thermometer.	External Casing. Lower Thermometer.	External Casing. Mean.	Differential temperature.	Zenith distance.
		SEPTEMBER 2, 1871.			
Fah.	Fah.	Fah.	Fah.	Fah.	° ′
83·5	76·0	70·0	73·0	10·5	33 0
84·2	77·0	71·5	74·2	10·0	
85·5	79·0	74·2	76·6	8·8	32 50
86·0	83·5	74·5	79·0	7·0	
89·0	84·0	75·5	79·7	9·2	33 0
90·5	85·0	76·5	80·7	9·2	
92·0	85·5	78·0	81·7	10·2	33 10
93·0	86·5	79·0	82·7	10·2	
94·0	87·8	80·0	83·9	10·1	33 21
94·5	89·0	81·5	85·2	9·2	
95·5	90·0	82·5	86·2	9·2	33 32
96·5	90·5	83·5	87·0	9·5	
98·0	91·5	84·5	88·0	10·0	33 44
99·0	92·0	85·0	88·5	10·5	
100·0	93·0	86·0	89·5	10·5	33 56
101·0	93·5	86·5	90·0	11·0	
101·5	94·0	87·0	90·5	11·0	34 8
93·1	86·9	79·5	83·3	9·79	33 24
		SEPTEMBER 6, 1871.			
94·5	88·0	81·5	84·7	9·7	35 56
95·5	88·5	83·0	85·7	9·7	
96·5	89·5	84·5	87·0	9·5	35 41
97·5	90·0	85·0	87·5	10·0	
98·0	90·0	85·0	87·5	10·5	35 26
98·5	90·5	85·5	88·0	10·5	
99·0	90·5	85·7	88·1	10·9	35 11
100·0	91·0	86·5	88·7	11·2	
100·3	91·0	87·0	89·0	11·3	34 56
100·3	91·2	87·5	89·3	11·0	
100·5	91·5	88·0	89·7	10·8	34 41
98·2	90·2	85·3	87·8	10 45	35 33
		SEPTEMBER 27, 1871.			
78·5	64·0	64·0	64·0	14·5	44 0
79·0	65·0	64·0	64·5	14·5	
79·5	65 0	64·5	64·7	14·7	44 55
79·5	63·0	65·0	64·0	15·5	
79·5	64·0	65·0	64·5	15·0	45 51
79·0	64·5	65·0	64·7	14·2	
79·0	64·5	65·5	65·0	14·0	46 48
79·0	64·5	65·5	65·0	14·0	
79·0	65·0	65·5	65·2	13·8	47 46
79·1	64·4	64·9	64·65	14·45	45 16

TABLE B, showing the result of employing different thermometers.

Diameter of Bulb 0·30 in.

1¼ inch tube. Sun.	1¼ inch tube. Fluid.	1¼ inch tube. Diff.	Zenith distance.	3 inch tube. Sun.	3 inch tube. Fluid.	3 inch tube. Diff.	Zenith distance
Fah.	Fah.	Fah.	° ′	Fah.	Fah.	Fah.	° ′
74	60	14	50·32	77·5	62 1	15·4	49 54
74·5	60·3	14·2	50·24	78·5	62·3	16·2	50 3
75	60·7	14·3	50·16	79	62·5	16·5	50 12
75·5	61	14·4	50·8	79	63	16	50 21
76	61	15	50·1	79	63	16	50 30
75·0	60·6	14·4	50·16	78·6	62·6	16·0	50 12

Diameter of Bulb 0·58 in.

1¼ inch tube.			Zenith distance.	3 inch tube.			Zenith distance
Sun.	Fluid.	Diff.		Sun.	Fluid.	Diff.	
Fah.	Fah.	Fah.	° ′	Fah.	Fah.	Fah.	° ′
83·6	62·6	21	49 54	79·2	60·1	19·1	50 32
85·5	63	22·5	50 3	81	60·3	20·7	50 24
86·4	63·4	23	50 12	82·5	60·7	21·8	50 16
86·7	63·5	23·2	50 21	82·7	60·7	22	50 8
87·7	63·7	23	50 30	83	61	22	50 1
85·9	63·2	22·5	50 12	81·7	60·6	21·1	50 16

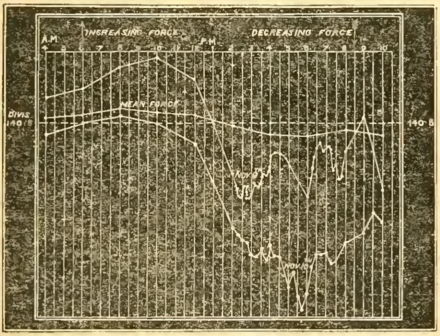
J. ERICSSON

MAGNETICAL AND METEOROLOGICAL OBSERVATIONS AT HAVANA

On the 9th and 10th day of November I noticed on my instruments two strong magnetic perturbations, during which a series of extraordinary observations was taken at intervals of five, of ten, and fifteen minutes. From these I was naturally drawn to think that an aurora borealis would be seen in higher latitudes, and was waiting for a confirmation of my views.

This I found in the numbers 16th and 23rd of November of your scientific journal, NATURE, which I have just received, and in which I see with great pleasure the description of the aurora borealis seen in England on the 9th and 10th of November in perfect accordance with my observations of those days.

Curves of the Horizontal Magnetic Force on the 9th and 10th days of November, 1871, compared with the Mean Force of the whole month.



SCALE { 1 *hour* = 0^m ·01 *in the line of the abscissæ*
5 *division of the scale of the Bifilar Magnetometer* = 0^m ·01 *in the line of the ordinates.*

Each one of these divisions of the scale corresponds in parts of horizontal force to K = 0·000099573.

As it will not be devoid of interest to know to what an extent an aurora borealis, when seen in England, exerts its influence on the magnetic variations of a place situated in the Tropics and in very remote longitude, I take the liberty of sending you the curves of the horizontal magnetic force as registered by the bifilar magnetometer on the 9th and 10th of November, together with the curve of the mean horizontal force of the whole month. A comparison between them and those taken in other places will be, I hope, very pleasant to those who are interested in magnetic researches.

My observations on the bifilar magnetometer are reduced to the temperature of 77° Fah. The variation of the thermometer attached to it was 0°·8 during the whole perturbation.

The magnetic instruments I make use of are those of the Observatory of Makerston, Scotland, which were arranged and sent many years ago to this Observatory by order of General Sabine at the request of P. Secchi, of the Roman Observatory.

Another perturbation, although not so intense as those already described, was observed on the 2nd of November. It began at ten o'clock in the morning, and lasted the whole day.

A very remarkable one was also observed on the 17th and 18th of June; it began at ten o'clock in the evening of the 17th.

On the 21st of August, while a hurricane was felt in St. Thomas, and an aurora borealis seen from the Observatory of Dun Echt, Aberdeen, I noticed an extraordinary variation, which attained its maximum between four and six o'clock in the afternoon. A similar one occurred on the 24th.

Finally, on the 16th and 17th of August two great hurricanes swept the shores of Florida, and their influence upon the magnetic force can be perfectly noticed on the curves of those days.

BENEDICT VINES

Havana, Dec. 21, 1871

NOTES

We alluded some time since to the threatened destruction of one of the most notable megalithic monuments in this country, the Great Circle at Avebury, in Wiltshire. All archæologists will be glad to hear that Sir John Lubbock has added one more to his eminent services to science by the purchase of the site on which the Circle stands. It is right also that the meed of praise should be awarded to those of the residents in the district whose zeal has been directed towards the attainment of this object, and who have thus shown their sense of the value of the monument which is one of the glories of their county. We refer especially to the Rev. Bryan King, the vicar of the parish, Mr. Kemm, Mr. George Brown, and the Rev. Alfred Charles Smith, Hon. Secretary of the Wiltshire Archæological and Natural History Society. It is to be hoped that their example will stimulate similar zeal for the preservation of monuments in other parts of the country.

DR. T. STERRY HUNT, chemist to the Canadian Geological Survey, has been appointed to the chair of Geology in the Massachusetts Institute of Technology.

MR. HENSMAN has been appointed Lecturer on Botany at the Middlesex Hospital, in the place of Dr. T. S. Cobbold, F.R.S., who has received the appointment of Lecturer on Parasitic Diseases.

AT the meeting of the Royal Geographical Society on Monday evening last, Sir Henry Rawlinson, the President, announced that the vessel with the Livingstone Expedition on board arrived at Malta on the 23rd inst., and was to reach Port Said on Sunday, and leave Suez on Monday night. By the accounts to hand all on board were pronounced to be well, and in the highest spirits. The finances of the expedition were in a highly satisfactory state, many contributions being remarkably striking, as showing the great interest taken in the enterprise not only in this, but in many distant countries. A contribution of 100 guineas had been received from a former member at Stockholm, who had always taken a deep interest in the travels and discoveries of Dr. Livingstone. The Italian Royal Geographical Society had also sent a contribution of 15*l.* 15*s.*, while national committees to assist the fund had been formed in Scotland and Ireland, who were working most energetically. The town of Glasgow has subscribed 1,000*l.*, Edinburgh 400*l.*, and Dublin promised to be equally generous. Similar interest had been awakened in Chicago, whence 100*l.* had come in to be placed at the disposal of the Livingstone Expedition; and on the whole it might be said the announcement of the undertaking had been hailed with general satisfaction throughout the civilised world. Exclusive of two sums of 400*l.* and 600*l.* odd, the latter the balance of the former Government grant, there was now standing to the credit of the expedition a sum of 4,200*l.*

THE following gentlemen were on Saturday last elected to Junior Studentships in Natural Science at Christchurch, Oxford:—Mr. D. A. Greswell, Commoner of Balliol College, Mr. B. Hainsworth, of Manchester Grammar School, Mr. W. A. Smith, of Clifton College. These scholarships are of the annual value of 75*l.*, together with the rooms rent free.

AT the examination recently concluded at the Melbourne University, there were no less than 225 competitors, of whom 86 passed the matriculation examination, and 108 the civil service examination. Many of the names in the former were included in the latter, but on the other hand, there were some who passed the larger, the matriculation examination, who did not pass the smaller examination, that for the civil service. The reason is, that for the matriculation any six subjects serve to qualify, while for the civil service, of the four subjects, two given ones are essential. The examinations this time had a novel feature, from there being three lady candidates, all of whom passed. The Council of the University, however, has passed a resolution to the effect that the successful ladies should not be allowed to matriculate. No reasons have been given for this decision, but it is presumed that the obstacle is a legal one.

THE *Academy* states that the President of the Geographical Society of Italy has written to the papers to say that the Conservator of the Bibliothèque Royale of Belgium has discovered a MS., in twelve chapters, containing the original autograph account of the discovery of Australia by Manuel Godinho, a Portuguese navigator, who touched there in 1601, and whose priority to the Dutch sailors, who arrived three or four years later, has been unduly neglected. Mr. Ruelens vouches for the authenticity of the MS., which was brought to light at the Antwerp Exhibition, though it passed unnoticed in the crowd.

PROF. CLEVELAND ABBE, in an article entitled "Historical Note on the Method of Least Squares," in the American Journal of Science and Arts, shows that this method, though first published in a printed form by Le Gendre in 1806, and invented by Gauss in 1795, was published in 1808 by Prof. Robert Adrain, at that time in New Brunswick, N. J., in the "Analyst," he having been independently led to this invention by the study of a prize problem offered some months previously in that periodical.

AN important addition has been made to the list of works devoted to inquiries and instructions in regard to the great fisheries in the form of a paper, by M. Achille Costa, upon the fisheries of the Gulf of Naples, published by the Royal Institute for the Encouragement of Natural Science, &c., of Naples. The subject is treated under four heads: first, a description of the various modes by which fishing is prosecuted in the Gulf of Naples, whether commendable or otherwise, with engravings of the nets and other apparatus used; second, the consideration of the various modes of fishing, and their relationship to the present and prospective supply; third, memoranda in regard to the localities in which the different kinds of fish and other marine animals are to be found, and the favourite places for depositing their spawn; and fourth, a systematic catalogue of the different species of marine animals found in the Gulf of Naples, and gathered for the purpose of serving as food.

PROF. MARSH reports to the *American Journal of Science* the discovery, during his explorations in 1871, of a remarkable fossil bird. It was found in the Upper Cretaceous of Western Kansas, and the remains consist of the greater portion of the skeleton, at least five feet in height, and which, although a true bird, as is shown by the vertebræ and other parts of the skeleton, differs widely from any known recent or extinct forms of that class, and affords a fine example of a comprehensive type. The bones are all well preserved. The femur is very short, but the other portions of the legs are quite elongated. The metatarsal bones appear to have been separated. On his return the professor proposes to describe this unique fossil under the name of *Hesperornis regalis.*

IN the expedition against the Losshais, who have attacked our tea plantations in Cachar, the interests of science have been cared for. Lieutenant Browne, 44th Foot, known in India as an able naturalist, has charge, with a trained native from the Indian Museum at Calcutta, to act as collector. Something is expected from the unexplored regions of the Losshai country.

HERR PAUSCH, a member of the late German polra expedition, recently made a communication to the German Anthropological Society in regard to certain abandoned habitations of the Esquimaux in East Greenland. He remarked that at each of seven different points they found three stone houses, some of them certainly over one hundred years old. These were winter huts, the remnants of their summer abodes being indicated by stone rings. In many places there were indications of stone graves, and from the skeletons found in them tolerably well-preserved crania were obtained, agreeing with the Eastern Esquimaux type as described by Virchow, and exhibiting the carnivorous habit in the highest degree. Remains of wood carving, tolerably well executed, occurred with the dead bodies, and in the heap were found bone knife-handles, harpoons of bone, arrow-tips, and even knife-shaped pieces of iron, probably obtained from the English expedition of 1823.

IN referring to the explorations of Dr. Hayden about the Yellow Stone Lake during the past summer, mention was made of the fact that the trout all seemed very much infested with a peculiar kind of worm, which interfered considerably with the enjoyment of eating them. Specimens of this animal have been submitted to Prof. Leidy, of Philadelphia, who reports that they represent a new species or type of worm, of the genus *Dibothrium.* Two species of the genus have long been known as infesting salmon and other members of the trout family in Europe, but both are decidedly different from the new form just mentioned.

THE Trustees of the Museum of Comparative Zoology at Harvard College, Cambridge, U.S.A., have issued their Annual Register for 1870, together with the Report of the Director, Prof. Agassiz. It is stated that the accessions to the Museum during the past year had been very great and of surpassing importance. Foremost stands Deyrolle's collection of Curculionidæ, presented by Mrs. A. Hemenway; next the collection of Galls of Baron d'Osten-Sacken, presented by him; then the magnificent collection of Fossil Plants of M. Lesquereux, especially remarkable for the exquisite selection of the specimens it contains, and that of Insects of Texas, made by Mr. J. Boll, both of which have been bought by the Museum; and not least the unparalleled collection of Neuroptera, brought to America by Dr. Hagen, and now deposited in the Museum. There are special reports on the Mammalia and Birds by Mr. J. A. Allen; on the Fishes by Dr. Franz Steindachner; on Conchology by J. G. Anthony; on the Articulata by Dr. Hagen; and on the Palæontological collections by Prof. Shaler, Mr. J. B. Perry, and Dr. G. A. Macak.

WE have received the Register of the Trustees, Officers, and Students of the Lehigh University, South Bethlehem, Penn., U.S., for the year 1871-72. The University was founded by a gift, in the year 1865, from the Hon. Asa Parker, of the sum of 500,000 dols., and a site of land containing 56 acres in the Lehigh Valley. The purpose of the founder was "to provide the means for imparting to young men of the valley, of the state, and of the country, a complete professional education, which should not only supply their general wants, but also fit them to take an immediate and active part in the practical and professional duties of the time. The system determined upon proposes to discard only what has been proved to be useless in the former systems, and to introduce those important branches which have been heretofore more or less neglected in what purports to be a liberal education, and especially those industrial pursuits which tend to develop the resources of the country,—pursuits, the paramount claims and inter-relations of which natural science is daily displaying—such as Engineering, Civil, Mechanical, and Mining; Chemistry, Metallurgy, Architecture, and Construction." For this purpose, special classes in all the above-named subjects have been instituted; and by the liberality of Robert H. Sayre, one of the trustees of the University, an Astronomical Observatory has been erected in the University grounds and placed under the care of the Professor of Mathematics and Astronomy, for instruction of students in Practical Astronomy. The Observatory contains an equatorial, by Alvan Clark, of six inches clear aperture, and of eight feet focus; a zenith sector, by Blunt; a superior astronomical clock, by William Bond and Sons; a meridian circle and a prismatic sectant, by Pistor and Martins.

Dr. E. Askenasy, in his "Beiträge zur Kritik der Darwinschen Lehre," contrasts the doctrine of Natural Selection as carried out to its full extent by Darwin in his "Origin of Species" and "Variation of Animals and Plants under Domestication," with the modified form of theory adopted by Nägeli in his "Conception and Origin of Species in Natural History."

The first part of Dr. N. J. C. Müller's "Botanische Untersuchungen" treats of the separation of carbonic acid by the green parts of plants under the influence of sunlight, and is illustrated by a plate, delineating, in the form of curves, the effects of the different rays in the solar spectrum.

Dr. Gerard Krefft, in a paper on the Australian Vertebrata, Fossil and Recent, points out how valuable would be a general study of Natural History in a country like Australia, where every pool and creek teems with animal life, numerous mussels, various kinds of cray-fish, turtles, frogs, lizards, freshwater snakes, and other creatures, all of which are more nourishing to a starving human being than the wretched nardoo on which the lamented Burke and Wills tried to subsist. He advocates the establishment of district museums, and that the children should be taught to observe the habits and economy of different animals, in particular of those which are useful, by which means the wealth of the country would be much increased. Dr. Krefft promises hereafter a complete natural history of Australian Vertebrates, which will be the first ever published.

The "American Horological Journal," published in New York, of which several numbers lie on our table, contains not only articles of special interest to manufacturers and vendors of clocks and watches, but others on Spectrum Analysis, and kindred scientific subjects.

"Index to Prices" is responsible for the following:—The demand for human hair is so great that it is impossible to supply it. Price has risen to 16*s.* a pound. As much as 1,000 dols. has been offered for a "head of hair" six feet long. Some ladies dress fifty to sixty miles of hair every morning.

At the meeting of the Society of Arts held last week, Dr. Brands, Inspector-General of Forests to the Government of India, said that the cinchona plantations were now become almost forests. Before long they would be able to be coppiced every six or eight years, just as oak coppices were treated in Germany, Scotland, and elsewhere, every fifteenth or eighteenth year, and this would probably be the simplest and most profitable mode of getting the bark. The introduction of ipecacuanha into India was also alluded to. Dr. Masters expressed an opinion that there must be dozens, if not scores, of plants indigenous to that country, having the same medical properties as ipecacuanha, which could be much more easily utilised.

According to the editor of the *Journal of Conchology*, of Paris, the Paris Museum received twenty-three shots from cannon of the German besiegers in the course of the siege, destroying many of the plant-houses. Two of these balls exploded in the conchological laboratory, in the care of Prof. Deshayes, causing great injury to the specimens, and the *Septaria* in the general collection were literally ground to powder. The large collection of shells of the lower sands of the Paris basin was entirely destroyed. This is much to be lamented in a scientific point of view, as it contained many types. A ball also passed through a glass case containing the Unios and Anodonta.

At a late meeting of the State Dental Society of Pennsylvania one of the members, Dr. Barker, is reported in the *Dental Times* (July 1871) to have read an essay on Irregularity of Teeth, the circumstances favouring it, and suggestions on its prevention and treatment. The essayist held the opinion that a retrograde metamorphosis is going on in human teeth. To obviate this there must be improvement in the mode of living, the use of more substantial food, and from the time of the appearance of the deciduous teeth children should be under the care of an educated dentist; so that when the permanent teeth begin to erupt they may be properly guided, and a regular arch result. As a rule the first permanent molars should be extracted to make room for the succeeding teeth, for the jaws of the Anglo-Saxon race are shortening, and no longer have room for thirty-two teeth. How will this end?

On January 28, the town of Schamachi, in the Caucasus, was totally destroyed by a succession of earthquakes. Few houses remain standing, and many lives have been lost.

A Correspondent of the *Globe* writes to say that the recent intelligence, describing the total destruction of the city of Oran in Chilé by an earthquake, must be a mistake. He says, that the city of Oran in the province of Salta, in the Argentine Confederation, was destroyed by an earthquake, on October 22, last year, but very few lives were lost. This is the earthquake referred to in Nature (p. 251), but the date was there wrongly given as November 15.

Between ten and eleven at night, on December 12, two shocks of earthquake were felt at Serampore, in quick succession. The second and the strongest lasted about ten seconds, and seemed to move from north to south. The vibrations were very strong, but no great amount of damage was done.

The Rangoon Mail states that on the night of December 12, an earthquake which lasted about ten seconds was felt at Prome. The wave appeared to travel from north-east to south-west. The shocks were stated to be severe, and followed in quick succession, but no damage is reported in the town. The earthquake occurred on the night of the new moon. A letter received from Herzadak states that an earthquake was felt there on the same night. In another paragraph we give an account of an earthquake felt at the same time at Serampore.

On the 12th of December at 10.5 P.M. an earthquake was felt at Calcutta with a shock lasting eight seconds, and moving from east to west. It was felt at Ducca about the same time, but its direction was considered to be from north to south. It was also felt at Akyab and in Burmah.

WALLACE ON THE ORIGIN OF INSECTS*

AMID all the discussions to which the question of the Origin of Insects has given rise, it is to me surprising that one of the most ingenious and remarkable theories ever put forth on a question of natural history has not been so much as once alluded to. More than six years ago, Mr. Herbert Spencer published, in his "Principles of Biology," a view of the nature and origin of the annulose type of animals, which goes to the very root of the whole question; and, if this view is a sound one, it must so materially affect the interpretation of all embryological and anatomical facts bearing on this great subject, that those who work in ignorance of it can hardly hope to arrive at true results. I propose, therefore, to lay before you a brief sketch of Mr. Spencer's theory, with the hope of calling attention to it, and inducing some of you to take up what seems to me to be a most promising line of research; and, although the question is one on which I feel quite incompetent to form a sound judgment, I shall call your attention to the light which it seems to throw on some of the most curious anomalies of insect structure.

The theory itself may be ennunciated in very few words. It is, that insects, as well as all the Annulosa, are not primarily single individuals, but that each one is a compound, representing as many individuals as there are true segments in the body, these individuals having become severally differentiated and specialised to perform certain definite functions for the good of the whole compound animal.

Mr. Spencer first calls attention to the fact, that among the undoubtedly compound animals (which are almost all found in the sub-kingdoms, Cœlenterata and Molluscoida) the several individuals are rarely combined in such a manner as to necessitate any physiological division of labour among them. The associated individuals of a Hydrozoon or an Ascidian are each free to spread their tentacles, to draw in currents of water, and to select their food, without in any way interfering with each other, because the compound animal is either branched or approximately hemispherical, and thus there is no necessity for any of the combined individuals to become especially modified with regard to the rest. But should a compound animal have its component individuals arranged in a linear series, there would most probably arise a marked difference of conditions between the two situated at the extremities and those between them. If they remained united, some modification must have occurred to adapt each to its condition. But if, further, the series should be fixed at one end, the other being free, a new differentiation must arise; for the two ends being very differently situated, the intermediate ones will also differ accordingly as they are nearer one end or the other. Here there is a cause for the differentiation of united individuals that does not exist in any branched or other symmetrical arrangement than a linear one. Some of the Salpidæ show such a rudimentary linear aggregation, but their mouths and vents being lateral the individuals are so similarly situated that no differentiation need occur. A little consideration will show us that this is one of those cases in which perfectly transitional forms are not to be expected. A permanent union of individuals in a linear series, such as to necessitate differentiation of function among them, could only be effected by a series of co-ordinated gradations, each of which would have so great an advantage over its predecessor as to necessitate its extinction in the struggle for existence. We cannot expect to find the union without the differentiation, or the differentiation without the complete union; and it will, therefore, be impossible to prove that such was the origin of any group of animals, except by showing that numerous traces of separate individualities occur in their organisation, and cannot be explained by any of the known laws of development or growth in animals not so compounded.

In the structure of the lower Annelids we do find strong indications of such an ancestral fusion of distinct individuals. These animals are composed of segments, not merely superficial, but exhibiting throughout a wonderful identity of form and structure. Each segment has its branchiæ, its enlargement of the alimentary canal, its contractile dilatation of the great blood-vessel, its ganglia, its branches from the nervous and vascular trunks, its organs of reproduction, its locomotive appendages, and, sometimes, even its pair of eyes. Thus every segment is a physiological whole, having all the organs essential to life and multiplication. Again, just as other compound animals increase by gemmation or fission, so do these. The embryo leaves the egg a globular ciliated gemmule; elongation and segmentation then take place, always in the hinder part, so as to elongate the compound animal without interfering with the more specialised anterior segment. In the Nemertidæ, and some Planaria, spontaneous fission occurs, each part becoming a perfect animal, and in the Tænia this is the usual mode of reproduction. The account given by Professor Owen in his "Comparative Anatomy of Invertebrates" is very suggestive of Mr. Spencer's view. He says:—"On the first appearance of the embryo annelid it usually consists of a single segment, which is chiefly occupied by a large mass of unmetamorphosed germ-cells. And these are not used up, as in higher animals, in developing the tissues and organs of an undivided or individual whole, but, after a comparatively slight growth and change of the primary segment, proceed in the typical orders to form a second segment of somewhat simpler structure, and then repeat such formations in a linear series, perhaps more than a hundred times. So that we may have a seeming individual annelid, consisting of many hundred segments, in which a single segment would give all the characteristic organisation of such individual, except some slight additions or modifications, characterising the first and last of the series." He also tells us that spontaneous fission has now been observed to take place in almost every order of Annulata; and, in many, artificial fission produces two distinct individuals. In some cases the compound animal consists of very few segments, three only in the genus Chætogaster, the fourth always separating as a zooid, and forming a new animal. In the higher Articulata, the process of gemmation goes on to a considerable extent in the egg, and even afterwards in some cases, but more or less irregularly. Thus the larva of Iulus is hatched with eight segments, and at the first moult it acquires six new ones, which are added between the last and the penultimate.

The gradual fusion of the once distinct individuals into a complete unity, is shown in a very interesting manner as we advance from the lower to the higher forms. In the Annelida, Dr. Carpenter tells us, the spiracles of each segment are separate, and do not communicate internally with those of other segments. In the Myriapoda they partially communicate, while in the Insecta they communicate perfectly by a system of anastomosing vessels. The same thing is indicated by the various positions of the chief spiracles. In Smynthurus among the Poduridæ there are only two, opening under the side of the head immediately beneath the antennæ. In Solpugidæ (Arachnida) they are situated between the anterior feet; in some spiders they open near the end of the abdomen, in others at its base. The position of the mouth and eyes at the anterior extremity of the body, and the vent at the posterior, are obviously what would arise as soon as any specialisation of function in the series of zooids occurred. It is not, therefore, surprising that we never find these change their position. But for the respiratory and generative organs there is no such necessity for fixity of position, and as *they* existed originally in every segment, we can well conceive how, as articulate forms become more and more modified, it would sometimes be useful to the compound animal for these organs to become abortive or developed in different parts of the body. We have seen that this is to some extent the case with the former organs, but it occurs to a much greater extent with the latter.

The most generalised form is to be seen in the intestinal worms, each segment of which possesses a complete hermaphrodite reproductive apparatus; so that, in this respect, no less than in their capacity for spontaneous fission, these creatures are really what we should expect the early type of compound animals to be. This, however, is a rare case, but even in the much higher leeches there are testes in no less than nine of the segments, and Dr. Williams discovered a direct passage from the spermatheca to the ovaries, which seems to indicate internal self-fertilisation. It is, however, in the lower Arthropoda that we find the most curious diversities in the position of these organs. In the Glomeridæ the genital openings in both sexes are situated in the third segment, just behind the insertion of the second pair of limbs. In the Polydesmidæ the female organs are in the third segment, while those of the male are in the seventh segment. In Iulus the same organs are situated in the fourth and seventh segments respectively. The Chilopoda, on the other hand, have them near the end of the body, as in most insects. In the Acarina the ovaries open on the middle of the abdomen or on the under side of the thorax, either between or behind the last pair of legs. In spiders the seminal orifice is at the base of the abdomen, but the palpi are the intromittent

* Extracted from an Address read at the Anniversary Meeting of the Entomological Society of London on the 22nd January, 1872, by Alfred R. Wallace, F.L.S., F.Z.S., President, &c.

organs; these are spoon-shaped, and are besides armed with horny processes, hooks, and other appendages, and must be looked upon as true generative organs. In the Astacidæ the sexual organs of the male are at the base of the first pair of abdominal legs, those of the female at the base of the third pair. Among the true winged-insects there is one remarkable case of abnormal position of these organs, in the dragon flies, which have the seminal vessels in the ninth, while the complex male sexual organs are situated in the second, abdominal segment. It is interesting to note that this curious anomaly occurs in an order which is considered to be of the greatest antiquity and most generalised type among the true insects.

There are many other facts of a similar character to those I have now touched upon, and they all become clearly intelligible on the theory of Mr. Spencer, that the Annulosa are really compound animals, or, as he expresses it, "aggregates of the third order;" while the other great groups of highly organised animals—Mollusca and Vertebrata—are typically simple animals, or "aggregates of the second order," (the cells of which their structures are built up being "aggregates of the first order"). Nothing of a similar character is to be found among the two latter groups. No molluscous or vertebrate animal can be divided transversely so that the separate segments shall be in any degree alike, and contain repetitions of any important organs. The distinct separation of parts in the vertebral column has been acquired, for it is less visible in the lower types than in the higher (the reverse of what obtains among insects), and in the lowest of all is quite absent; while in none is there any corresponding multiplicity or displacement of respiratory, circulatory, or generative organs. The vertebral column corresponds rather to the segmented shell of the Chiton, and has no more relation than it to the essential plan of the more important vital organs. Neither does any mollusk or vertebrate undergo spontaneous fission, nor that complete and progressive segmentation in the process of development which is characteristic of all Annulosa; nor do they ever exhibit the phenomena of parthenogenesis or alternation of generations, the essential feature of both which is, that numerous individuals are produced from a single fertilised ovum, by a process analogous to (or perhaps identical with) ordinary gemmation, and both which phenomena sometimes occur even among the higher insects.

In concluding this short sketch of a remarkable theory, I would observe, that if it is a true one it at once invests the objects of our study with a new and exceptional interest; because they are the most highly developed portion of a group of animals which will, in that case, differ fundamentally in their plan of structure from all other highly organised forms of life. In the study of the habits, instincts, and whole economy of insects, we shall have to keep ever in view the conception of a number of individualities fused into one, yet perhaps retaining some separateness of mental action, a conception which may throw light on many an obscure problem, and which will perhaps materially influence our ideas as to the nature of life itself. We must remember also, that if the insect is really a compound animal, then the only true homology that can exist between it and a vertebrate, or a mollusk, will be one between a single segment and an entire animal, and the search after any other will be so much lost time. Especially must the acceptance of this theory have an important bearing on all embryological and genetical studies; and if the facts and arguments adduced by its learned and philosophical author do make out even a *primâ facie* case in its favour, it must deserve the careful and unbiassed consideration of all who endeavour to solve the problem of the Origin of Insects.

THE AUSTRALIAN ECLIPSE EXPEDITION

WE have already announced that no scientific results are to be expected from the Australian Eclipse Expedition, owing to the unpropitious state of the weather. The following particulars are obtained from the *Melbourne Argus*:—

"The five days intervening between the arrival at No. VI. Island and the eclipse were employed by the astronomical party in erecting and testing the instruments. Tents had to be put up, brick foundations and pedestals built, and distances determined. There was plenty of hard work, and the time at the disposal of the astronomers was found to be none too much. Nor were those who had to sleep on shore with the instruments to be envied. Possession of the island was hotly disputed by legions of rats, who behaved in the most impudent manner. They boldly eyed the operations in the daytime, winking wickedly from behind the tufts of grass. Every night they held a corroboree in the tents, coursing over the instruments and the forms of the wearied sleepers, gnawing hats and any baggage which promised a toothsome morsel; and in some instances they had the audacity to bite the men who attempted to brush them away. The passengers filled up the interval by visits to the mainland, and one or two of the neighbouring reefs and islets. On Thursday, December 8, Mr. Moore formed a party and went to Cape Sidmouth, the boat carrying provisions for three or four days. A native on the beach seemed much alarmed at their approach. When they landed he ran off at full speed and was not seen again. Only two other blackfellows showed themselves, though the tracks and camp fires proved that there were many in the neighbourhood. These blacks were known to be hostile, and it was necessary to take precautions to guard against a surprise. The master of the schooner *Challenge*, from Sydney, bound for Cape York, passed with his vessel a few yards astern of the *Governor Blackall* that morning. On hearing that a party had set out with the intention of landing at Cape Sidmouth, he expressed the consoling opinion that if they entered the bush they would never come out of it again. But no such disaster befel.

"On the hills, which rose abruptly a few hundred yards from the beach, were well-defined quartz reefs, and the neighbourhood presented all the appearance of auriferous country. A few miles from Cape Sidmouth was found an enormous heap of the bones of the dugong, the strange mammal which inhabits these seas. There were nearly two tons of bones, piled up in fantastic array, with all the skulls on top. At every turn were ant-hills, rising in solid cones from 6 ft. to 12 ft. high, and almost as hard as granite. Some of them had fine pinnacles, and these airy minarets, clustered together in graceful shapes, had a very pleasing effect. The numerous screw pines were also an agreeable feature in the landscape. The mountains, eight or ten miles inland, were well wooded, with occasional abrupt squares of grassed land.

"Mr. Moore prosecuted his botanical researches on the mainland during two days. Those who understand botany may be interested to learn from his account that the high ground at the cape is sparsely covered with stunted growths and trees, chiefly *Eucalypti* and *Grevillea chrysantha*. Advancing into the interior, broad-leaved acacias and arborescent species of *Hakea* and *Melaleuca* principally characterise the open forest country. There are belts of thick jungle scrub of no great width, in which a very slender and graceful palm, which is believed to be new, occurs in great abundance. A species of *Nepenthes*, or pitcher plant, is also found in great profusion. Araliaceous trees are numerous. Ferns are scarce, but in the open forest the ground is thickly covered with *Schizæa dichotoma*. A very remarkable plant was found as an undergrowth in this, having large white bracts and bright green foliage. It is supposed to be a species of *Mussænda*. Toward the north of the cape is a long, low, flat country, chiefly covered with mangrove. The sandy patches contain a variety of undershrubs and climbers, with a tree here and there. The silk-cotton plant (*Cochlospermum gossypium*) also varies the scene with its delicate flower. Among these shrubs a very interesting plant—a species of *Eugenia*—was found. It bears a fruit about the size and colour of a cherry, having a pleasant sub-acid flavour. This fruit was largely eaten by the party, and the tree which bears it is supposed to be well worthy of cultivation. The vegetation is otherwise principally characterised by a species of *Busbeckia*, *Elæodendron*, *Hibiscus*, *Bauhinia*, and a species of *Banksia*. After leaving the mainland the party visited No. VII. Island of the Claremont group, where Mr. Brazier added an *Auricula* and a *Bulimus* to his previous collection of shells, which included specimens of the genera *Diplommatina*, *Pupa*, *Helicarion*, *Helix*, *Truncatella*, *Pythia*, and *Cassidula*. Had the expedition selected a portion of the mainland for the observing point, there would have been some interesting and extensive explorations in the interior. The party were fully equipped with arms and ammunition, some supplied by the Government and some privately owned, but with the ship nine miles off, and the limited time at our disposal, much exploration was impracticable. In any case, there was no anchorage for the vessel within two miles of the shore, and that was one of the reasons why the island was preferred for the observatory.

"On Thursday afternoon, some of the excursionists went in the captain's boat to look for shells on a small sandbank which had come into view, and landed on an island considerably smaller than the fish that Sindbad mistook for *terra firma*. It was intended to visit No. VII. Island, but it seemed that the country we were in search of had gone under water—its custom in the afternoon—and we sailed over part of it. On Friday a visit was paid to the reef, which extends for three or four miles from one extremity of No. VI. Island. The party landed on a patch of sand, and waded about three miles in 2 ft. of water over a coral bottom, in quest of shells. Here we had the wonders of the deep and its strange inhabitants laid at our feet in all their rich variety of colour. Some curious specimens were obtained. There were enormous clams, capable of holding a man's foot in their grip, abundance of *bêche-le-mer*, pearl oysters, all kinds of star fish (some of the most beautiful ultramarine), and many sorts of coral. One member of the party picked up a handsome live conch shell, weighing about 14 lb. Another was delighted with a strange creature belonging to the star fish order. When first taken from the water it had all the appearance of a pentagonal plum cake of about 2 lb. weight, beautifully encrusted with sugar crystals and profusely ornamented with coloured caraways But removed from the sea water the glories of this appetising-looking creature only survived a brief period. When we had been a couple of hours prospecting on this rocky bottom of the ocean the tide rose rapidly, and we had no sooner got into the boat than the whole reef dropped out of view. The attractions of No. VI. Island proper were exhausted for the majority of people in a very brief space, but one or two were sometimes to be seen meandering along the beach, the very pictures of placid contentment. The presence of a porter bottle in one hand and an oyster knife in the other seemed to suggest that they had been visiting some of the oyster beds. They were so full of blessed condition that conversation was superfluous, and on these occasions we passed them without making a remark to disturb their dreamy happiness.

"Repeated attempts had been made since leaving Sydney to catch fish, but without success, only one small one having been hooked. This afternoon, however, great sport was afforded by the sharks. The bathers who went over the ship's side every morning had been warned that there were several of these villanous footpads of the sea about; but nothing but the sight of these rapacious monsters on deck sufficed to induce them to abandon the practice. The method adopted in catching these sharks enabled both anglers and riflemen to take a part. As soon as a shark was hooked his head was drawn about six inches out of water, and three or four conical balls lodged in that ugly flat prominence settled him before he was hauled on deck to be drawn and quartered. In this way six, measuring from 9ft. to 12ft. in length, were disposed of in the course of an hour and a half, besides two which were shot in the sea, and turned over on their backs to sink. After this experience the morning ablutions of the company were limited to splashing about the decks under the hose.

"Most of the company slept through the night on deck. With the marvels of the stellar firmament above, whichever way the eye was directed, we became contemplative astronomers, like the Chaldean shepherds of old. The striking garnitude of the sky formed an endless scene to gaze at and admire. Little wonder that the ancients made the heavenly bodies objects of religious veneration. When the sun had finished his daily round, we watched the lesser light that rules the night, making her stately procession through the heavens, and the infinite variety of stars moving in concert through boundless space. There is much of the charm of romance in the study of the science which teaches us that there are other globes, in comparison with which the earth is but a speck, and proves to us that the 'patines of bright gold' with which the sky is inlaid are not simply points of light, but worlds like our own, with systems of satellites moving in their appointed courses in obedience to the laws of nature. These unknown countries afford abundant scope for interesting speculation. The mind endeavours to picture the circumstances of their inhabitants, and to conjecture, by some earthly standard, what their pursuits may be. But the imagination refuses to believe that the occupants of these bright worlds are subject to the conditions which bind those who dwell upon 'the dim spot which men call earth,' and that they have cities like ours, with their sins and their sorrows. There were some stars in the firmament which old residents of Australia had not seen for many years. While our vessel was progressing northwards, constellations unknown in the south had been coming into view, and we saw Cassiopeia and Perseus gradually rise above the horizon with great brilliancy. Apart from the scenery of the heavens, the sea was beautifully phosphorescent. When the phosphorescence was stirred all the sparks were converted by the action of the retina into lines of light, which played around the ship in radial streamers.

"No time was lost by the astronomical party when they had once effected a landing on Eclipse Island, as we christened the point of observation. The islet was soon converted into a bustling little canvas town. From nearly every tent some instrument peered, all pointing in the one direction, as though these mortals, with their puny optics, thought to stare out of countenance the great Eye of Day. The Victorian party had two analysing spectroscopes and an integrating spectroscope, both equatorially mounted. The first was in the hands of Mr. Ellery, and the second was to be worked by Mr. Foord, both gentlemen having assistants to use the finding telescopes attached to pick out portions of the corona for examination. The two analysing spectroscopes were for examining the nature of the light of the chromosphere and the corona; and the integrating spectroscope, entrusted to Mr. M'George, was designed to examine the nature of the whole light, all the observations being directed with a view to determining the character of the orb from which the light proceeds. Prof. Wilson had two Savart's polariscopes. The object of polariscopic observations is to ascertain whether the light of the corona is that of a self-luminous body or a reflected light; also, in the case of its being a reflected light, to determine the angle of incidence, the great question to be settled being whether the corona is an appendage of the sun, or whether it exists in our atmosphere. There was also a magnetic theodolite to record magnetic disturbances. Mr. Moerlin, assisted by Mr. Walter, had charge of the photographic department. The principal instrument was one of Dalmeyer's rapid rectilinear lenses of 4in. aperture and 30in. focal length, giving an image of about three-tenths of an inch in diameter, equatorially mounted, and driven by clockwork. It was intended to take ten views during the totality. Mr. White, assisted by Mr. Black, directed the instruments for determining the position of the station and predicting the time of the different phases of the eclipse. On the morning of the 7th December a brick pier to support the transit instruments was built. The pier was made square, as the instruments had to be placed not only in the meridian for the accurate determination of the time and longitude, but also at right angles to the meridian for finding the altitude. The first observations were made by an eight-inch altazimuth, which does not require such a massive stand as the transit. This gave very nearly the local time and the direction of the meridian. By means of these data the transit was fixed at right angles to the meridian, the finding of the latitude by this method being more troublesome and requiring finer weather than the finding of the time. On the first night the sky was rather cloudy, so that only two complete observations could be taken. The next night three observations were obtained, and the third night four observations were made. This being considered sufficient for the latitude, the instrument was next morning placed in the meridian, but the weather was so unfavourable that no observations could be taken in that position, so that the altazimuth had to be resorted to for the time observations.

"The Sydney party were furnished with an equatorial telescope, made by Merz, of Munich, with 7¼in. clear aperture and 10ft. 4in. focal length, mounted on the German plan. Attached to the telescope was an apparatus for taking photographs in the principal focus of the object glass; also a photographic lens and camera, by which a second series of photographs could be taken simultaneously, the photographic lens having a 3in. aperture and 30in. focal length. There were, in addition, two small telescopes of 2in. aperture, with a magnifying power of 20, mounted equatorially and driven by clockwork, and a third telescope of 3½in. aperture and 4ft. 6in. focal length. The party intended to take a double series of photographs, to make two independent drawings, and to make naked-eye drawings and observations. The duties were apportioned as follows:—Mr. Russell, the Government astronomer, was to take photographs with the large telescope; Mr. Beaufoy Merlin photographs with the camera, the Rev. W. Scott and Lieutenant Gowlland to make drawings with small telescope, and Mr. W. M'Donnell to act as timekeeper. The passengers were furnished with diagrams, and each received instructions to pay special attention to some one particular portion of the phenomena. When the day of the eclipse arrived the instruments were all working admirably. There had been numerous rehearsals to secure the utmost economy of time, and all felt that nothing but clear weather was needed for success.

"On Monday afternoon, the 11th of December, for the first time since leaving Melbourne, the sky became seriously overcast. The clouds had been gathered in dense dark masses all the earlier part of the evening, and at ten o'clock at night there was an awful thunderstorm, which lasted over an hour. The glow of the lightning, which came down in sheets of flame, and the rattle and crash of the thunder which followed the flash instantly, were inexpressibly grand. It was something quite beyond the experience of any one on board. A portion of the astronomical party returned from the shore in the middle of the storm. While they were ascending the ship's side the lightning struck the iron rigging, leaped across from stanchion to stanchion in balls of fire, and broke off at the ropes depending from the dead-eyes with loud crackling noises before it reached the sea. The vessel was lit up from stem to stern with a blinding light, and those on deck could see nothing for some seconds after each flash. The party in the boat were so much affected in this way that some alarm prevailed at first. Each one thought he had been deprived of sight, and asked his neighbour how it was with him. Had we been in a wooden ship the consequences would in all probability have been serious. This storm unfortunately did not clear the atmosphere. Next morning, the day of the eclipse, every eye was turned heavenward. To our dismay there was not a speck of sky to be seen. At ten o'clock there were several breaks in the clouds, and the sun showed himself for a few seconds, but an hour and a half later all was dense cloud again. Things looked brightest at mid-day, when there seemed to be a possibility of a fine afternoon. Then dark clouds swept up from the horizon, and extinguished almost every hope. At two o'clock there was yet another chance, though a faint one. This was tantalising. Every interest centred in a few patches of sky and their relations to the neighbouring clouds. They were aggravating clouds of every imaginable form and variety—cirrus, stratus, cumulus, nimbus—all were there at various times of the day, assuming the most distressing shapes, but giving no promise of dissolving.

"The computation of the duration of the eclipse was found to be very accurate, the eclipse occurring, as near as could be judged, three or four seconds before the predicted time. The computation was as follows:—First contact, 1h. 15m. 6s.; commencement of totality, 2h. 42m. 23s.; end of totality, 2h. 45m. 49s.; last contact, 4h. 1m. 6s. At the time of the first contact there was scarcely a rift in the canopy of clouds. The sun was wholly obscured. A few seconds later a passing glimpse was obtained, showing that the encroachment of the dark body of the moon on the bottom edge of the sun's disc had begun. Then all was dark again, excepting a faint luminous spot indicating the radiant body's position. A sharp shower fell at this time, and the instruments exposed had to be covered up. A drizzling rain continued during the remainder of the afternoon. At the faintest indication of a break in the clouds the astronomers ran out of their tents, and endeavoured to take observations, but without any result. Seven minutes before the commencement of totality there was a gleam of light from the sun, but the phase of the eclipse could not be discerned. We caught a momentary glimpse of the silver sickle of the sun at the top, just before the full obscuration. Then darkness fell suddenly like a pall on the surrounding objects, and we knew that totality had begun. It was a strange weird light at first. The large billowy clouds assumed olive and purple tints, and then changed to an ashen hue. These colours were reflected in the sea with some variations of light green and copper. Men looked livid in the light, and everything around had a most unearthly appearance. The steamer at anchor showed with a wonderful distinctness; every line, spar, and bit of cordage stood out against the horizon with the sharpness of a highly-magnified stereoscopic picture. There was no total darkness, owing, probably, to the amount of light diffused in the clouds. During totality, newspaper print could be read without much difficulty. Nor was there any perceptible diminution in the temperature. The three minutes and a half seemed exceedingly short. We saw nothing of the corona beyond a brief glimpse of a luminous mark shining faintly through the vapours. Some said they detected a decided red tinge. The clouds turned black, the tints disappeared from the sea, and utter darkness seemed coming upon us, when a few rays of light played upon the edges of a great bank of clouds in the N.W., some of the grey tints of dawn appeared, and daylight came back with a rush, as from the lifting of a veil. A hawk which had been sailing about swept down into a bush on the island to roost as soon as totality began. When daylight returned, he was astonished to find himself within a few feet of forty or fifty men, and flew off in wild alarm. Though daylight had returned, the sun was still hidden by the clouds. A minute later we faintly saw the re-appearance of the solar limb at the bottom like a fine luminous thread, when more clouds interposed and shut out the great luminary for the remainder of the afternoon. This was all that was vouchsafed to us of the grand phenomena of a total solar eclipse. Never was Nature more assiduously wooed to reveal her treasures to science. But it was all to no purpose. Of the upward and onward march of the moon, the successive disappearance of the solar spots, the brilliant breaking into view of Bailey's Beads, the passage of the shadow through the air, the rose-coloured prominences and coronal radiations during totality, the reappearance of the solar crescent, and the final retreat of the lunar shadow into space, we had seen nothing. No observation could be taken by instrument. Mr. Russell exposed a photographic plate for twenty seconds during totality, but got no result.

"Nothing remained but to pack up and head the ship for home. The work was commenced before the eclipse was over, the rain falling dismally all the time, and was completed in less than three hours. The disappointment to all was very great. It was especially felt by the astronomical party, but they bore it bravely, as became men who had faithfully performed their duty. When over dessert that evening Mr. Ellery proposed in the interests of science, "Success to the Other Eclipse Expeditions," there was not one who did not cordially wish that all the other observers might have presented to their view the radiant globe projected on an azure sky, instead of the mountains of dull cloud that desolated our hopes.

"Later in the evening the schooner *Matilda*, bound for Sydney, from Torres Strait, with twelve tons of pearl shell, came alongside. The master and first officer reported having seen the eclipse very distinctly while near Night Island, in lat. 13° 9′ S., long. 143° 39′ E., about 15 miles from No. VI. island. They were not aware that the eclipse was going to occur, and at first took the darkness for approaching bad weather, until one of them happening to look under the mainsail, observed the phenomenon. Though wholly unprepared for the eclipse, they gave a very intelligent account of it, on being carefully examined by Prof. Wilson. Mr. Walton, master of the *Matilda*, stated that he had just ordered some clothes that were drying to be taken down, as bad weather seemed to be coming on, when he happened to look up and see the eclipse. It was so dark that he had to light the binnacle lamp. On a diagram being handed to him, he correctly indicated the points of disappearance and first reappearance of the sun. He drew on a black disc a line showing the boundary of the ring of light round the dark body of the moon, narrower in the right-hand bottom quadrant, and wider, with a projection, in the left-hand top quadrant. The colour of the light, he said, was whitish, like ordinary sunlight. He was particularly asked if he saw any pink light, and said no. He described the boundary as being sharp, and clear towards the moon, but rough and irregular outwards. The breadth of the annulus which he drew was about 1-16th of the diameter of the black disc. He said he and the other officers differed as to the duration of the darkness. The time was variously guessed at from five to ten minutes. His own opinion was that it was seven or eight minutes. There were no clouds on the sun at the time, and the blue sky was visible. Some of the South Sea Islanders on board were very much alarmed, and wept plenteously. Mr. Hore, first officer of the *Matilda*, stated that on his attention being directed to the eclipse, he went below to fetch his sextant in order to use the dark glasses. The captain called to him to make haste, as he was losing the best of it. On coming on deck he saw the dark body of the moon, surrounded by a fine ring of red light, outside of which was a broader ring of paler red light; while all outside of that was as black as night. His drawing on the card showed the breadth of the inner ring to be one-eighth the diameter of the moon, and the breadth of the outer on to be seven-sixteenths of the diameter of the moon. On being pressed as to the colour, he said it was not like fire itself, but like the glow of fire when the fire is concealed. The illustration he used was the glow of a house on fire seen from behind another house. Only one cloud passed over the sun during the eclipse, and that was a very small one. Peter R. Cooper, carpenter on board the *Matilda*, drew a line showing the boundaries of the inner and outer annuli, the inner one extending rather more than half round, the point of first reappearance being the middle of it, the outer one extending less than a quadrant, and being entirely on the upper right-hand side. He described the colour as being like the upper ground part of a kerosene lamp shade in the cabin to which he pointed. The sun looked watery. When he first saw it it was coming from behind scud. There was no sky which could be called blue. It was a whitey sky. Cooper's drawing was marked with radial lines extending across the outer annulus from the inner.

"The return voyage was begun at daylight on the morning of the 13th of December. The only lasting traces of the astronomers left on the island were the photographers' dark rooms and the brick foundations used for the instruments, in which were entombed two bottles containing coins and newspapers and some particulars of the expedition. A member of the party, animated by something of the spirit of Old Mortality in his desire to preserve from oblivion the mortuary memorials of the expedition, inscribed this touching record on the slab which formed the top of one of these pedestals:—'Sacred to the memory of the Australian Eclipse Expedition.'"

SOCIETIES AND ACADEMIES

London

Royal Society, February 15.—"On the Induction of Electric Currents in an infinite plane sheet of uniformly conducting matter," by Prof. Clerk Maxwell, F.R.S.

The currents are supposed to be induced in the sheet by the variation in position or intensity of any system of magnets or electromagnets.

When any system of currents is excited in the sheet, and then left to itself, it gradually decays, on account of the resistance of the sheet. At any point on the positive side of the sheet, the electromagnetic action is precisely the same as if the sheet, with its currents, retaining their original intensity, had been carried away in the negative direction with a constant velocity R, where R is the value, in electromagnetic measure, of the resistance of a rectangular portion of the sheet, of length 1 and breadth 2π. This velocity, for a sheet of copper of best quality of one millimetre thickness, is about twenty-five metres per second, and is, therefore, in general comparable with the velocities attainable in experiments with rotating apparatus.

When an electromagnet is suddenly excited on the positive side of the sheet, a system of currents is induced in the sheet, the effect of which on any point on the negative side is, *at the first instant*, such as exactly to neutralise the effect of the magnet itself. The effect of the decay of this system of currents is therefore equivalent to that of an image of the magnet, equal and opposite to the real magnet, from the position of the real magnet, in the direction of the normal drawn away from the sheet, with the constant velocity R.

When any change occurs in an electromagnetic system, whether by its motion or by the variation of its intensity, we may conceive the change to take place by the superposition of an imaginary system upon the original system; the imaginary system being equivalent to the difference between the original and the final state of the system.

The currents excited in the sheet by this change will gradually decay, and their effect will be equivalent to that of the imaginary system carried away from the sheet with the constant velocity R.

When a magnet or electro-magnet moves or varies in any continuous manner, a succession of imaginary magnetic systems like those already described is formed, and each, as it is formed, begins to move away from the sheet with the constant velocity R. In this way a train or trail of images, is formed, moves off, parallel to itself, away from the sheet, as the smoke of a steamer ascends in still air from the moving funnel.

When the sheet itself is in motion, the currents, relatively to the sheet, are the same as if the sheet had been at rest, and the magnets had moved with the same relative velocity. The only difference is, that whereas when the sheet is at rest no difference of electric potential is produced in different parts of the sheet, differences of potential, which may be detected by fixed electrodes are produced in the moving sheet.

The problem of Arago's whirling disc has been investigated by MM. Felici and Jochmann. Neither of these writers, however, has solved the problem so as to take into account the mutual induction of the currents in the disc. This is the principal step made in this paper, and it is expressed in terms of the theory of images, by which Sir W. Thomson solved so many problems in Statical Electricity. In the case of the whirling disc, the trail of images has the form of a helix, moving away from the disc with velocity R, while it revolves about the axis along with the disc. Besides the dragging action which the disc exerts on the magnetic pole in the tangential direction, parallel to the motion of the disc, the theory also indicates a repulsive action directed away from the disc, and an attraction towards the axis of the disc, provided the pole is not placed very near the edge of the disc, a case not included in the investigation. These phenomena were observed experimentally by Arago, *Ann. de Chimie*, 1826.

February 22.—"On a New Hygrometer." By Mr. Wildman O. Whitehouse.

"On the Contact of Surfaces." By William Spottiswoode, M.A., Treas. R.S.

In a paper published in the "Philosophical Transactions" (1870, p. 289), I have considered the contact, at a point P, of two curves which are co-planar sections of two surfaces (U, V); and have examined somewhat in detail the case where one of the curves, viz., the section of V, is a conic. In the method there employed, the conditions that the point P should be sextatic, involved the azimuth of the plane of section measured about an axis passing through P; and consequently, regarded as an equation in the azimuth, it showed that the point would be sextatic for certain definite sections. It does not, however, follow, if conics having six-pointic contact with the surface U be drawn in the planes so determined, that a single quadric surfaee can be made to pass through them all. The investigation therefore of the memoir above quoted was not directly concerned with the contact of surfaces, although it may be considered as dealing with a problem intermediate to the contact of plane curves and that of surfaces.

In the present investigation I have considered a point P common to the two surfaces, U and V; an axis drawn arbitrarily through P; and a plane of section passing through the axis and capable of revolution about it. Proceeding as in the former memoir, and forming the equations for contact of various degrees, and finally by rendering them independent of the azimuth, we obtain the conditions for contact for all positions of the cutting plane about the axis. Such contact is called circumaxal; and in particular it is called uniaxal, biaxal, &c., according as it subsists for one, two, &c. axes. If it holds for all axes through the point it is called superficial contact; and in the memoir some theorems are established relating to the number of sections along which contact of a given degree must subsist in order to ensure uniaxal contact, as well as to the connection between uniaxal and multiaxal contact. At the conclusion of sec. 3 it is shown that the method of plane sections may, in the cases possessing most interest and importance, be replaced by the more general method of curved sections.

In the concluding section a few general considerations are given relating to the determination of surfaces having superficial contact of various degrees with given surfaces; and at the same time have indicated how very much the general theory is affected by the particular circumstances of each case. The question of a quadric having four-pointic superficial contact with a given surface is considered more in detail; and it is shown how in general such a quadric degenerates into the tangent plane taken twice. To this there is an apparently exceptional case, the condition for which is given and reduced to a comparatively simple form; but I must admit to having so left it, in the hope of giving a fuller discussion of it on a future occasion.

The subject of three-pointic superficial contact was considered by Dupin, "Développements de Géométrie," p. 12, and, as I have learnt since the memoir was written, a general theorem connecting superficial contact and contact along various branches of the curve of intersection of two surfaces (substantially the same as that given in the text) was enunciated by M. Moutard.*

In a corollary to this theorem, M. Moutard states that through every point of a surface there can be drawn twenty-seven conics, having six-pointic contact with the surface. This number is perhaps open to question; and I have even reason to think, from considerations stated to me by Mr. Clifford, that the number ten, given in my memoir above quoted, may be capable of reduction by unity to nine. But this question refers to the subject of that earlier memoir rather than to this.

Geological Society, February 7.—Mr. Prestwich, F.R.S., president, in the chair. 1. "Further Notes on the Geology of the neighbourhood of Malaga," by M. D. M. d'Orueta. In this paper, which is a continuation of a former note laid

* Poncelet, "Applications d'Analyse à la Géométrie," 1864, tom. ii. p. 363

before the society (*see* Q. J. G. S. xxvii., p. 109), the author commenced by stating that his former opinion as to the Jurassic age of the rocks of Antequera is fully borne out by later researches upon their fossils. They apparently belong to the Portlandian series. The author made considerable additions to his description of the Torcal, near the foot of which he has found a sandstone containing abundance of *Gryphæa virgula* and *Ostrea deltoidea*. This he regards as equivalent to the Kimmeridge clay. In the Torcal he has also found a soft, white, calcareous deposit, overlying the limestones of supposed Portlandian age, and containing a fossil which he identifies with the Tithonian *Terebratula diphya*. The author discussed the peculiar forms assumed by the rocks of the Torcal under denudation, which he supposed to be due originally to the upheaval caused by the rising of a great mass of greenstone, portions of which are visible at the surface on both sides of the range. 2. "On the River-courses of England and Wales," by Prof. A C. Ramsay, F.R.S., The author commenced by describing the changes in the physical conformation of Britain during the Jurassic and Cretaceous periods, and the relations which the deposits found during those periods bore to the Palæozoic rocks of Wales and the north-west of England. He stated that the Miocene period of Europe was essentially a continental one, and that it was closed by important disturbances of strata in central Europe, one effect of which would be to give the secondary formations of France and Britain a slight tilt towards the north-west. To this he ascribed the north-westerly direction of many of the rivers of France; and he surmised that at this period the rivers of the middle and south of England also took a westerly course. The westerly slope of the cretaceous strata of England was also, he considered, the cause of the southern flow of the Severn, between the hilly land of Wales and the long slope of chalk rising towards the east. The Severn would thus establish the commencement of the escarpment of the chalk, which has since receded far eastward. The author believed that after the Severn had cut out its valley the cretaceous and other strata were gradually tilted eastwards, causing the easterly course of the Thames and other rivers of southern and eastern England. In these and other cases adduced by the author, the sources of these rivers were originally upon the chalk near its escarpment; and it is by the recession of the latter (which was followed by the formation of the oolitic escarpment) that its present relation to the river-courses has been brought about. The author also referred to the courses followed by the rivers of the more northern part of England, and indicated their relations to the general dip of the strata.

Geological Society, February 16.—Mr. Joseph Prestwich, F.R.S., president, in the chair.—The Secretary read the reports of the council, of the Library and Museum Committee, and of the auditors. The general position of the society was described as satisfactory, although, owing to the number of deaths which had taken place among the Fellows during the year 1871, the society did not show the same increase which has characterised former years. In presenting the Wollaston gold medal to the Secretary, Mr. David Forbes, for transmission to Prof. Dana, of Yale College, Connecticut, the President said:—"I have the pleasure to announce that the Wollaston Medal has been conferred on Prof, Dana, of Yale College, Newhaven, U.S.; and in handing it to you for transmission to our Foreign Member, I beg to express the great gratification it affords me that the award of the Council has fallen on so distinguished and veteran a geologist. Prof. Dana's works have a world-wide reputation. Few branches of geology but have received his attention. An able naturalist and a skilful mineralogist, he has studied our science with advantages of which few of us can boast. His contributions to our science embrace cosmical questions of primary importance—palæontological questions of special interest—recent phenomena in their bearings on geology, and mineralogical investigations so essential to the right study of rocks, especially of volcanic phenomena. The wide range of knowledge he brought to bear in the production of his excellent treatise on Geology, one of the best of our class books, embracing the elements as well as the principles of geology. His treatise on Mineralogy exhibits a like skill in arrangement and knowledge in selection. In conveying this testimonial of the high estimation in which we hold his researches to Prof. Dana, may I beg also that it may be accompanied by an expression how strongly we feel that the bonds of friendship and brotherhood are connected amongst all civilised nations of the world by the one common, the one universal, and the one kindred pursuit of truth in the various branches of science."—Mr. David Forbes, in reply, said that it was to him a great pleasure to have, in the name of Prof. Dana, to return thanks to the society for their highest honour, and for this mark of the appreciation in which his labours are held in England. It had rarely if ever occurred in the history of the society that the Wollaston medal had been awarded to any geologist who had made himself so well known in such widely different departments of the science, for not only was Prof. Dana pre-eminent as a mineralogist, but his numerous memoirs on the Crustaceans, Zoophytes, coral islands, volcanic formations, and other allied subjects, as well as his admirable treatise on general Geology, fully testify to the extensive range and great depth of his scientific researches. — The President then presented the balance of the proceeds of the Wollaston donation fund to Prof. Ramsay, F.R.S., for transmission to Mr. James Croll, and addressed him as follows:—"The Wollaston fund has been awarded to Mr. James Croll, of Edinburgh, for his many valuable researches on the glacial phenomena of Scotland, and to aid in the prosecution of the same. Mr. Croll is also well known to all of us by his investigation of oceanic currents and their bearings on geological questions, and of many questions of great theoretical interest connected with some of the great problems in Geology. Will you, Prof. Ramsay, in handing this token of the interest with which we follow his researches, inform Mr. Croll of the additional value his labours have in our estimation, from the difficulties under which they have been pursued, and the limited time and opportunities he has had at his command."—Prof. Ramsay thanked the president and council in the name of Mr. Croll for the honour bestowed on him. He remarked that Mr. Croll's merits as an original thinker are of a very high kind, and that he is all the more deserving of this honour from the circumstance that he has risen to have a well-recognised place among men of science without any of the advantages of early scientific training; and the position he now occupies has been won by his own unassisted exertions. The President then proceeded to read his Anniversary Address, in which he discussed the bearings upon theoretical Geology of the results obtained by the Royal Commission on Water-Supply and the Royal Coal Commission. The Address was prefaced by biographical notices of deceased Fellows, including Sir Roderick I. Murchison, Mr. William Lonsdale, Sir Thomas Acland, Sir John Herschel, Mr. George Grote, Mr. Robert Chambers, and M. Lartet.—The ballot for the Council and Officers was taken, and the following were duly elected for the ensuing year:—President—The Duke of Argyll, K.T., F.R.S. Vice-Presidents—Prof. P. Martin Duncan, F.R.S., Prof. A. C. Ramsay, F.R.S., Warington W. Smyth, F.R.S., Prof. John Morris. Secretaries—John Evans, F.R.S., David Forbes, F.R.S. Foreign Secretary, Prof. T. D. Ansted, F.R.S. Treasurer—J. Gwyn Jeffreys, F.R.S. Council—Prof. T. D. Ansted, F.R.S., the Duke of Argyll, F.R.S., W. Carruthers, F.R.S., W. Boyd Dawkins, F.R.S., Prof. P. Martin Duncan, F.R.S., R. Etheridge, F.R.S., John Evans, F.R.S., James Fergusson, F.R.S., J. Wickham Flower, David Forbes, F.R.S., Capt. Douglas Galton, C.B., F.R.S., Rev John Gunn, M.A., J. Whitaker Hulke, F.R.S., J. Gwyn Jeffreys, F.R.S., Sir Charles Lyell, Bart., F.R.S., C. J. Meyer, Prof. John Morris, Joseph Prestwich, F.R.S., Prof. A. C. Ramsay, F.R.S., R. H. Scott, F.R.S., W. W. Smyth, F.R.S., Prof. J. Tennant, Henry Woodward.

Zoological Society, February 20, Prof. Flower, F.R.S., in the chair.—The secretary read a report on the additions that had been made to the society's menagerie during the month of January, 1872, and called particular attention to a young king penguin (*Aptenodytes pennanti*), presented by Mr. F. P. Cobb, of Port Stanley, Falkland Islands, and to a collection of African land tortoises, transmitted by Dr. Grey of Cradock, Cape Colony.—The secretary also called attention to the female Sumatran rhinoceros (*Rhinoceros sumatrensis*) just added to the society's menagerie.—A paper was read by Mr. J. W. Clark, on the visceral anatomy of the hippopotamus, as observed in the young specimen of this animal which had died in the society's gardens on the 10th January, 1872. After giving an account of the morbid appearances noticed, Mr. Clark described in detail the stomach of this specimen, which appeared to differ in some points from those examined by previous authorities.—A communication was read from Dr. J. S. Bowerbank, F.R.S., containing the second part of his "Contributions to a General History of the Spongiadæ," in which was contained a full account of two species of the genus *Geodia*.—A paper by the

Rev. O. P. Cambridge was read, "On the Spiders of Palestine and Syria," in which was given a general list of the Araneidea of those countries, together with descriptions of numerous new species, and the characters of two new genera. — A communication was read from Dr. John Anderson, containing descriptions of some Persian, Himalayan, and other reptiles, either new or little known to science. A second paper by Dr. Anderson contained some further remarks on the external characters of the new Burmese macaque, which he had recently described under the name *Macacus brunneus*.—A communication was read from Count Thomaso Salvadori, containing a note on a specimen of Lidth's jay (*Garrulus lidthii*), in the collectian of the King of Italy, which had originally been received alive from Japan. Mr. D. G. Elliot read a note on a Cat described by Dr. Gray in the Proceedings of the Zoological Society for 1867, as *Felis pardinoides* from India, which Mr. Elliot considered to be identical with *Felis Geoffroyii* of S. America.

Manchester

Literary and Philosophical Society, February 6.—E. W. Binney, F.R.S., president, in the chair. Dr. Joule, F.R.S., called attention to the very extraordinary magnetic disturbances on the afternoon of the 4th instant, and from which he anticipated the aurora which afterwards took place. The horizontally suspended needle was pretty steady in the forenoon of that day, but about 4 P.M. the north end was deflected strongly to the east of the magnetic meridian, and afterwards still more strongly to the west. The following were the observations made :—

Time	Deflection from the Magnetic Meridian. ° ′	Time	Deflection from the Magnetic Meridian. ° ′
4.0 P.M. . .	0 50 E.	6.10 P.M. . .	1 24 W.
4.30 ,, . .	0 47 W.	6.12 ,, . .	1 8 ,,
4.55 ,, . .	2 22 ,,	7.41 ,, . .	0 10 ,,
4.58 ,, . .	3 0 ,,	7.43 ,, . .	0 0 ,,
5.9 ,, . .	3 45 ,,	8.9 ,, . .	0 42 ,,
5.12 ,, . .	0 52 ,,	8.31 ,, . .	0 10 ,,
5.23 ,, . .	5 36 ,,	8.54 ,, . .	1 18 ,,
5.24 ,, . .	2 28 ,,	8.58 ,, . .	0 52 ,,
5.35 ,, . .	0 52 ,,	11.3 ,, . .	0 5 ,,
5.55 ,, . .	0 52 ,,		

Mr. Sidebotham states that he also expected the magnificent aurora on account of the violent disturbance of the needle at Bowdon, amounting to at least 3°. Observation with the spectroscope by Dr. Joule showed a bright and almost colourless line near the yellow part of the spectrum. This line appeared in whatever part of the heavens the instrument was directed, and could be plainly seen when the sky was covered with clouds and rain was falling. When looking at the most brilliant red light of the aurora a faint red light was seen at the red end of the spectrum, and beyond the bright white line, towards the violet end, two broad bands of faint white light. Mr. Thomas Harrison stated that he saw the aurora on last Sunday evening from $6^h 15^m$ to $9^h 30^m$ and took spectroscopic observations thereon from various parts of the sky. In each case, however, he discovered only one bright yellow line, situated between D and E, being on Kirchhoff's scale about 1255 to 1260. He is not acquainted with any known substance that gives a corresponding line. The line throughout was very clear and decided, both in the narrow and wide slit ; but he failed to discover any continuous spectrum. The line was also very perceptible by reflection from those parts of the sky in which no trace of aurora was visible ; and although the streaks were both red and white, the spectroscope appeared to give the aurora as a monochromatic light.

Kilkenny

Royal Historical and Archæological Association of Ireland, January 17.—The Mayor of Kilkenny in the chair. Rev. J. Graves (hon. sec.) read the report for 1871. The following members were elected :—Earl of Dunraven, Rev. W. H. Fraser, L. Daniel, J. Lloyd, G. Reade, W. Irvine, J. Martin, W. J. Lemon, A. Gibb, A Menzies, F. Barton, and W. Moore ; the Rev. Dean Watson and B. Delanny, were raised to Fellows. —"Historical Documents of 1644" were exhibited by the hon. sec., one of which contained a key to the cipher used in the correspondence between Ormonde and the confederate leaders at the time. The following papers were read : "On a recent discovery of Coins at Mullaboden, Ballymore Eustace, co. Kildare," by Rev. J. F. Shearman ; "On Kilkenny, past and present," by P. Watters ; "On some Unrecorded Antiquities in Yar-Connaught," by G. H. Kinahan ; "On some Antiquities of Oak at Bellisle, co. Fermanagh," by W. F. Wakeman.

BOOKS RECEIVED

English.—Principles of Geology, 11th edition, Vol. i. : Sir C. Lyell (J. Murray).—Scottish Meteorology, 1855-1871, Edinburgh Observatory.—A Treatise on the Theory of Friction : J. H. Jellett (Macmillan).—The Climate of Uckfield : C. L. Prince (Churchill).

America.—Transactions of the Albany Institute, Vols. 1-6.—Transactions of the Society for the Promotion of Useful Arts in the State of New York, Vol. iv., Part II.—Annals of the Dudley Institute, Vols. i. and ii.—Annual Address before the Albany Institute : O. Meads.—The Advice of a Father to his Son : N. François.

DIARY

THURSDAY, February 29.

Royal Society, at 8.30.—On the relative Power of 34 Substances to Prevent the Development of Protoplasmic and Fungus Life, and in Arresting Putrefaction : Prof. Crace-Calvert, F.R.S.

Society of Antiquaries, at 8.30.—Further Facts in the History of the Early Discovery of Australia : R. H. Major, F.S.A.

FRIDAY, March 1.

Royal Institution, at 9.—Measuring Temperature by Electricity : C. W. Siemens.

Geologists' Association, at 8.—On the Geology of Hampstead, Middlesex : C. Evans, F.G.S.—Note on a recently exposed Section at Battersea : J. A. Coombs.

Archæological Institute, at 4.

SATURDAY, March 2.

Royal Institution, at 3.—Demonology : M. Conway.

SUNDAY, March 3.

Sunday Lecture Society, at 4.—On the Icelandic Language and its similarity to English. The Literature of Iceland, Old and Modern : Jon A. Hjaltalin.

MONDAY, March 4.

Entomological Society, at 7.

Anthropological Institute, at 8.—Anthropological Collections from the Holy Land, No. III. : Capt. R. F. Burton and Dr. C Carter Blake.—Race Characteristics as related to Civilisation : J. Gould Avery.

London Institution, at 4.—Elementary Chemistry : Prof. Odling, F.R.S.

Royal Institution, at 2.—General Monthly Meeting.

TUESDAY, March 5.

Zoological Society, at 9.—Notes on an Ostrich, recently living in the Society's collection : A. H. Garrod.—Catalogue of the Birds found in Ceylon, with some remarks on their habits and local distribution, and descriptions of two new species peculiar to the Island : E. W. H. Holdsworth.

Society of Biblical Archæology, at 8.30.

Royal Institution, at 3.—On the Circulatory and Nervous Systems : Dr. Rutherford.

WEDNESDAY, March 6.

Geological Society, at 8.—On *Prognathus Güntheri* (Egerton), a new genus of Fossil Fish from the Lias of Lyme Regis ; On two Specimens of Ischyodus from the Lias of Lyme Regis : Sir P. de M. Grey-Egerton, Bart., M.P., F.R.S.—How the Parallel Roads of Glen Roy were formed : Prof. James Nichol, F.G.S.—Notes on Atolls or Lagoon Islands : S. J. Whitnell.

Society of Arts, at 8.—On the *Goliath* Training Ship : Capt. Bourchier.

Microscopical Society, at 8.

Pharmaceutical Society, at 8.

THURSDAY, March 7.

Royal Society, at 8.30.

Society of Antiquaries, at 8.30.

Royal Institution, at 3.—On the Chemistry of Alkalies and Alka Manufacture : Prof Odling, F.R.S.

Linnean Society, at 8.

Chemical Society, at 8.

CONTENTS

NOTICE

We beg leave to state that we decline to return rejected communications, and to this rule we can make no exception. Communications respecting Subscriptions or Advertisements must be addressed to the Publishers, NOT *to the Editor.*

THURSDAY, MARCH 7, 1872

A FRENCH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

IN France there is at the present time a movement of regeneration in the scientific world, slow indeed, and difficult to be seen through the troubles on the surface, but the evidence of it is incontestable. The actual activity is great; publications of every kind appear, some quite new, as the *Journal de Physique*, the *Archives de Zoologie;* others improved and extended, as the *Annales de l'Ecole Normale*. The *Comptes Rendus* of the Paris Academy, which are the weekly *résumé* of French science, have rarely been so full of important memoirs, while research, almost dead to England, promises regeneration for French science.

To take a recent example of this activity, we may cite about fifty notices relative to the aurora borealis of last month, coming from every part of France. This amount of attention paid to a phenomenon which, a few years ago, would have excited nothing more than a mere curiosity, evidences the actual aspirations connected with, and a natural taste for, scientific subjects. But what must specially strike the English scientific world is the recent foundation of a French Association for the Advancement of Science, on the model of the British Association, without any other modifications than those which must result from the different characters of the two nations.

Though this proposal has not reached its complete extension (no publicity having been as yet given to it) it is possible, from the rapidity with which the working committee was constituted, the large amount of money collected, and the sympathies expressed on all sides, to predict for the younger sister of the British Association a great success.

The proposed statutes, which have been drawn up and provisionally adopted at a series of meetings at which MM. Balard, Berthelot, Briot, Broca, Claude Bernard, Combes, Cornu, Decaisne, Delaunay, Descloiseaux, De Luynes, Dumas, Friedel, P. Gervais, A. Girard, G. Hachette, Lacaze-Duthiers, Laugier, Levasseur, Loewy, Marié-Davy, V. Masson, Pasteur, Serret, Tisserand, and Wurtz were present, are as follows :—

ART. I.—The Association proposes to favour by every means in its power the progress of the sciences, their practical application, and the diffusion of scientific knowledge. For this purpose it will exercise its influence principally by meetings, conferences, and publications; by gifts of instruments or money to persons engaged in researches, observations, or experiments, scientific enterprises which it would have approved or provoked. It appeals to all those who consider the culture of the sciences as necessary to the greatness and prosperity of France.

ART. II.—The Association is established with a capital divided into shares of 500 francs each, subscribed by members who take the title of founders. It will commence its operations as soon as 200 of these shares, forming a capital of 100,000 francs, shall have been subscribed.*

ART. III.—The Association shall consist of founders and ordinary associates, who shall pay an annual subscription of 20 francs. This subscription can always be compounded by the payment of the sum of 200 francs once for all.

ART. IV.—The number of founders or associates is unlimited, and all enjoy the same privileges. The names of the founders shall, however, always appear at the head of the lists, and these members receive gratuitously and for ever all the publications of the Association, as many copies as they have subscribed shares of 500fr.

ART. V.—The seat of the Council of the Association shall be at Paris.

ART. VI.—Each year the Association shall hold in one of the towns of France a general session, the duration of which shall be eight days.

ART. VII.—In the general session the Association shall be divided into sections, of which the number and functions shall be fixed by the general assembly on the proposition of the Council. These sections shall be attached to the four groups of Mathematical, Physical and Chemical, Natural, and Economical and Statistical science. Every member of the Association shall choose each year the section to which he wishes to belong. He can nevertheless take part in the work of the other sections, but only with consultative voice (*voix consultative.*)

ART. VIII.—The bureau of the Association is composed :—1, of the president and secretary; 2, of the presidents and secretaries of sections; 3, of the treasurer and the librarian.

ART. IX.—The Association shall be managed gratuitously by a Council composed—1, of the bureau of the association; 2, of members elected in the general assembly to the number of three by each section.

ART. X.—At the commencement of each session the presidents, vice-presidents, and secretaries of the sections are nominated directly by a relative majority of the sections.

At the end of each session, the Association, united in general assembly, shall name the town where the following session shall take place, fix a programme for that session, and nominate by relative majority the president and secretary for the following year, and also the members of the Council.

The president and secretary shall be taken in turn from each of the four sections. If either is prevented from attending, he shall be replaced by one of the presidents or secretaries of the divisions of the section to which he may belong.

ART. XI.—The Council charged with the organisation of the session in the town selected can for that purpose elect an honorary president.

ART. XII.—All members of the Association are asked to take part in elections by voting either in person or by letter (*par correspondence*).

ART. XIII.—The Council represents the Association. It has full power to carry on and administer the social business, both active and passive. It shall receive all funds belonging to the Association, of whatever kind they may be. It shall invest in Government securities the funds arising from the shares subscribed by the founders, and from the compounding of annual subscriptions by the associates. It shall superintend the expenditure of the disposable funds voted by the Association on its proposal. It shall make all rules necessary for maintaining internal order and the execution of the present statutes. It shall convoke the Association, and arrange the programme of the meeting, in conformity with deliberations made in the general assembly.

The Council shall nominate and constitute the special committees for the funds for encouragements, for publications, and for conferences.

The Council shall deliberate in due form and by the majority of members present; nevertheless no resolution shall be valid unless it shall have been deliberated upon in the presence of one-fourth, at least, of the members of the Council.

ART. XIV.—The Council shall prepare annually the budget of expenses of the Association, and shall read in the annual general session a detailed account of receipts and expenses of work themselves (*de l'enercice écoulé*).

ART. XV.—The statutes can be modified on the proposition of the Council, and by a majority of two-thirds voting in the general assembly. The proposed modifications shall be indicated beforehand in the Convocatory letters addressed to all members of the Association.

PROPOSED SECTIONS

1st Section—*Mathematical Science*

1st, Division of Mathematics, Astronomy, and Geodetical

* This amount has been exceeded some weeks since. It was subscribed by scientific men, and by the greater number of the councils of the railway, industrial, and financial companies.

Science; 2nd Mechanics; 3rd, Navigation; 4th, Civil and Military Engineering.

2nd Section—*Chemical and Physical Science*

5th, Physics; 6th, Chemistry; 7th, Meteorology and Physics of the Globe.

3rd Section—*Natural Sciences*

8th, Zoology and Zootechny; 9th, Botany; 10th, Geology and Mineralogy; 11th, Medicine.

4th Section—*Economic Sciences*

12th, Agriculture (*Agronomie*); 13th, Ethnography and Geography; 14th, Statistics.

We are told that certain modifications may be introduced before the final constitution of the Association, but they are not likely to change the general character of the institution.

Amongst the promoters of this Association are to be found the names most celebrated in French science, showing that this scientific movement is a general one, and answers to a pressing want.

A peculiar feature will be remarked: the general spirit of the statutes denotes a very decided tendency to *decentralisation.* Up to the present time French science has had the reverse tendency,—to attract to Paris all the intelligent strength of the nation. The result, most excellent for Paris, which constitutes one of the greatest scientific centres in the world, has been very disadvantageous for the country. The provincial *facultés* (local universities) have been deprived of the most important of their members, and are actually very far from answering to the scientific standard of the metropolis. If we add now that the *Ministère de l'Instruction Publique* not only has insufficient funds for these institutions of higher instruction, but considers the *facultés* as sources of revenue by the granting of degrees, it will be understood that it is the right time to act vigorously to raise the taste for science in the parts of the country remote from the capital.

Too much encouragement cannot be given to the founders of the French Association in their task of decentralising science in France. The first result will be to create real scientific centres, vigorous with a new life, and diffusing a great activity. It cannot be objected that the genius of the nation is opposed to such a decentralisation, and that all aspirations must necessarily converge towards Paris. This is an error. The town of Montpellier gives the example of a Faculté de Médecine, of which the reputation is scarcely inferior to the Paris *faculté*. It is equally certain that Toulouse the town of *Jeuxfloraux*, Lyons, Marseilles, Clermont, and many others, under a vigorous impulse, could also become great scientific centres. To aim at this object, nothing will be better than to show every year the whole scientific *corps* of Paris, the scientific *Etat-major* transporting itself to a remote city or town, liberally giving lectures and conferences, and promoting researches and experiments. Thus the metropolis will greatly help the scientific renovation, and will show that it wishes not to attract to itself the whole force and consideration, but to diffuse its own energy over the whole country.

It is probable that the first meeting will be held this year at Lyons, the second town of France, at the end of August or the beginning of September. To the interest of the meeting would be added the attraction of a great industrial exhibition.

We cannot do otherwise than wish a great success to the French Association. We are happy to see that all parties are uniting in their exertions in such a direction; that a good number of associates, independent or belonging to scientific societies, are giving in their adhesion to the new association. Amongst the congratulations which the Association ought to receive at its birth, no doubt one of the first will be addressed by the British Association. This will be for England both a duty and an honour. A nation must always be happy to be valued and proud to be imitated.

QUETELET'S CONTRIBUTIONS TO THE SCIENCE OF MAN

Physique Sociale, ou Essai sur le Développement des Facultés de l'Homme. Par Ad. Quetelet. (Brussels, 1869.)

Anthropométrie, ou Mesure des différentes Facultés de l'Homme. Par Ad. Quetelet. (Brussels, 1870.)

TWO lines of research into the Science of Man, of the highest moment as well in theoretical Anthropology as in practical Ethics and Politics, both to be always associated with the name of Quetelet, are now discussed at large in his Social Physics and Anthropometry. The two great generalisations which the veteran Belgian astronomer has brought to bear on physiological and mental science, and which it is proposed to describe popularly here, may be briefly defined. First, he has been for many years the prime mover in introducing the doctrine that human actions, even those usually considered most arbitrary, are in fact subordinate to general laws of human nature; this doctrine, maintained in previous publications, especially in the earlier edition of the first-named work some thirty-seven years ago, is now put forth in its completest form. Second, he has succeeded in bringing the idea of a biological type or specific form, whether in bodily structure or mental faculty, to a distinct calculable conception, which is likely to impress on future arguments a definiteness not previously approached.

The doctrine of the regularity and causality of human actions was powerfully stated some fifteen years ago by Mr. Buckle in the introduction to his "History of Civilisation." Buckle is here essentially the exponent of Quetelet's evidence, from which, indeed, as a speculative philosopher he draws inferences more extreme than those of his statistical teacher. To Quetelet is due the argument from the astonishing regularity from year to year in the recurrences of murders and suicides, a regularity extending even to the means or instruments by which these violent acts are committed; his inference being broadly "that it is society which prepares the crime, the criminal being only the instrument which executes it." From various other sources Buckle brought together other pieces of evidence, especially one which is now quoted by all who discuss the subject, the regularity from year to year of letters posted, whose writers forget to direct them. It may by this time be taken as proved by such facts that each particular class of human actions may be estimated, and to a great extent even predicted, as a regular product of a definite social body under definite conditions. To quote another luminous instance of this regularity of action, M. Quetelet gives a table of the ages of marriage in Belgium (Phys. Soc. i. p. 275). Here the numbers of what may be called normal marriages, those between men under 45 with women under 30, as well as of the less usual unions where the women are between 30 and 45, show the sort of general regularity which one would expect from mere consideration of the circumstances. The astonishing feature of the table is the regularity of the unusual marriages. Disregarding decimals, and calculating the approximate whole numbers in their proportion to 10,000 marriages, the table shows in each of five five-year periods from 1841 to 1865, 6 men aged from 30 to 45 who married women aged 60 or more, and 1 to 2 men aged 30 or less who married women aged 60 or more. M. Quetelet may well speak of this as the most curious and suggestive statistical document he has met with. These young husbands had their liberty of choice, yet their sexagenarian brides brought them up one after the other in periodical succession, as sacrifices to the occult tendencies of the social system. The statistician's comment is, "it is curious to see man, proudly entitling himself King of Nature and fancying himself controlling all things by his free-will, yet submitting, unknown to himself, more rigorously than any other being in creation, to the laws he is under subjection to. These laws are co-ordinated with such wisdom that they even escape his attention."

The admission of evidence like this, however, is not always followed by the same philosophical explanation of it. Buckle finds his solution by simply discarding the idea that human action "depends on some capricious and personal principle peculiar to each man, as free-will or the like;" on the contrary, he asserts "the great truth that the actions of men, being guided by their antecedents, are in reality never inconsistent, but, however capricious they may appear, only form part of one vast scheme of universal order, of which we, in the present state of knowledge, can barely see the outline." M. Quetelet's argument from the same evidence differs remarkably from this. His expedient for accounting for the regularity of social events without throwing over the notion of arbitrary action, is to admit the existence of free-will, but to confine its effects within very narrow bounds. He holds that arbitrary will does not act beyond the limits at which science begins, and that its effects, though apparently so great, may, if taken collectively, be reckoned as null, experience proving that individual wills are neutralised in the midst of general wills (p. 100). Free-will, though of sufficient power to prevent our predicting the actions of the individual, disappears in the collective action of large bodies of men, which results from general social laws, which can accordingly be predicted like other results regulated by natural laws. We may perhaps apprehend the meaning of Quetelet's views more clearly from another passage, where, to show how apparently isolated events may be really connected under some wide law, he compares single facts to a number of scattered points, which seem not related to one another till the observer, commanding a view of a series of them from a distance, loses sight of their little accidents of arrangement, and at the same time perceives that they are really arranged along a connecting curve. Then the writer goes on to imagine, still more suggestively, that these points might actually be tiny animated creatures, capable of free action within a very narrow range, while nevertheless their spontaneous movements would not be discernible from a distance (p. 94), where only their laws of mutual relation would appear. M. Quetelet can thus conciliate received opinions by recognising the doctrine of arbitrary volition, while depriving it of its injurious power.* His defence of the existence of free-will is perhaps too much like the famous excuse of the personage who was blamed for going out shooting on the day he had received the news of his father's death, and who defended himself on the ground that he only shot very small birds. But it is evident that the statistics of social regularity have driven the popular notion of free-will into the narrow space included between Quetelet's restriction and Buckle's abolition of it. In fact, no one who studies the temper of our time will deny the increasing prevalence of the tendency of the scientific world to reject the use of the term free-will in its vulgar sense, that of unmotived spontaneous election, and even to discourage its use in any other sense as apt to mislead, while its defenders draw their weapons not so much from observation of facts as from speculative and dogmatic philosophy.

To those who accept the extreme principle that similar men under similar circumstances must necessarily do similar acts; and to those, also, who adopt the notion of free-will as a small disturbing cause which disappears in the large result of social law, the regularity of civilised life carries its own explanation. Society is roughly homogeneous from year to year. Individuals are born, pass on through stage after stage of life, and die; but at each move one drops into another's place, and the shifting of individuals only brings change into the social system, so far as those great general causes have been at work which difference one age from another, the introduction of different knowledge, different principles, different arts, different industrial materials and outlets. The modern sociologist, whatever his metaphysical prepossessions, looks at society as a system amenable to direct cause and effect. To a great extent his accurate reckonings serve to give more force and point to the conclusions of rough experience; to a great extent, also, they correct old ideas and introduce new aspects of social law. What gives to the statistical method its greatest scope and power is that its evidence and proof of law applies indiscriminately to what we call physical, biological, and ethical products of society, these various effects acting and re-acting on one another. A few instances may be given to show the existence of the relations in question, without attempting to show their precise nature, nor to trace the operation of other determining causes.

Thus, for instance, the mode of life affects its length Statistics show that the mortality of the very poor is about half as much again as the mortality of the very rich; while as to the influence of professions, it appears that in Germany only twenty-four doctors reach the age of seventy as against thirty-two military men and forty-two theologians. The propensity to theft bears a distinct relation to age; thus the French criminal statistics estimate the propensity to theft between the ages of twenty-one and twenty-five, as being five-thirds as much as between the ages of thirty-five and forty. The

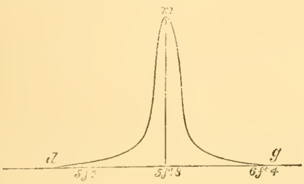
* In regard to the relation of statistics to the doctrine of fatalism, see Dr. Farre's "Report on the Programme of the Fourth Session of the Statistical Congress."

amount of criminality in a country bears a relation, indirect and as yet obscure, but unmistakeable, to its education, or rather, to its want of education. In France, in 1828-31, the constant percentage of accused persons was about as follows: could not read or write, sixty-one; imperfectly, twenty-seven; well, twelve. The comparison of this group of numbers with those taken lately in England shows a great change of proportion—evidently resulting from the wider diffusion of education; but the limitation of crime to the less-educated classes is even more striking: cannot read or write, thirty-six; imperfectly, sixty-one; well, three. Again, for an example of connection of physical conditions with moral actions, we may notice a table, showing how the hours of the day influence people who hang themselves. (Phys. Soc. ii. 240.) The maximum of such cases, 135, occurred between six and eight in the morning; the number decreased slightly till noon, and then suddenly dropped to the minimum; there being 123 cases between ten and twelve o'clock, against only 32 between twelve and two o'clock. The number rose in the afternoon to 104 cases between four and six, dropping to an average of about 70 through the night, the second minimum, 45, being between two and four o'clock in the morning. Here it is impossible to mistake the influences of the periods of the day; we can fancy we see the poor wretches rising in the morning to a life of which the misery is beyond bearing, or can only be borne till evening closes in; while the temporary relief of the midnight sleep and the midday meal are marked in holding back the longing to self-destruction. Madness varies with the season of the year: the maximum being in summer, and the minimum in winter (p. 187); a state of things which seems intelligible enough. Again, it is well known in current opinion that more children are born in the night than in the day; in fact, there are about five night-born against four day-born, the maximum being about midnight, the minimum a little before noon (i. p. 208). Why this is no one yet knows; it is a case of unexplained law. But another not less curious law relating to births seems to have been at last successfully unravelled. In Europe about 106 boys are born to every 100 girls. The explanation appears to depend on the husband being older than the wife; which difference again is regulated by prudential considerations, a man not marrying till he can maintain a wife. In connection with this argument it must be noticed that illegitimate births show a much less excess of male children (p. 168). Here, then (if this explanation may be accepted), it appears that a law which has been supposed to be due to purely physiological causes is traceable to an ultimate origin in political economy.

The examples brought forward by Quetelet, which thus show the intimate relation between biological and ethical phenomena, should be pondered by all who take an interest in that great movement of our time—the introduction of scientific evidence into problems over which theologians and moralists have long claimed exclusive jurisdiction. This scientific invasion consists mainly in application of exact evidence in place of inexact evidence, and of proof in place of sentiment and authority. Already the result of the introduction of statistics into inquiries of this kind appears in new adjustments of the frontier line between right and wrong, as measured under our modern social conditions. Take, for instance, the case of foundling hospitals, which provide a "tour," or other means for the secret reception of infants abandoned by their parents. It has seemed and still seems to many estimable persons an act of benevolence to found and maintain such institutions. But when their operation comes to be studied by statisticians, they are found to produce an enormous increase in the number of exposed illegitimate children (Phys. Soc. i. 384). In fact, thus to facilitate the safe and secret abandonment of children is to set a powerful engine at work to demoralise society. Here, then, a particular class of charitable actions has been removed, by the statistical study of its effects, from the category of virtuous into that of vicious actions. An even more important transition of the same kind is taking place in the estimation of almsgiving from the ethical point of view. Until modern ages, through all the countries of higher civilisation, men have been urged by their teachers of morality to give to the poor, worthy or unworthy; the state of public opinion being well exemplified by the narrowing of the word "charity" from its original sense to denote the distribution of doles. Yet, when the statistics of pauperism were collected and studied, it was shown that indiscriminate almsgiving is an action rather evil than good, its tendency being not only to maintain, but actually to produce, idle and miserable paupers. In our time a large proportion of the public and private funds distributed among the poor is spent in actually diminishing their industry, frugality, and self-reliance. Yet the evil of indiscriminate almsgiving is diminishing under the influence of sounder knowledge of social laws, and genuine charity is more and more directed by careful study of the means by which wealth may be spent for the distinct benefit of society. Such examples as these show clearly the imperfection and untrustworthiness of traditional, or what is called intuitive morality, in deciding on questions of right and wrong, and the necessity of appealing in all cases to the best attainable information of social science to decide what actions are really for or against the general good, and are therefore to be classed as virtuous or vicious.

Moreover, it is not too much to say that the comparatively small advance which moral science has made since barbaric ages has been due to the repugnance of moralists to admit, in human action, the regular causality which is the admitted principle of other parts of the action of the universe. The idea of the influence of arbitrary will in the individual man has checked and opposed the calculations which now display the paramount action of society as an organised whole. One point in M. Quetelet's doctrine of society requires a mention for its practical bearing on morals. There has seemed to some to be an immoral tendency in his principle that virtuous and vicious acts are products, not merely of the individual who does them, but of the society in which they take place, as though the tendency of this view were to weaken individual responsibility, and to discourage individual effort. Yet, when properly understood, this principle offers a more strong and definite impulse to the effort of society for good and against evil, than the theory which refers the individual's action more exclusively to himself. M. Quetelet's inference from the regular production of a certain amount of crime year by year from a society in a certain condition, is embodied in his maxim that society prepares the crime and the criminal executes it. This should be read with a comment of the author's. "If," he says, "I were to take up the pavement before my house, should I be astonished to hear in the morning that people had fallen and hurt themselves, and could I lay the blame on the sufferers, inasmuch as they were free to go there or elsewhere?" Thus every member of society who offers a facility to the commission of crime, or does not endeavour to hinder its commission, is, in a degree, responsible for it. It is absurd to suppose that the crimes in great cities are attributable altogether to the free agency of the poor wretches who are transported or hung for them. The nation which can and does not prevent the existence of a criminal class is responsible collectively for the evil done by this class. This we can see plainly enough, although the exact distribution of the responsibility among the different members of society may be impossible to determine. Such a theory, of course, casts aside the revenge-theory of criminal law, assimilating the treatment of criminals to the operation of a surgeon healing a diseased part of the body, if possible, or, if not, rendering it harmless or removing it.

The wealthy and educated classes, whose lives seem to themselves as free from moral blame as they are from



legal punishment, may at first hear with no pleasant surprise a theory which inculpates them as sharers in the crimes necessarily resulting from the state of society which they are influential in shaping. Yet this consideration is by no means one of mere hopeless regret, for coupled with it is the knowledge that it is in their power, by adopting certain educational and reformatory measures, so to alter the present moral status of society as to reduce the annual budget of crime to a fraction of its present amount. Thus the doctrine that the nation participates in and is responsible for the acts of its individual members is one which widens the range of duty to the utmost. The labours of M. Quetelet in reducing to absolute calculation this doctrine of the solidarity of human society, entitle him to a place among those great thinkers whose efforts perceptibly raise that society to a higher intellectual and moral level. Here, as everywhere, the larger comprehension of the laws of nature works for good and not for evil in the history of the world.

Some slight account has now to be given of M. Quetelet's doctrine of typical forms, as displayed in the "homme moyen," or "mean man," of a particular nation or race. This is no new theory; but since the publication of the "Physique Sociale" in 1835, the author has been at work extending and systematising it, his last results being shown in the present works. First, it must be pointed out that the term "homme moyen" is not intended to indicate what would be popularly meant by an "average man." An average or arithmetical mean of a number of objects may be a mere imaginary entity, having no real representative. Thus, an average chessman, computed as to height from the different pieces on the board, might not correspond to any one of the actual pieces. But the "homme moyen" or central type of a population really exists; more than this, the class he belongs to exceeds in number any other class, and the less nearly any other class approaches to his standard the less numerous that class is, the decrease in the number of individuals as they depart from the central type conforming to a calculable numerical law. The "mean man" (the term may probably be adopted in future researches, and when technically used its popular meaning will cease to interfere with this special one)—the "mean man" thus stands as a representative of the whole population, individuals as they differ from him being considered as forms varying from his specific type.

To realise a conception which even among anthropologists has scarcely yet become familiar, it is desirable to show by what actual observations M. Quetelet was led to the discovery of his principle. When a large number of men of a practically homogeneous population are measured, and arranged in groups accordingly, it becomes evident that the individuals are related to one another by a law of distribution. A central type is represented by the most numerous group, the adjoining groups becoming less and less numerous in both directions. Thus, on classifying the measured heights of some 26,000 American soldiers of the Northern army during the late war, the proportionate number of men to each height was ascertained to be as follows (Phys. Soc. i. p. 131; Anthropom. p. 259):—

Height, inches	60	61	62	63	64	65	66	67	68
No. of men in 1,000 .	1	1	2	20	48	75	117	134	157
Height, inches	69	70	71	72	73	74	75	76	
No. of men in 1,000 .	140	121	80	57	26	13	5	2	

Here it is seen that the mean man is a little under 5 ft. 8 in. in height, the numbers of men shorter and taller diminishing with evident regularity, down to the few representatives of the very short men of 5 ft. and under, and the very tall men of 6 ft. 4 in. and over. The law of relation of height to numerical strength is shown graphically by the binomial curve figured above, where the abscissæ (measured from an origin on the left) represent the heights of the men, and the ordinates the relative numbers of men corresponding to each height. The maximum ordinate, representing the number of mean men, is at m = about 5 ft. 8 in., the ordinates on both sides diminishing almost to nothing as they reach the dwarfish and gigantic limits d and g, and vanishing beyond.

Again, measurement round the chest, applied to the soldiers of the Potomac army, shows a similar law of grouping (Phys. Soc. ii. 59; Anthropom. p. 289).

Round chest, inches	28	29	30	31	32	33	34	35
No. of men in 1,000 . . .	1	3	11	36	67	119	160	204
Round chest, inches	36	37	38	39	40	41	42	
No. of men in 1,000 . . .	166	119	68	28	13	4	1	

Here the mean man measures about 35in. round the chest, the numbers diminishing both ways till we reach the few extremely narrow-chested men of 28in., and the few extremely broad-chested men of 42in. These two examples may represent the more symmetrical cases of distribution of individuals on both sides of a central type, as worked out by M. Quetelet from various physical measurements applied to large numbers of individuals. Here the tendency to vary is approximately equal in both directions. Where the tendency to vary is perceptibly different in the two directions the curve loses its symmetry, as in the figures representing the weights of women at different ages (Anthropom. p. 349), and the number of marriages of men and women at different ages (Phys. Soc. i. 272). The actual series of numbers given by observation are placed beside series computed according to the law of the expanded binomial, the same which is applied in the theory of probabilities to such calculations as that of the proportionate distribution of less probable events on each side of a most probable maximum term, the distribution of errors of observation of a single object, and of accidental variations in general. It is the closeness of approximation between the observed and calculated series of variations, computed not only as to the dimensions, but the actions of man, which gives to M. Quetelet's theory its remarkable definiteness and precision.

The diagram of statures here figured, which may be looked upon as representing a nation measured in one particular way, at once impresses on the mind a conception of a race-type materially differing from the vague notions hitherto current. It is seen that individual men of different statures are required to constitute a nation, but they are required in less and less proportion as they depart in excess or defect from the central type. The nation is not even complete without its dwarfs and giants. In fact, if all the monstrously short and tall men of a particular country were put out of sight, and the census of the population taken according to stature, the national formula thence deduced would enable a statistician to reckon with considerable accuracy how many dwarfs and giants of each size had been removed.

M. Quetelet's investigations further prove, or tend to prove, that similar laws of variation from the central type govern the distribution of individuals classed according to other bodily dimensions, and also according to physical qualities such as weight and strength, it being borne in mind that the particular expressions with their descriptive curves differ for the various qualities or faculties of man, being also in some cases much less symmetrical than in others. An absolute coincidence of the series of observed facts with the numerical law chosen to express them would be too much to expect; it is a great deal to obtain even a rough coincidence. For instance, when the strength of a number of men is estimated by a dynamometer, the maximum number showed 140 to 150 degrees on the scale, the number of weaker and stronger men being both fewer from this point, groups following approximately the proportions of the coefficients of a binomial of the 6th order; the numbers are reduced as follows from the table (Anthropom. p. 365):—

Renal force, degrees	90	100–110	120–130	140–150	160–170
No. of men in 64	1	8	14	20	15
Binom. coeff.	1	6	15	20	15

Renal force, degrees	180–190	200
No. of men in 64	6	1
Binom. coeff.	6	1

In the various numerical examples here given, the element of age is not introduced, the ages of the individuals being calculated or taken as uniform. The problem of variation of numerical distribution of a population at different ages is treated by M. Quetelet in a comparatively simple case, that of the stature-curve. Here a curve approximating to a parabola is laid down, the ages of man from birth onward being measured along its axis; each double ordinate of this curve forms the base on which a binomial curve is erected perpendicularly, the vertices of these curves forming a curve of mean stature, of the nature of a curve of mortality (Anthropom. p. 264). How far M. Quetelet may succeed in his contemplated purpose of carrying his method from the physical into the intellectual and moral nature of man, it is premature to judge.

Without entering into the more intricate and difficult problems opened by this theory of central types, it is evident that the bearing of its main conception on the problems of anthropology and biology in general is highly important. Some able anthropologists have accepted the theory of the mean or central standard as a basis for the comparison of races, but this line of research is still in its infancy. In M. Quetelet's last volume a principle is worked out which serves as a bridge between the old and new methods. His experience is that in a well-marked population no extraordinary number of observations is required for the determination of the mean man. In former ages, one result of the national type being so preponderant in number and so easily recognisable was that the bodily measurements of any man of ordinary stature and proportions could be trusted to give, with reasonable accuracy, the standard measures of the nation, such as the foot, cubit, fathom, &c. In the same manner M. Quetelet finds a small number of selected individuals sufficient for ascertaining the standard national proportions of the human body, male and female, from year to year of growth; his tables, founded for the most part on Belgian models, are given in an appendix. This method is applicable to the purposes of general anthropology. Thus a traveller, studying some African or American race, has to select by mere inspection a moderate number of typical men and women, by comparison of whose accurately admeasured proportions he may approximate very closely to a central race-type.* It is not necessary to dwell on the obvious difficulties of connecting the standard types of mixed nations with the races composing them. The stature-curve of England differs visibly in proportions from that of Italy, the measurements of Scotch and American soldiers show very different mean and extreme terms, and the problems of race underlying these differences are of a most complex character, the more so when the consideration is introduced of the race-type varying within itself from century to century. M. Quetelet is naturally apt, when expressing his views in an exordium or a peroration, to draw a good deal on the anticipated future results of his admirable method; but in judging of the value of his doctrine of central types, the best criterion is his actual success in reducing the observed facts of nature to numerical

* Thus General Lefroy's measurements of thirty-three Chipewyan Indians ("Journal of the Ethnological Society," vol. ii. p. 44, 1870) are sufficient to determine the stature of the mean man as about 5 ft. 7 in., the number of individuals in this maximum group being 8. It is even possible to guess from this small number of measurements the numerical law of variation in the tribe, the series of groups from 5 ft. 3 in. to 5 ft. 11 in. being as follows:— 1, $1\frac{1}{2}$, $2\frac{1}{2}$, 6, 8, $4\frac{1}{2}$, $4\frac{1}{2}$, 3, 1.

calculation. The future must show how far it will be possible to apply to the theory of species the definition of central specific forms, from which varieties calculably diminish in numbers as they depart in type.

E. B. TYLOR

OUR BOOK SHELF

Magnetism. By Sir W. Snow Harris and H. M. Noad. (London: Lockwood and Co.)

THIS is a good book, and we are glad to see the subject of magnetism fully treated in a popularly written text-book. It is a second edition of Sir William Snow Harris's rudimentary treatise, with considerable and important additions by the editor. The part of chief importance which is added is Chapter viii., which deals with the more recent progress of terrestrial megnetism. This chapter consists of thirty pages, and the author has managed to condense into that space a wonderfully large amount of interesting, useful, and accurate information on the subject. In so short a space we must be content with results rather than with particulars, but the matter contained in this chapter, in point of importance, accuracy, and exhaustiveness, places the present treatise, as far as terrestrial magnetism is concerned, much before any similar book with which we are acquainted. The correction of the compass in iron ships is entered into in the last chapter. The telegraph is scarcely touched upon, but this perhaps rather belongs to a treatise on electricity. We have a chapter on theories of terrestrial magnetism. The theory of Gauss should never be classed, as it is here, and indeed as it is generally classed, along with theories like those of Halley or Hanstein, or with such things as electro-magnetic theories and the like. The word "theory" in these cases means quite a different thing from what it means when applied to Gauss's investigations. Hanstein and the like all make some physical hypothesis, which may or may not be the case; but Gauss makes no such assumption at all, except in so far as he supposes that the needle at all parts of the earth's surface is affected by forces due to the same origin, and varying inversely as the square of the distance, which has been experimentally proved to be the law according to which magnetic forces act. He then shows how the effect on a needle can be expressed in terms of an infinite series which is necessarily mathematically convergent and true, and he then uses an approximation to that series, which approximation is justified fully by experiments similar to those made by the late Prof. Forbes at the top and bottom of the Faulhorn. Gauss's theory, then, is a truly scientific theory, inasmuch as it involves no unjustified physical hypothesis, but is a logical deduction from observed facts and established principles, and in this differs radically from the other theories which are too often classed with it. Dr. Noad has been so successful in Chapter viii. that we cannot help wishing he had introduced a chapter also on this subject.

JAMES STUART

The Amateur's Flower-Garden: a Handy Guide to the Formation and Management of the Flower Garden and the Cultivation of Garden Flowers. By Shirley Hibberd. Illustrated with coloured plates and wood engravings. (London: Groombridge and Son, 1871.)

MR. HIBBERD is a practised writer on gardening subjects, though his books have not much claim to be considered as scientific treatises, but rather as pretty gift-books to lie on the drawing-room table and give to its furniture a *quasi*-scientific air. That they have their use cannot be doubted, but it is not a very high one. The worst part of this book is the illustrations. From the letterpress may be doubtless culled some useful hints as to the planting and management of a flower-garden, though we do not think it equal in this respect to some other works, such as those by Mr. Robinson, which are less under the trammels of time-honoured prejudices and superstitions. But many of the illustrations, including some of the woodcuts and nearly all the coloured plates, are simply atrocious. The drawings of a show pelargonium (p. 80), pansy (p. 45), ranunculus (p. 156), carnation (p. 117), and some others, are mere caricatures, and unworthy of a place in any work which bears the least pretensions to a scientific character.

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 Survival of the Fittest

I HAD designed sending a note to you, critical of the abstract of my paper on "The Laws of Organic Development," republished from the *American Naturalist* in one of your recent issues, before I read the remarks of Mr. Spencer in your number of February 1.

If Mr. Spencer will examine the Essay itself (for sale by McCalla and Stavely, 237, Dock Street, Phila., or Naturalists' Book Agency, Salem, Mass.*) he will find that I have there exclusively employed his phrase "Survival of the Fittest." The expression "Preservation of the Fittest," not used by Mr. Spencer, was inadvertently introduced in writing the abstract. This was done hurriedly between the sittings of the Amer. Assoc. Adv. Sci. for a reporter of the *New York Tribune*, and was subsequently printed by the *Naturalist* while I was absent on the Plains of Kansas. It therefore contains several obscurities, the result of an attempt to abridge, and a number of typographical blunders. The essay will be found to be free from these.

There being no misrepresentation of Mr. Spencer's views on this point, I notice the second objection he makes. Where, in the sentence regarding the Survival of the Fittest, I say that "this neat expression no doubt covers the case, but it leaves the origin of the fittest entirely untouched," Mr. Spencer regards my language as an "indirect statement that I" (Mr. S.) "have done nothing to explain the origin of the fittest."

It is plain enough that my remark does not apply to Mr. Spencer or to his writings, but exclusively to the doctrine of Natural Selection, and to Mr. Spencer's terse phrase, "which no doubt covers the case," *i.e.* Natural Selection (not the whole theory of Evolution). I cannot see that this language can be tortured into the interpretation Mr. Spencer places upon it, but Mr. Spencer's language decidedly implies that my statement is literally correct.

I am, however, well aware that Mr. Spencer has done more than any living man to explain the "Origin of the Fittest," and on this account in particular his name does not appear in my criticism. Another reason for its omission is that I have taken the liberty not to read his work, "The Principles of Biology," because I have suspected, from my reading of other works of this philosopher, that it is in advance of other treatises on the subject. I postponed it until, by investigation "in the shop," I should have attained to some definite views based on reasoning uninfluenced by the opinions of others, hoping to use "The Principles of Biology" thereafter in such a way as its merits and justice to its author should require.

EDWARD D. COPE

Philadelphia, Feb. 20

Ethnology and Spiritualism

THERE is only one point in Mr. Tylor's communication (NATURE, Feb. 29, p. 343) on which it seems desirable that I should say a few words, in order that I may not be supposed to assent to what I conceive to be a most erroneous view. Mr. Tylor suggests that the phenomena that occur in the presence of what are called mediums, are or may be of the same nature as the subjective impressions of persons under the influence of a powerful mesmeriser. Five and twenty years ago I was myself

* Under the title, "The Method of Creation of Organic Types."

a practised mesmeriser, and was able to produce on my own patients almost the whole range of phenomena which are exhibited in public as illustrative of "mesmerism" or "electro-biology." I carried on numerous experiments in private, and paid especial attention to the conditions under which the phenomena occur. During the last seven years I have had repeated opportunities of examining the phenomena that occur in the presence of so-called "mediums," often under such favourable conditions as to render trick or imposture simply impossible. I believe, therefore, I may lay claim to some qualifications for comparing the mesmeric with the mediumistic phenomena with especial reference to Mr. Tylor's suggestion, and I find that there are two great characteristics that broadly distinguish the one from the other.

1. The mesmerised patient never has *doubts* of the reality of what he sees or hears. He is like a dreamer to whom the most incongruous circumstances suggest no idea of incongruity, and he never inquires if what he thinks he perceives harmonises with his actual surroundings. He has, moreover, lost his memory of what and where he was a few moments before, and can give no account, for instance, of how he has managed to get out of a lecture-room in London to which he came as a spectator half an hour before, on to an Atlantic steamer in a hurricane, or into the recesses of a tropical forest.

The assistants at the *séances* of Mr. Home or Mrs. Guppy are not in this state, as I can personally testify, and as the almost invariable *suspicion* with which the phenomena are at first regarded clearly demonstrates. They do not lose memory of the immediately preceding events; they criticise, they examine, they take notes, they suggest tests—none of which the mesmerised patient ever does.

2. The mesmeriser has the power of acting on "certain sensitive individuals" (not on "assemblies" of people, as Mr. Tylor suggests), and all experience shows that those who are thus sensitive to any one operator are but a small proportion of the population, and these almost always require previous manipulation with passive submission to the operator. The number who can be acted upon without such previous manipulation is very small, probably much less than one per cent. But there is no such limitation to the number of persons who simultaneously see the mediumistic phenomena. The visitors to Mr. Home or Mrs. Guppy all see whatever occurs of a physical nature, as the records of hundreds of sittings demonstrate.

The two classes of phenomena, therefore, differ fundamentally; and it is a most convincing proof of Mr. Tylor's very slender acquaintance with either of them, that he should even suggest their identity. The real connection between them is quite in an opposite direction. It is the mediums, not the assistants, who are "sensitives." They are almost always subject to the mesmeric influence, and they often exhibit all the characteristic phenomena of coma, trance, rigidity, and abnormal sense-power. Conversely, the most sensitive mesmeric patients are almost invariably mediums. The idea that it is necessary for me to inform "spiritualists" that I believe in the power of mesmerisers to make their patient believe what they please, and that this "information" might "bring about investigations leading to valuable results," is really amusing, considering that such investigations took place twenty years ago, and led to this important result—that almost all the most experienced mesmerists (Prof. Gregory, Dr. Elliotson, Dr. Reichenbach, and many others) became spiritualists! If Mr. Tylor's suggestion had any value, these are the very men who ought to have demonstrated the subjective nature of mediumistic phenomena; but, on the contrary, as soon as they had the opportunity of personally investigating them, they all of them saw and admitted their objective reality.

ALFRED R. WALLACE

Development of Barometic Depressions

If I have misrepresented Mr. Ley's views, the misrepresentation was certainly unintentional; but after fairly considering his letter in NATURE of February 29, I am unable to see that I have misrepresented his views, so far as they are exposed in his "Laws of the Winds prevailing in Western Europe." Part II., of course, I ignored. It is not yet published; for aught I know, is not yet written; and as I have not the pleasure of a personal acquaintance with Mr. Ley, it is difficult to understand how I could be expected to express any opinion on a book which is still in the womb of the future. But as to the present work, Part I., which I read and reviewed, it is mainly occupied with instances, ingeniously worked out, in illustration of the rule which he distinctly enunciates, that revolving storms are due to the depression of the barometer caused by a heavy rain over a large area. Perhaps, in the same way, Part II. is to be mainly occupied by an examination and discussion of the still more numerous instances in which revolving storms have not followed heavy rain over a large area; and if so, I shall be glad in due time to give it my best attention. But for the present, having before me merely the author's existing work, I repeat what I have, in effect, already said, that the occasional or even frequent sequence of rain and storm does not establish between the two a relationship of cause and effect.

A very casual examination of our own registers, and those of Western Europe generally, would show that instances of rainfall quite as great as any that Mr. Ley adduces, happen very frequently without any storm following; and clearly if Mr. Ley's rule is sound, it must apply to all instances which cannot be claimed as exceptions, and that not only in our own latitudes, but in other parts of the world, and especially in those parts where the rainfall is excessive. It was certainly not "necessary" to travel to Khasia for instances of the failure of this rule; but its failure was exhibited in the most full and clear manner by a reference to that extraordinary rainfall.

Mr. Ley speaks of some "fact" relative to the Himalayas which "may be denied." I do not quite understand what fact he means. The facts I have spoken of are the "heavy and long continued precipitation," and a very great depression of the barometer." If it is either of these that he wishes to deny, I can only say that his doing so confirms my former statement that he has confined his investigations too exclusively to Western Europe. But when I spoke of the one as causing the other, it was not as stating a fact, but as suggesting a probability; whilst whether there is or is not "a region in which Ballot's rules are contravened" I am unable to say; if there is I have not discovered it, and I don't know where it is, but it is not near the Himalayas, where, so far as we know, the circuit of the wind is quite in accordance with Buys Ballot's Law, though on a scale of extreme magnitude—of such magnitude indeed that our observations do not extend to the end of it. It is curious that an author who, like Mr. Ley, writes sensibly within his professed boundaries, should have limited his studies so closely as he appears to have done; but as the remark to which I have just referred shows pretty conclusively that he has not examined into the range of the barometer in India, so the remark which he makes about the advance of cyclones "in the West Indies, *e.g.*," shows that he is strangely in the dark as to the variations of temperature in the tropical Atlantic.

The columns of NATURE are not the place to discuss at length such well-worn subjects as either Buys Ballot's law or the influence of the earth's rotation, and certainly whether the earth's rotation does or does not produce the effect attributed to it, was quite beyond the scope of my former allusion to it; but I said and repeat that its influence is not "obvious," that an argument based on it is not a "truism," and that to apply these words to a point that is at any rate doubtful is both objectionable and improper.

J. K. L.

Solar Intensity

I HAVE read with interest the criticism in your last number of Padre Secchi's Solar Intensity Apparatus. With reference to the single point of the discordant results obtained by thermometers with bulbs of different size, I would observe that I encountered a similar difficulty some years ago in investigating the adaptability of the instrument invented by Herschel, commonly called the "black bulb *in vacuo*," to regular comparable meteorological observations. I found that the large bulbs always gave a higher reading than the small bulbs. I supposed this to proceed from the colder stem depriving the blackened bulb of its heat, the larger bulb, of course, losing less than the smaller, and I overcame the difficulty entirely by having about an inch of the stem as well as the bulb coated with lamp-black. I am not sure, however, that this would answer so well in a non-exhausted chamber. Probably a small bulb will always be cooled by convection more rapidly than a large one.

In the excess of the temperature indicated by the improved instruments I have referred to over the temperature of the air, at the same height—usually 4ft.—above the soil (which is also very nearly the temperature of the outer glass in which the blackened thermometer is enclosed), we have not indeed an absolute measure of solar intensity; for all measures of that must, it would seem, depend on the substance exposed and the conditions as to cooling, &c., under which the exposure takes place, but a sensitive test by which slight variations in its amount can be determined, and the amount at different places and different times compared.

F. W. STOW

The Aurora of February 4

THE following is an account of the aurora of February 4 as seen by a gentleman living in Russia, at Anspatti, in the province of Vitebsk. After stating that the barometer had risen very high (30·2), he says:—"To-night, as I drove home from Reugarten, there was the most beautiful aurora borealis I ever saw. It began in the north-west, and gradually rose higher and higher, till at last it reached the horizon a little north of east, and such a broad band, or rather succession of bands, that it covered half the heavens. It was a bright rose colour, and its light and colour were reflected by the snow, so that the whole earth was rosy; though it was between nine and ten o'clock, and there was no moon, it was nearly as light at day. It is still in full force as I am writing, and I can see it from my window, but it constantly changes its form and colour." I think the latitude of the place is 56 or 57.

J. M. H.

Aurora Island

NATURE for May 25 (which has only just reached this part of the world) contains a note respecting the reported disappearance of Aurora Island in the New Hebrides. In that note the small upraised coral island of that name north-east of Tahiti is confounded with Aurora—a high volcanic island—more than 40° to the west of the former. It is scarcely to be wondered at that the mistake should be made when the name of the Island is alone given; but when "Aurora Island, one of the New Hebrides group," is spoken of as being to the "north-eastward" of the well-known island of Tahiti one feels surprised at the misconception.

Has it yet been clearly defined to which Aurora the report refers, and is it not more probable that the captain's chronometer was out, or that his reckoning was incorrect, than that either island has really been submerged? A few months ago Dr. George Bennett, F.L.S., of Sydney, New South Wales, showed me a sketch which he made of Aurora in the New Hebrides some years ago. From that the island appears very mountainous, and the map of Melanesia, in Petermann's Geographische Mittheilungen (1870), makes it about twenty miles long and 2,000ft. high.

S. J. WHITMEE

Samoa, South Pacific, Nov. 4, 1871

P.S.—The following notes of earthquakes in the Samoa group may be of interest to some of your readers:—

May 14, 1871.—2.5 P.M. First a vertical, followed by a horizontal, shock.
July 1, ,, —9.30 A.M. Slight horizontal shock.
,, 16, ,, —12.10 P.M. Vertical shock.
Aug. 3, ,, —12.15 P.M. Slight horizontal shock, accompanied by a loud rumbling noise.
Sept. 23, ,, —6.45 A.M. A slight horizontal shock.

I was absent from Samoa from September 1870 to April 1871. During that time there were eight shocks of earthquake in the group; but the dates and other particulars were not noted. One is reported as having been the most severe shock known here. Earthquakes have been more frequent in Samoa for the past year or two than formerly.

FOUL AIR IN MINES AND HOW TO LIVE IN IT

I.

I BEG to forward you for publication in NATURE an account of some very interesting experiments recently made at Chatham, on the employment of a respirator in military mining. They were conducted in a thoroughly practical manner by Mr. J. Edward Gibbs, a highly intelligent young officer of Engineers, who, I may add, has given the respirator a very convenient form, and, I trust, will continue the work he has so well begun.

It is to be borne in mind that the cotton wool employed in the respirator is not to be *steeped* in glycerine, but *moistened* with this substance; the wool ought to be well teased until all its fibres are wettedb, ut they must not form a clot.

JOHN TYNDALL

"When on duty at the Defensive Mines one day during the mining operations of July and August 1871, three men were brought out in a fainting state, caused by a rush of foul air in untamping. Thinking some means might be devised for preventing such accidents, and the consequent loss of time and panic, I consulted with Captain Malcolm, R.E., who proposed Prof. Tyndall's firemen's respirator for consideration. Colonel Lennox sent me to the maker to inquire, and I returned with one.

"With the assistance of Quartermaster-Sergeant Ingram of the Chemical Laboratory, and several books of reference, I have collected the following notes:—

"After exploding a charge of gunpowder at a gallery-head, it becomes dangerous to untamp, because of the poisonous gases produced by the combustion of the powder. These gases are CO_2, N, CO, HS, C_2H_4, and H. The only gases that are present in sufficient quantities to harm are CO_2 and CO. CO_2 to the amount of $\frac{1}{200}$ (·005) of the bulk of the air at the gallery-head would render it unfit to sustain life. CO to the amount of $\frac{1}{100}$ (·01) would do the same. 100 lbs. of powder evolve 22559·38 cubic in. of gas at 60° F. and 30″ B., of which 9429·7896 are CO_2, and 2249·848 are CO.

"Miners working in the presence of the foul air from the explosion suffer in two ways. If affected suddenly, they feel a burning at the nape of the neck, and their limbs tremble, they turn giddy and faint. This is to be attributed chiefly to the CO. The miners are also affected in a slower manner by the CO_2. They feel their breathing becoming difficult, as if there were a weight on their chest, with a tight feeling in the head; if not brought into the fresh air they are in time overcome and faint. This also brings on headache, on coming into fresh air.

"Any method of getting rid of the foul gases by chemical means must interfere greatly with the progress of the work. In any case there would be considerable difficulty in destroying the CO, as it has neither acid nor basic properties. A *good* system of ventilation through hose would clear the galleries of the foul air, but would not overcome the difficulty of untamping, because at any moment of the process there may be a rush of foul gas, which would take effect on the men at work, before the ventilation could carry it away.

"A good respirator worn by each of the men employed at untamping might overcome this difficulty. Prof. Tyndall's respirator for firemen is constructed with a view to enable the men to inhale pure air when at work in a burning house, by separating the smoke and noxious vapours. It consists of two parts; (1) the mouth-piece; (2) the body of the respirator.

"The mouth-piece is an invention of a Mr. Carrick, hotel-keeper at Glasgow, who had patented it.* It has two valves, *i* and *e*. (See NATURE, June 15, 1871.) The air inhaled comes from below, up through the body of the respirator and through *i*. The exhaled breath closes *i*, and escapes through *e*, thus keeping the contents of the body of the respirator cool. There is an aperture *o*, which fits closely round the lips, and to prevent respiration through the nose, there is a nose-pad fixed on top of the mouth-piece. A wire-gauze partition separates the mouth-piece from the contents of the body of the respirator.

* This is not the mouth-piece now adopted.—J.T.

"The body of the respirator is about 4in. or 5in. long, and contains at the top a layer of cotton wool, moistened with glycerine to prevent any solid particles escaping into the mouth from lower layers, and also to stop those very minute particles of the smoke that may not have been arrested below. Next comes a layer of dry cotton wool, then a layer of charcoal fragments, another layer of dry cotton wool, and then some fragments of slaked lime. Below this comes some more cotton wool, and then the wire-gauze cover or cap at the bottom.*

"For smoke the layer of lime is not necessary, but in the mines it would be of the greatest use, because it has a great attraction for CO_2. The layer of charcoal would absorb the CO and the HS in the air, and the mixture inhaled would be perfectly innocuous. The disadvantages of this respirator in its present form for mining purposes are—that it is too long, and an effort is required in breathing through the small valves.† Mr. Ladd, of Beak Street, Regent Street, the maker of these respirators, has made some improvements in the mouth-piece, which may overcome some of the inconveniences of the old pattern.

"I received permission to use the R.E. workshops for experimenting on the shape best suited for use in the mines. Tyndall's respirator has been severely tested in dense and pungent smoke from pinewood, and it succeeded to the perfect satisfaction of Captain Shaw, Chief Officer of the London Fire Brigade. Firemen are to wear it attached to hide helmets, but for the mines any arrangement which will support the respirator and keep it close to the mouth during work, without being hot or uncomfortable, will suffice.

"*Experiments made with the Respirator.*—On Saturday, August 19, 1871, a trial of the respirator was made in the Chemical Laboratory, S.M.E., in the presence of Colonel Lennox, Dr. Fox, and others. I was shut up in an air-tight cupboard, with the respirator on. By my side were jars containing CO and CO_2 in a proportion of $\frac{1}{200}$ each of the cubic content of the cupboard (141,698·4 cubic in.), not allowing for the space occupied by my own body and the stool on which I sat. The respirator contained animal charcoal and lumps of slaked lime mixed together, thus dispensing with one layer of cotton wool. After emptying all the jars, I remained for ten minutes in the full mixture (fifteen minutes in all) without the slightest discomfort except from the awkward shape of the respirator. I was then called out.

"On Monday, the 21st, another trial was made in the presence of Dr. Fox and Lieuts. Abney and Galwey. This time a rabbit and three birds were placed in the cupboard with me. The respirator contained, in addition to the charcoal and slaked lime, a small quantity of sulphate of soda. The only cotton wool used was a small layer soaked‡ in glycerine at the top, and a thin layer of dry wool at the bottom. The sulphate of soda was introduced according to Prof. Graham's advice, in order to give an atom of O to the CO to form CO_2, becoming itself sulphite of soda. The content of the cupboard is 141,698·4 cubic in.: from this would have to be deducted the space taken up by my body, say $3\frac{1}{2}$ cubic ft. (Dr. Parke's Hygiene), or, roughly, 6,000 cubic in., leaving 135,698 cubic in. 1,890 cubic in. of CO_2 in jars were introduced from a pressure bag, making altogether:—

1,890 cubic in. of CO_2
1,921 cubic in. of CO,

or 3,811 cubic in. of poisonous gases in addition to my exhaled breath, or about 3 per cent. of the capacity of the cupboard. In order to perfect the mixture of the gases, I waved a towel about constantly, and after the end of the trial, a taper being extinguished at the top of the cupboard showed that the CO_2 had been stirred up to the top. The rabbit and two birds died at the same time, about twenty-three minutes after the cupboard was closed, while the CO from the pressure bag was being introduced. I stayed in the cupboard thirty minutes (five minutes after the mixture was completed and seven minutes after the death of the animals). When I came out I felt a pressure on my ears, as when descending too rapidly in diving. Dr. Fox said that this was produced by my blood, my heart then beating at a high rate.

"This is satisfactory, as showing that the gases had not affected me, but only the exertion of breathing through the respirator, for thirty minutes, combined with the heat of the close atmosphere in which I was.

"To prove that the gases did not affect me, I quote some extracts from Dr. Park's 'Hygiene':—'Dr. Angus Smith says the breathing of CO_2 to the extent of 1·5 to 2 per cent. produces slowness of heart action, while the respirations become quickened if not gasping; this is perceptible with as little as ·1 per cent. Less than $\frac{1}{2}$ per cent. of CO has produced poisonous symptoms, and more than 1 per cent. is rapidly fatal to animals. CO in excess produces loss of consciousness, slowness of heart action, and finally paralysis of the heart.

"The slowness of diffusion of the two gases was remarkably shown by the effect on the third bird. The cage which held it was suspended at the top of the cupboard. The bottom, back, and top were of wood, the other sides were of wood for about $1\frac{1}{2}$ in. and then of wire. The bird, which was at first on a perch, was very soon affected by the impure air, and fell to the bottom of the cage. Here the wooden bottom and sides evidently supported a layer of pure air, for although the bird had lost consciousness, and indeed was considered to be dead, yet after being brought out into fresh air, it was revived by ammonia, and after an hour or so fluttered away. The other animals, that were not so protected, died before all the gases had been introduced.

"On examining the sulphate of soda, very little was found to have been changed into the sulphite; it would, therefore, seem that a constant change occurred, the sulphate giving up oxygen to the CO, becoming sulphite, and then the sulphite taking oxygen from the air to form the sulphate. Whether the good effect of the first change compensates for the loss of free oxygen in the second change is a question for the opinion of a chemist; however, Prof. Graham's recommendation is of great weight.

"All that were present agreed that the trial was perfectly satisfactory, and I think this is a fair conclusion. For the object in view throughout has been to devise some plan by which a man may work for some time in a foul mine, and may be secure from the effects of a rush of foul gas caused in untamping, &c.

"Defensive mines, though small, poison the ground more effectively than overcharged mines, which allow most of the gas to escape. I have before shown the total amount of CO and CO_2 evolved by the explosion of 100 lbs. of powder, which, according to our late operations, seems to be an average charge. It is probable that a large proportion of these gases would escape into the air, and that the rest would be diffused equally all round the charge. Therefore only a small amount is likely to be encountered at any one point. Hence it would seem that the respirator, which has succeeded with very powerful mixtures of poisoned air, would be quite enough to guard the miners from any of the gases from explosions.

"It only remains now to hit upon a convenient shape which will not render the breathing laborious. If we succeed in this, it is likely that the respirator would be of use also in civil work, such as exploring mines in search of bodies after a colliery accident."*

J. E. G.

* This order may be varied in different ways without prejudice to the respirator.
† These objections have been in great part met by the recent forms of the respirator.
‡ See remark in the introduction above.

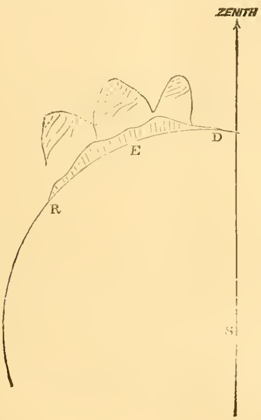
* This is one of the purposes contemplated by myself, but the suggestion of Mr. Gibbs is independent and original.—J. T.

THE STRUCTURE OF THE CORONA

AMONG the parties organised to observe the Total Eclipse of 1869, Aug. 7, that sent from the Cincinnati Observatory was probably favoured above all others in the advantage of having a comparatively elevated station and an exquisite atmosphere. The publication of the work done by this party has been delayed by the fact that for a year subsequent to the eclipse I was wholly absorbed in the labour attending the maintenance of the "Weather Bulletin of the Cincinnati Observatory," and my subsequent occupations in the present office have entirely prevented me thus far from even attempting the reduction of our observations: the original note-books are at present packed away with the library of the Observatory, awaiting the removal and rebuilding of that institution.

My own attention was expressly given to the structure of the corona and coronal streamers, for which purpose I used the full aperture of an exquisite six-inch objective (one that had received a prize at the Paris Exposition Universelle), and which was loaned to the Eclipse Expedition by Mr. T. G. Taylor, of Philadelphia.

A short notice of the principal features noted by myself was sent at once to the editor of the *Astronomische*



Nachrichten, but has not yet been published, and I therefore take the liberty of restating through your wide-spread journal the simple phenomena that I then saw.

Our station was at Sioux Falls City (formerly Fort Dakotah), in the south-eastern corner of Dakotah Territory, latitude 44°, longitude 97°, elevation about 1,500 feet above sea-level, in the midst of an extended plateau. Rain and cloud had continued up to a few hours previous to the critical moment, but the atmosphere during the eclipse was of surpassing steadiness and clearness.

The altitude of the sun at time of totality was about 40°, the local time 3.30 P.M., the duration of totality 4 minutes. No sooner had totality begun than, after sketching in most of the prominences as points of reference, I viewed the corona without darkening glasses, and with a magnifying power of probably 120 diameters. The whole interval of totality was, unfortunately, not at my disposal, owing partly to the very rough and faulty stand supporting the telescope (everything had to be transported 100 miles by mules into a wilderness), and partly to an interruption by one of the members of the party; but there seemed to me to be no doubt of the facts as recorded, nor was I conscious of the least undue emotion that might have interfered with my reliability as a witness, although it was the first total eclipse that I have had the pleasure of observing.

As seen through my inverting telescope, the structure of the large protuberance on the right hand lower limb was well made out. The neighbourhood of the sun was examined to a distance of its own diameter (a radius of possibly one degree from the sun's centre), but no trace of the coronal rays as they were seen by others of my party. The evidence as to the existence, shape, and positions of these streamers, as given by my six assistants, was conclusive as to their actual appearance, with the usual variations as to details.

That they were not detected by the six-inch glass was probably due to their diffused light and the small field of view. On the apparent upper and left hand limb of the sun, the six-inch glass showed the long series of red prominences depicted in the photographs published by the Naval Observatory. Above the greater portion of the arc of the sun's limb thus covered, and extending somewhat farther to the right, appeared to rise up three and possibly more conical masses of pearly light.

These were distinctly contrasted against the light diffused as the background of the field of view, and there was every evidence that they had an identical structure and cause. The outline of each of the pearly mountains was that of a rounded cone, as shown in the drawing—exactly resembling the kilns used in some branches of pottery and other manufactures. The apices of the cones were blunted or truncated, and not well defined; the outlines of the sides of the cones were quite sharp down to within a few minutes of the sun's limb, when all three appeared to begin to lose their distinctive characteristics.

The height of the apices above the limb varied between one-half and two-thirds of the solar radius; the diameters of the bases of the cones were probably included between seven and three minutes. Each apex was of a slightly dusky shade compared with the body of the cone.

The most interesting feature was an unmistakable striation upon the surface of each cone; the striæ apparently twisting spirally around up to the apex opposite to the movement of the hands of a watch, as represented in the accompanying drawing.

I noticed no colouration of these striæ other than their darker hue. The details of this striking and new phenomenon interested me so much that I naturally enough lost the observation of the third contact. The pearly cones were on that limb of the sun from which the moon was moving, and the details were every moment becoming more distinct, when the growing height of the bank of red protuberances was followed by the too speedy apparition of the glowing sun beneath.

Chagrin at the loss or imperfect observation of the third contact caused me to forget to note whether the three cones continued in view for any number of seconds thereafter. From the time of first recognising the pearly cones, until their disappearance, probably thirty seconds elapsed (I am writing without my note-book or other aid to memory), and I did not note any change in the appearance of the striæ.

The middle one of these cones caught my eye more especially, and the impression was that the other two, especially that on the right, was some distance behind it, or possibly obscured by a cloud of haze in the solar atmosphere. At the time it seemed to me that I saw in the central cone a column of smoke and hot gas ascending high above the area of red flames, then visible on the surface of the sun, and that the other two cones corresponded to other areas of red flames behind. The difference in character and position between these cones and the coronal streamers as observed by the others with the naked eye, and with opera glasses, seemed to argue that the latter were very probably individual subjective phenomena, or that they originated in the earth's atmosphere whilst the pearly cones existed in the solar atmosphere and constituted a true solar corona.

My long delay in making this communication to the scientific world will be excused, I trust, in view of the imperative demands made upon my time during the two years that have elapsed since the Eclipse of 1869. I shall be deeply interested to learn whether the phenomena seen by myself may not be repeated on some other occasion and be studied by more experienced observers.

I may add that I had hastily provided myself with a Nicol's prism in hopes of making at least some trial of the nature of the coronal light; but the rude apparatus did not work satisfactorily, and I confined myself to details of structure; indeed, in my earnest gaze upon the novel phenomena I quite forgot the polarising apparatus.

CLEVELAND ABBE

Office of the Chief Signal Officer, U.S. Army, Washington, Feb. 6, 1872

EARTH-CURRENTS AND THE AURORA BOREALIS OF FEBRUARY 4, 1872

IT is unfortunate that more accurate observations of the electrical phenomena accompanying auroral displays cannot be made upon the telegraph wires of this country. The truth is, public business cannot be made to suffer for scientific investigation, and at such moments the disturbance of the wires makes it more than ever imperative that delays should not occur. The whole efforts of the staff are directed to maintain the communications intact, hence the observations made on February 4 are not very numerous, though they are sufficiently interesting to deserve record.

At Portsmouth twenty-six observations were made of the direction and strength of the earth-currents on a wire extending from Portsmouth to London, *viâ* the London and South-Western Railway—a length of 74 miles, giving a resistance of 995 ohms. These were as follows:—

Time.	Direction of	Strength.	Remarks.	Time.	Direction of Current	Strength.	Remarks.
5.54	P	30°	The observations were made upon an ordinary vertical galvanometer, and 40° was equal to about 20 Daniell's cells.	6.41	P	40°	No observations made between 6.41 & 7.35.
6. 8	,,	20		—	—	—	
6.11	,,	10		7.35	—	0	
6.13	,,	30		7.42	N	41	
6.15	,,	25		7.56	,,	68	
6.17	,,	40		8.6	—	0	
6.19	,,	30		8.8	—	0	
6.21	N	15		8.15	N	35	
6.23	,,	24		8.22	P	12	
6.25	P	20		8.28	—	0	No observations made between 8.30 & 9.0 P.M.
6.27	,,	20		8.30	—	0	
6.29	—	0		—	—	—	
6.31	P	8		9 0	—	0	
6.33	,,	13		9.40	—	0	

P means Positive from London to Portsmouth.

The officer who made these observations writes:—"Strong deflections arising from earth currents were observed on all circuits except the local ones. The duration of the currents changed from north to south at intervals of a few minutes, and varied in strength from 1° to 68°. The strength of the current was proportionate to the length of the wire. Thus Chichester circuit (a short one) was affected less than the Guildford, and the latter less than the London circuits. The working was maintained to London with comparative ease by looping two circuits together at each end." The latter method is that usually adopted to overcome the disturbance due to earth currents, but of course it is only applicable in places where there are two wires or more.

Another officer at the Waterloo Station, London, observed the deflections gradually appear on every needle circuit, of which many concentrate at that station. They commenced about 2 P.M., and from that period to 8 P.M. they had all alike been more or less disturbed. It was noticed that the needles moved over gradually, not by a continuous motion, but by jerks, resembling that of the minute hand of a large clock. This has, however, been proved to be due to the friction of the pivots, and not to any pulsations in the currents.

The currents were always most apparent, and first noticeable on the longest lines, and as the lengths of the circuits terminating at Waterloo are very variable, this gradual appearance was very interesting. Lines running south-west and west appear to have been most affected.

All the wires in the Channel Islands were also very much disturbed. In fact Jersey was broken down to England for three hours, owing to the fact of there only being one cable. The section most affected was that between England and Guernsey. It was also noted that the wires in France were very much influenced.

The records from abroad show that, as in previous cases of storms of this character, the effect has been simultaneous all over the globe. The French Atlantic cable was seriously affected; the strength of the current was at one time equal to 90 Daniell cells. It was at times impossible to read even with condensers in circuit. The American lines were also disturbed in the East, West, and North, but not in the South.

It is much to be regretted that simultaneous observations cannot be made in various parts of the globe, detailing, in comprehensible units of measurement, the direction and strength of these currents, as well as the exact time of their appearance and disappearance. We might then arrive at some knowledge of their cause.

Southampton, Feb. 24 W. H. PREECE

THE DARMSTADT POLYTECHNIC SCHOOL

THE following epitome of the programme of the "Grand Ducal Hessian Polytechnic School of Darmstadt" may interest the readers of NATURE as a further illustration of the facilities offered in Germany for technical training of the highest and most practical kind.

The object of the school is stated to be a thorough scientific, as well as artistic, education, for all technical pursuits, assisted by appropriate practical exercises. The institution affords special facilities for the education of architects, engineers, mechanical and chemical technicists, manufacturers, craftsmen, and agriculturists. The institution is divided into the following sections:—(1) the Lower School; (2) the School of Architecture; (3) of Engineering; (4) of Machinery; (5) of Technical Chemistry; and (6) of Agriculture.

The Lower School aims at giving a general instruction in mathematics, natural science, and design, as a foundation for the special pursuits taken up afterwards. For admission into the school it is necessary that the student shall be sixteen years of age, and have received such an education as would be afforded by the highest class of a "Realschule," or the third course of a "Gymnasium," with the exception of the dead languages. This implies a knowledge of algebra, as far as equations of the second order, an acquaintance with logarithms, with plain geometry, and the elements of solid geometry, practice in German style, a knowledge of the outlines of history, and some practice in linear and free-hand drawing.

Examinations are held in the lower school at the end of each half-year, in the other divisions at the end of each year; a diploma is only given if the student gives satisfactory evidence of having completely mastered one of the branches of technical study in which special instruction is given. The payments consist of an entrance-fee of 5 fl., and a payment of 50 fl. per annum; and in addition 6 fl. is charged for every day's work of 7 hours in the chemical laboratory; 10 fl. for 2 afternoons of 3 hours in the physical laboratory.

In addition to the subjects required in each special department, lectures or instruction are given in the following subjects, and attendance at some of them is strongly recommended to all students, in order to give a wider culture than would be attained by exclusive attention to his special pursuit:—(1) Exercises in Literature and History; (2) the French and English Languages; (3) the General History of Art; (4) National Economy; (5) Commercial Knowledge; (6) the Principles of Jurisprudence; (7) Physical Geography; (8) Zoology; (9) Systematic Botany; (10) Singing and Gymnastics.

The course in the Lower School extends over two years, in which the following subjects are compulsory:—First year. (1) History and Literature with the German Languages; (2) Higher Algebra; (3) Stereometry and Trigonometry; (4) French; (5) Outline Geometry; (6) Free-hand Drawing. Second year. (1) History and Literature with the German Language; (2) Analytical Plane Geometry; (3) Algebraic Analysis, the Differential and Integral Calculus; (4) Higher Algebra; (5) Experimental Physics; (6) Mechanics; (7) French; (8) Free-hand Drawing; (9) Outline Geometry.

In the special schools for Architecture, Engineering, Mechanics, Technical Chemistry, and Agriculture, the entire course extends over a period of from two to four years. The extent to which the studies are carried will be illustrated by the following abstracts of the curriculum in the Agricultural School, the shortest of the courses:—First year (1) Experimental Physics; (2) Experimental Chemistry; (3) Chemical Exercises; (4) Histology and Morphology; (5) Vegetable Physiology; (6) Systematic Botany (with excursions); (7) Zoology; (8) Mineralogy; (9) The Study of Rocks; (10) Anatomy of Domestic Mammalia; (11) Physiology of Domestic Mammalia; (12) External form of Domestic Mammalia; (13) Agricultural Implements and Machines; (14) National Economy; (15) Mathematics; (16) The Drawing of Plans. Second year—(1) Chemical Exercises; (2) Agricultural Chemistry; (3) Practical Microscopy; (4) Practical Physiology; (5) The Diseases of Plants; (6) General Agriculture; (7) Special Agriculture; (8) General Breeding of Animals; (9) Special Breeding of Animals; (10) The Commerce of Agriculture; (11) The Cultivation of Garden, Orchard, and Vine; (12) Internal Diseases of Domestic Mammalia; (13) External Diseases of Domestic Mammalia; (14) Technology (Heating and Lighting); (15) Agricultural Book-keeping; (16) Irrigation, Tilling, &c.; (17) History and Literature of Agriculture; (18) Practical Geometry.

To assist in the studies of the pupils there are chemical and physical laboratories, an experimental farm, mineralogical, zoological, and botanical collections, models of machinery, designs, libraries, excursions into the country, &c. Under special circumstances students can be admitted as "Hospitanten" to certain only of the studies, without going through the entire course; but care is taken that this does not interfere with the regular studies of the other students.

LAKE VILLAGES IN SWITZERLAND

IT is satisfactory to find that the correspondents of some of the daily journals are now in the habit of giving scientific information to their readers. The following is taken from the *Standard*:—

"An interesting archæological discovery has recently been made on the shores of the Lake of Bienne. The Swiss Government has been for a long time endeavouring to drain a considerable tract of land between the two lakes of Morat and Bienne, but in order to do this effectually it has been found necessary to lower the level of the latter by cutting a canal from it to the lake of Neuchatel. At the beginning of the present year the sluices were opened, and the waters of the Lake of Bienne allowed to flow into that of Neuchatel. Up to the present time the level of the Bieler See has fallen upwards of three feet, and this fall has brought to light a number of stakes driven firmly into the bed of the lake. This fact becoming known, a number of Swiss archæologists visited the spot, and it was decided to remove the soil round these stakes to see whether any remains of a Lacustrine village, which they suspected had been raised upon them, could be traced. At a distance of between five and six feet from the present bed of the lake the workmen came upon a large number of objects of various kinds, which have been collected and are at present under the custody of Dr. Gross, of Locrass. Among them are pieces of cord made from hemp, vases, stags' horns, stone hatchets, and utensils used apparently for cooking. The most precious specimen is, however, a hatchet made of néphrite (the name given to a peculiarly hard kind of stone from which the Lacustrines formed their cutting instruments). This hatchet is sixteen centimetres long by seven broad, and is by far the largest yet discovered in any part of Switzerland, no other collection having any measuring more than eight centimetres in length. A quantity of the bones found at the same time have been sent to Dr. Uhlmann, of Münchenbuchsee, for examination by him, and he finds that they belong to the following animals, viz.:—stag, horse, ox, wild boar, pig, goat, beaver, dog, mouse, &c., together with a number of human bones. If the level of the lake continues to sink, it is hoped that further discoveries will be made, and the scientific world here is waiting the result of the engineering operations with keen interest."

NOTES

WE have great pleasure in announcing that Prof. Andrew C. Ramsay, F.R.S., has been appointed Director-General of the Geological Survey in the room of the late Sir Roderick I. Murchison.

AT the moment of going to press we have received the announcement of the death of Prof. Goldstücker, the eminent Sanscrit scholar. He died on Wednesday morning.

MR. G. B. AIRY, the Astronomer Royal, and Prof. Agassiz, were elected foreign associates of the *Académie des Sciences* at Paris in the room of the late Sir J. Herschel and Sir R. I. Murchison at the meeting on the 26th ult.

DR. MAXWELL SIMPSON, F.R.S., has been elected as successor to the late Dr. Blyth in the chair of Chemistry, Queen's College, Cork. Dr. Simpson is well known to men of science at home and abroad as an accomplished chemist, and one who has been especially distinguished for his original researches.

THE Crystal Palace Company's School of Art, Science, and Literature is about to take an important step, having for its object the emphasising of the science branch of the school, in order that eventually the south of London may be provided with an institution which, in a measure, may represent the Royal and London Institutions which already exist in the west and centre. The step consists in adding to the courses of lectures on scientific subjects already given special courses to be given from time to time by scientific men of eminence, similar to the courses given in the Institutions before referred to; and it is hoped that the same lectures and the same standard of excellence and illustration may be secured. As the lecture theatre of the school has been burnt down, the lectures, pending its rebuilding, are to be given in the theatre in the Crystal Palace; but it need scarcely be stated that these lectures have no connection with the Crystal Palace, except so far as the School of Art, Science, and Literature is connected with it, and that they will be given at a time when the

Palace is closed to the general public. Mr. Norman Lockyer has consented to give the first course of lectures. This step taken by the Committee is in every way to be commended, and we look with confidence to the success of these lectures as paving the way for others in various parts of the country, which may eventually do much towards popularising Science among the masses.

PROF. P. MARTIN DUNCAN, F.R.S., is now delivering the course of Lectures on Biology to the class for the Higher Education of Women at South Kensington, in the place of Prof. Huxley, who is still in Egypt for the complete restoration of his health.

MR. W. MARSHALL WATTS, D.Sc., of the London University, has been appointed to an assistant mastership in Giggleswick Grammar School, Yorkshire. The governors have settled that chemistry, including practical work in a laboratory, and physics, shall hereafter be taught in the school, and the teaching of these branches of science has been entrusted to Dr. Watts. Until recently Dr. Watts has had the main charge of the teaching of chemistry in the Manchester Grammar School, a school which has been eminently successful in obtaining scholarships in physical science at Oxford. Mr. E. K. Purnell, Scholar and Prizeman of Magdalen College, Cambridge, has also lately been appointed to a classical mastership in Giggleswick School.

THE Council of the St. Andrews Medical Graduates' Association are about to appeal to the many friends of the late Professor of Medicine in the University of St. Andrews, to aid them in an attempt to make a more fitting provision for his widow than Dr. Day's ill-health allowed him to accomplish. We regret to learn that such an appeal is necessary, and heartily wish it success.

THE Haberdashers' Company have recently awarded Mr. Webb, the Senior Wrangler of Cambridge, 50*l*. for three consecutive years; he having been a pupil of the Rev. C. M. Roberts at Monmouth, of which school the Haberdashers' Company are governors. They also propose to grant four exhibitions of 50*l*. to the children or grandchildren or apprentices of Liverymen of the Company under certain restrictions, to be tenable for three years. In addition to the above, one exhibition of 50*l*. will be specially granted to a scholar of any school under the Company's management. The sum of 150*l*. will also be appropriated towards assisting the education of children of the Livery of the Company. 100*l*. yearly will also be awarded as a prize to the inventors of anything new in haberdashery.

As the period of the Transit of Venus in 1874 approaches, astronomers both at home and abroad are becoming more and more active in their preparations; and the American committee on this subject, it is understood, has already decided in considerable part upon the stations to be occupied. Of the result of their conclusions we hope to give an account before long to our readers. In Russia the committee, under Prof. Struve, proposes the establishment of a chain of observers, at positions 100 miles apart, along the region comprised between Kamschatka and the Black Sea. The German committee has decided on recommending the organisation of four stations for heliometric observations of the planet during its transit, one of them in Japan or China, and the others probably at Mauritius, Kerguelen, and Auckland islands; and three of these, with the addition of a fourth station in Persia, between Mascate and Teheran, will be equipped for photographic observations also. The French, before the war, suggested that stations be established at St. Paul Island, New Amsterdam, Yokohama, Tahiti, Noumea, Mascate, and Suez. How far this programme will be carried out under the changed circumstances of that country remains to be seen.

WE have received a letter from a valued correspondent, calling attention to some defects in the arrangements for the study of the Natural Sciences, and especially of Botany, at the University of Cambridge. The letter we refrain from publishing, in the belief that the good work which is now proceeding at the Universities will be carried out eventually far more completely than it is at present, and that even Botany may ultimately receive the attention that it deserves.

THE Brazilian steamer to New York brings advices of the safe arrival at Pernambuco of the steamer *Hassler*, with Prof. Agassiz and party. They were to leave for Rio Janeiro, in company with the *Ticonderaga*, on Jan. 16. As there are several gentlemen on board who have undertaken to supply information in regard to the movements of the vessel, we shall doubtless before long have full accounts of the progress made up to the date mentioned; although in regard to the subject of deep-sea soundings and supposed discoveries connected therewith we must probably wait, for correct details, for the official report to be made by Count Pourtales direct to the Superintendent of the Coast Survey.

THE "Annual Report of the Secretary of the Interior for the United States on the Operations of the Department for the year ending October 31, 1871," states that the results of Prof. Hayden's expedition, in accordance with his instructions to investigate the geology and natural resources of the little known, but interesting, region about the source of the Yellowstone and Missouri rivers, shows it to have been a complete success, and fully to justify the liberal provision made by Congress for it. A preliminary report of the results was to be presented to Congress at an early date. A great amount of valuable notes and specimens, illustrating the agricultural, mineral, zoological, and botanical wealth of the West, was secured.

WE learn that the Smithsonian Institution has recently succeeded in obtaining two complete skeletons of the remarkable tapir of the highlands of the United States of Colombia, known to naturalists as *Tapirus pinchaque* or *roulini*. Previously only the skull had been obtained by Roulin, by whom it was first made known, and it was one of the rarities of the great anatomical collection at Paris. The Smithsonian Institution had before obtained a number of skulls and a skeleton of the still more remarkable tapir of Panama, which had remained undistinguished from the common species of Panama till within a few years, when first described, under the name of *Elasmognathus bairdii*, by Prof. Gill, from two skulls in the Smithsonian collection. There are no external or dental differences between the tapirs corresponding with the marked differences in the skulls; the external differences being confined to the contour of the forehead, the colour, and the character of the hair. In the mountain tapir, as might be expected in an animal dwelling in such elevated regions, the hair is long and coarse, and is of a black colour, strongly contrasting with that of the common tapir of South America; it is also somewhat smaller than that species, and has the forehead less arched from the occiput. It is confined to the highlands, and is separated, at least so far as is known, by quite a wide band of country from the common species.

THE Report of the officers of the Peabody Academy of Sciences of Salem, lately made to the trustees, presents a satisfactory statement of the progress made during the past year. This establishment received a moderate endowment from George Peabody, of London, and the income is expended in the care of the valuable museum belonging to the Academy. The directors of the establishment are Mr. F. W. Putnam and Dr. Packard. The principal additions to the museum of the Academy during the year have consisted mainly of insects and archæological specimens, and also a series of the animals inhabiting the Mammoth Cave of Kentucky. All of these, together with the collections previously in the museum, have been properly arranged and classified, and tend to render the museum very attractive. The report urges very strongly the propriety of securing an additional endowment, to enable the Academy to publish in its memoirs certain valuable scientific manuscripts now in hand, the alternative of being obliged to send them to some other establishment having more means at its disposal being greatly deplored, as they were based upon the collections of the Academy, and should legitimately appear under its auspices.

THE Clifton College Scientific Society has just issued the second part of its Transactions, containing the record of its proceedings from February to July, 1871. The president and secretary state in their Report that the papers read at the Society's meetings have been as numerous as previously, and the attendance of members and visitors has in no degree fallen off; and that, although there is still much to be desired in this respect, yet the number of working members is steadily increasing. The various sections of botany, zoology, entomology, geology, archæology, chemistry, and physics have, on the whole, done good work, the least satisfactory reports being in the case of zoology, chemistry, and physics. The great event of the half-year has been the long-expected opening of the new Museum and Botanic Garden, both of which institutions are well deserving of support from those outside the school who are able to assist in furnishing them. The Botanic Garden is already one of the very best to be met with anywhere in the provinces. Among the papers read before the society and printed in the Transactions, the following have struck us as especially excellent:—"A Scientific Visit to Cheddar," by the President and J. Stone; "The Church of St. Mary Redcliffe, Bristol," by R. W. Wilson; "The Coalfield of South Wales," by A. Cruttwell; "The Birds of Clifton," by D. Pearce; and an admirable paper on "The Spectrum," by W. A. Smith.

THE last number of the *Bulletin de la Société de Géographie* contains an article by Delesse on the oscillations of the coasts of France.

THE Annual Address, delivered before the Albany Institute, New York, by Orlando Meads, on May 25, 1871, has just reached us. It is chiefly occupied with a sketch of the history of this successful and enterprising institution.

THE *Poona Observer* of February 6 gives the following account of Indian Geological Excursions:—"The Principal of the Poona Civil Engineering College, Mr. T. Cooke, together with the Professor of Chemistry, Mr. S. Cooke, with about twenty students of the first class, proceeded on a geological excursion on January 29, and arrived here on Saturday morning last. After leaving Poona they arrived at Shabad, where they remained for a whole day. The next morning they left Shabad and arrived at Krishtna at about ten in the morning, and inspected the Krishtna Bridge. After inspecting several works of the G. I. P. R., they started for Poona on the afternoon of Friday. The thanks of the students as well as of the Principals are due to the G. I. P. R. Company, in kindly placing their waiting-rooms at the several stations where they halted, at the disposal of the boys. The expense of this excursion is to be borne solely by Government. The amount allotted for the purpose of this excursion was 500 Rs."

THE following account of the Aurora of February 4 appears in the *Times of India* of the following day:—"A magnificent Aurora was visible, from the Rawul Pindee portion of the Punjaub, last night, February 4, from 12 to past 12.30 o'clock. It occupied the northern quadrant of a clear sky, or rather more, the stars shining dimly through a glowing deep red hazy light reaching half way up the heavens, and which was crossed by thin vertical rays of white light stretching to the south. The night was calm but less frosty than usual at this season, and the oldest inhabitant who witnessed the display averred he had never seen anything like it in his life before." The suggestion made by our correspondent Mr. Earwaker, that we witnessed on that day a combination of the Northern and Southern Aurora, is thus confirmed.

SCIENCE IN PLAIN ENGLISH

IN a paper under this heading, in the *Boston Journal of Chemistry*, Mr. C. A. Joy, after quoting from our articles of June 22 and 29, 1871, proceeds thus:—We must admit that what Mr. Rushton says of English schools applies equally well to our own. Does anybody know of a preparatory school in the United States where instruction in science is given on a systematic plan by teachers especially fitted for the work, and with well-selected apparatus and judicious text-books, and where an equal value for excellence in science is given to pupils as for mathematics and the languages? There are, doubtless, some such schools, but it is my misfortune never to have heard of them. The truth is, there are few teachers. The custom in this world of studying everything else but the world we live in, which has been handed down to us from our ancestors, has precluded the possibility of anybody being fitted to teach the natural sciences excepting the few who have had the energy and the means to overcome every obstacle, and to learn something; and they are so rare that they are not to be had for ordinary schools. We are now in a fair way to acquire considerable knowledge of the planet Mars, its climate and physical condition; and it may be that we shall some day be favoured by a visit from an inhabitant of that distant world. The arrival of such a visitor would be rapidly heralded over the land, and he would be introduced to our best society, to the leading men of education; and as he would doubtless be possessed of an inquiring turn of mind, he would have many embarrassing questions to ask. He might address the inquiry to the gentleman on his right at the public dinner, which would be sure to be given to him, as to the composition of the crust of the earth; or he might ask what the glass windows were made of, and what form of light shone through them, or the water on the table and the air of the room might absorb his attention. If the respondent happened to be a University bred man the chances are ten to one he could not answer a single question; he would be forced to say that the study of the language of a people formerly occupying a small portion of the globe had monopolised all of his time, and prevented the acquisition of a knowledge of any of the natural phenomena around him; he might, in fact, have more knowledge of Mars than of the earth. It is probable that our visitor would be slightly astonished at the ignorance of the best educated members of the community. I do not know that we are bound to prepare ourselves for the approaching visit, but the very suggestion of it ought to startle us a little out of our propriety, and make us review the course of instruction we have pursued for so many years. As long as the requirements for admission to college are left just as they are at present, all persons who expect to go to college must follow a prescribed course or be found wanting. The teacher in a preparatory school knows that the pupil can attend only a certain number of hours, and to get up his task for admission to college nearly all this time must be devoted to classical studies. There is no time left for science, and it is not taught. This state of things has led to a violent controversy on the part of the advocates of the two systems, and the question appears to be no nearer a solution at the present time than it was many years ago. The advocates of classical training will not yield an inch of ground, and the scientists are equally firm. It is a pity that some compromise cannot be affected, as a knowledge of Latin and Greek is of great value to the scientific student, and ought not to be omitted. And as the classicists now have the colleges in their power, would it not be well for them to recommend a knowledge of language rather than of grammar, and a facility of reading generally instead of prescribing the precise number of chapters and verses? If the teacher of Chemistry, for example, were to insist upon the students studying 100 pages of Miller, 50 pages of Roscoe, two books of Gerhardt, the correspondence of Lavoisier, and the life of Berzelius, before presenting himself for examination, he would be looked upon as slightly deranged; and yet this is precisely what is done by our classical friends. A chemist can tell in half an hour whether the candidate is prepared to go on with a certain class; and he cares not how, when, or where the applicant obtained the knowledge. Not so our classical friends; they insist upon chapter and verse as if there were a charm in the prescribed number—and by so doing they do great harm to our schools. A friend of mine desired to put his son at a select school, and had a long conversation with the principal in reference to the studies he would have to pursue in order to fit him for college. The principal had the experience of thirty years in his calling, and knew precisely what was required. He produced his scheme of hours, and convinced the parent that in order to fit his son for college it would be necessary for him to devote a certain number of hours to the reading of a prescribed number of pages and verses of Latin and Greek; and to do this no deduction could be safely made. He showed that the average attendance of boys was about 6,000 hours, and by assigning to each hour its particular work, if not interrupted by accident or illness, the pupil would be able to come up to the prescribed standard. My friend tried to see if a few minutes could not be gained for a small amount of science, but the teacher, with his experience of thirty years, was inexorable, and he could not crowd in a knowledge of this world into the course of studies even edgewise. It has been sometimes said that the most ignorant members of our community are our men of education; and after looking over the scheme of studies which the victims of liberal education are obliged to follow, the paradoxical remark would almost appear to be true. It may therefore be asked, What change the advocates of reform would propose? I cannot attempt to answer this question for all parties, as there is little uniformity of belief on the subject; but it may be well to state the case of a prominent party in the modern agitation. We have a large class among us who admit the culture to be derived from the study of language, and who would not on any account banish Latin and Greek from the curriculum; but they would remove that study to a later part of the course, and replace it by scientific subjects. They think that those subjects which cultivate and strengthen the powers of perception, observation, and judgment, should be taught first. They would instruct the youth in a knowledge of the laws of health or physiology; they would have him know something about plants, animals, minerals, and the commonest laws of chemistry and physics, so that if the pupil is compelled to leave school at an early age, he would know how to take care of mind and body, and be enabled to turn his knowledge to some account. They would commence the study of Latin and Greek at a period when the mind is more mature, and thus avoid the enormous waste of time, the bad habits of droning over lessons, and the monopolising character of the present system. There are so many instances of persons who commenced the study of the classics at mature years, who have excelled all others, that the advocates of postponing languages to the latter part of a boy's course appear to be justified in their claim. If the study of Latin and Greek could be commenced after the student enters college, it is believed that more real progress would be made in the four years of the college course than is effected under the present arrangement of devoting ten years of a boy's life to this study. This is the compromise that many good men advocate. They wish the preparatory schools to be wholly given up to mathematical, scientific, and English studies, and to have the colleges assume the charge of the classics. Instead of devoting every hour of the preparatory course to languages, they would give the time to the sciences, and they would demand a knowledge of the general principles of science as a requisite for admission to college. This would be turning the tables entirely, and would afford scientific men a chance to try the effect of the modern education. The other side have had it all their own way for a long time, and it would appear to be no more than fair for them to let people of different views have a chance. Such a radical change as this cannot be accomplished at once. It would demand immense moral courage on the part of the trustees of a college to expose themselves to the cry of lowering the standard of study. They would have the alumni of existing institutions and the prejudices of the whole community against them, and it would require a generation before the majority would become reconciled to the new order of things. Another obstacle would also arise at the outset, and that would be the difficulty of securing competent teachers of the natural sciences. It is this obstacle that has stood in the way of the introduction of the study of science in our schools. There are far too few teachers. To surmount this difficulty in the city of New York a normal college for females and a free college for males have been established, and scientific schools have been founded in all parts of the country. These institutions are destined to work a great revolution. As soon as they have trained a sufficient number of teachers, we shall find our public schools affording a better education than at present, and their example will have to be followed by the owners of private schools, who desire to keep up with the progress of the age. What we want is science taught in plain English, and there is every prospect of our speedily attaining the desired end."

SCIENTIFIC SERIALS

Numbers 8, 9, and 10 of the 27th volume of the *Proceedings of the Swedish Academy of Sciences* (Öfversigt af Kongk. Vetenskaps Akademiens Förhandlingar) which have just reached us, contain several valuable contributions to science. The most important of these relate to zoological subjects. Thus we find from M. Anton Stuxberg the first portion of a paper modestly described as a contribution to the Myriopodology of Scandinavia, but containing a synonymic revision, with descriptions, of the Swedish Chilognatha, under which the author recognises the genera *Julus*, *Isobates*, *Blaniulus*, *Polydesmus*, *Craspedosoma*, *Glomeris*, *Polyxenus*, and *Polyzonium*, including in all eighteen species. M. G. Lindström contributes a paper on opercular structures in some recent and Silurian corals, in which he refers especially to *Goniophyllum pyramidale* and *Cystiphyllum prismaticum*. From M. Gustaf Eisen we have a most valuable contribution to the Oligochætal fauna of Scandinavia, illustrated with numerous figures on seven plates, and containing a monograph of the Scandinavian species of the genus *Lumbricus*, of which eight are recognised by the author. As the characters are given in Latin, and most of the species are found in this country, this paper will be of particular value to British naturalists. One species, *Lumbricus purpureus*, is described as new.—M. J. E. Areschoug communicates a list, with remarks, of a series of algæ collected by Dr. Hedenborg at Alexandria.—The longest paper is an account, by Prof. A. E. Nordenskiöld, of the Swedish Expedition to Greenland in 1870. This paper contains some interesting observations, illustrated with diagrams, on the glacial phenomena of Greenland; the remarks on the geology of the more interesting parts of the coast, especially those where fossil plants are found, are also of great importance; as is the account given of the supposed meteoric iron-stones of enormous size which have lately attracted so much attention. Analyses of the material of these masses by the authors, T. Nordström and J. Lindström, are given. Lists of the land plants and algæ collected on the expedition, and of the microscopic algæ obtained from the inland ice, form an appendix to the paper. M. P. T. Clevi contributes a paper on platinum-bases containing organic radicals, and M. G. R. Dahlander some investigations relating to the mechanical theory of heat.

THE *American Naturalist* for January (vol. vi., No. 1) commences with Prof. Agassiz's letter, already printed in our columns, on Deep-sea Dredgings. Mr. F. W. Putman follows, with an extremely interesting and well-illustrated article on the Blind Fishes of the Mammoth Cave of Kentucky and their Allies, a sequel to Mr. Packard's paper on the Blind Insects of the same locality in the previous number. Dr. R. H. Ward describes a new erecting arrangement, especially designed for use with binocular microscopes. One of the most interesting articles in the number is on the Rattlesnake and Natural Selection, by Prof. N. S. Shaler, who, from observation of the animal in its native haunts, regards the rattle as a useful appendage, imitating the note of the Cicada, and thus attracting birds which are in the habit of preying on that insect. Prof. Shaler states that, without committing himself to a belief in the sufficiency of natural selection to account for the existence of the snake's rattle, he has been driven step by step from a decided opposition to the whole theory, and compelled to accept it as a *vera causa*, though still thinking it more limited in its action than Mr. Darwin believes. There is the usual supply of interesting short notes on the various branches of natural history.

Journal of the Scottish Meteorological Society, October 1871, New Series, No. xxxii.—This number of the Journal of the Scottish Meteorological Society contains a paper by Mr. Buchan, the secretary, "On the Rainfall of Scotland," based on observations made at forty-six places during long series of years. The questions of droughts and excessively wet years are dealt with. As regards their geographical distribution it is shown that some have been felt over the whole of Scotland, whilst others have been restricted to the west or to the east of the country, or within still narrower limits; and as regards their recurrence, that there has been no perodicity observed, and that there is nothing in the observations of the past forty years to sanction the opinion that there has been any progressive increase or decrease in the Scottish rainfall. The important engineering question of the deficiency of the three driest consecutive years' rainfall from the average is carefully examined, and the conclusion is arrived at, that in estimating the rainfall of the three driest consecutive years, it will not be safe to deduct less than one-fourth from the average annual rainfall. Mr. Buchan contributes another paper "On the Temperature of the Soil compared with that of the Air," being a discussion of series of observations made twelve times daily in different parts of Scotland, at the instance of the Marquis of Tweeddale, president of the society. From the observations it is seen that the surface temperature of the soil is considerably colder than the air resting on it in winter, and con-considerably warmer in summer; and from the relations of the temperature of the soil to that of the air during changes of weather, some interesting results are drawn with reference to the influence of solar and terrestrial radiation on climate.—A brief notice of the winter climate of Malaga, detailed notes of the weather of the quarter, and tabulated returns from ninety-one stations, including several highly important stations in Iceland, Farö, and regions bordering on the Mediterranean, make up the number.

Journal of the Chemical Society, December 1871.—This number commences with a paper by Watson Smith, "On the Distillation of Wood," and although of considerable technical interest it does not present any new features.—A paper on Anthraflavic Acid follows, by W. H. Perkin. This is a substance which occurs in the artificial alizarin of commerce. Two distinct formulæ have already been assigned to this body by Drs. Schunck and Liebermann. This communication proves conclusively that these formulæ were wrong, and that in reality this acid is isomeric with alizarin, but unlike that body it possesses no tinctorial power.—Dr. Armstrong contributes a paper on the action of Nitric Acid on the Dichlorophenol Sulphuric Acids. The results obtained are very interesting, but seem to cast some doubt on the theoretical speculations of some German chemists on the constitution of those bodies.—The abstracts in this number occupy 100 pages, and comprise many papers of great value.—E. Baudiment has made an extensive series of experiments on the intimate action of substances which assist the decomposition of potassic chlorate and the disengagement of oxygen. Many bodies were tried, some of which, as cupric or manganic oxides, when heated with potassic chlorate, as is well known, yield oxygen very readily, in this case, when the temperature reaches a definite point, a sudden rise of 50° or 60° takes place with a tumultuous evolution of gas. The author has found that the decomposition of potassic chlorate is always accompanied with a disengagement of heat, so that this substance may be called an endothermic compound.

THE *Monthly Microscopical Journal*, February 1872.—"On the relation of Nerves to Pigment and other Cells or Elementary Parts," by Dr. Lionel S. Beale, F.R.S. After alluding to the tendency of opinion in these days to favour the conclusion that the finest branches of nerve fibres come into structural relation with the active elements of other tissues, Dr. Beale affirmed his opinion that, whatever may be the influence produced by the nerves upon the structure, he does not think it depends upon continuity of substance between the nerve and the tissue affected.—"Report on Slides of Insect Scales," sent to the Royal Microscopical Society by the Chevalier de Cerbacq, examined by Henry J. Slack.—"On the Structure of the Stems of the Arborescent *Lycopodiaceæ* of the Coal Measures," by W. Carruthers, F.R.S.—"On a Leaf-Bearing Branch of a Species of *Lepidodendron*." These papers contain the results of an examination of a series of specimens from Mr. John Butterworth, of Shaw, near Oldham.—"On Bog Mosses," by Dr. R. Braithwaite, F.R.S., part iii., Monograph of the European species. This paper includes an enumeration of species, and full description of *Sphagnum cymbifolium*, the first in the series.—"The advancing powers of Microscopic Definition," by Dr. Royston Piggott. This is a further contribution to the vexed question of beaded scales, and may be taken as a summary of Dr. Royston Piggott's views, of which the first portion appears in the present number of the journal.—"Microscopical Object-glasses and their Power," by Edwin Bicknell; "Remarks on a Tolles' Immersion, $\frac{1}{15}$," by Edwin Bicknell; "Maltwood's Finder Supplemented," by W. K. Bridgman. This latter communication offers a plan by means of which two correspondents may bring their "Maltwoods" into relation with each other, supposing that their indications do not coincide.—"On a new Micro-telescope," by Prof. R. H. Ward, reprinted from the "American Naturalist." This number of the journal is illustrated by four plates.

THE *Journal of Botany* for February is ornamented by a very good portrait of the late editor, Dr. Berthold Seemann. The original articles are fewer than usual, including only the conclusion of Mr. J. G. Baker's paper on the Botany of the Lizard Peninsula, and a case of poisoning by the seeds of *Macrozamia spiralis*, by Dr. George Bennett. There are, however, a good many interesting short notes and several valuable reprints, including Dr. W. R. McNab's Histological Notes, read before the Botanical Society of Edinburgh; a list of new species of phanerogamous plants published in Great Britain in the year 1871 in the *Annals and Magazine of Natural History*, *Botanical Magazine*, *Floral Magazine*, *Gardeners' Chronicle*, Hooker's *Icones Plantarum*, *Journal of Botany*, *Journal of the Linnean Society*, and *Refugium Botanicum*; and Canon Kingsley's admirable address to the Winchester and Hampshire Scientific and Literary Society, on Bio-Geology.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, February 21.—Prof. Ramsay, F.R.S., vice-president, in the chair. The following communication was read:—"Migrations of the Graptolites." By. Prof. H. Alleyne Nicholson, M.D. The author commenced by stating that the occurrence of the same species of marine animals in deposits in different areas is now generally regarded as evidence that such deposits are not strictly contemporaneous, but rather that a migration from one area to another has taken place; this migration he thought would probably in many cases be accompanied by modification. Applying these principles to the Graptolites, he endeavoured to show in what directions their migrations may have taken place. He excluded from the family Graptolitidæ the genera *Dictyonema*, *Dendrograpsus*, *Callograpsus*, and *Ptilograpsus*, and stated that the family as thus limited extended from Upper Cambrian to Upper Silurian times. The earliest known Graptolites were those of the Skiddaw Slates, which he thought would prove to belong to the Upper Cambrian series. The Skiddaw area he considered to extend into Canada, where the Quebec group belongs to it. Genera of Graptolites belonging to this area are represented in Australia, and this the author regarded as indicative of migration, but in which direction was uncertain. Having discussed the forms of Graptolites characteristic of the deposits in the Skiddaw-Quebec area, the author proceeded to indicate the mode in which the family is represented in the areas of deposition of the great Silurian series, namely, the Llandeilo areas of Wales and Scotland, the Coniston area of the North of England, the Gala area of South Scotland, the Hudson-River area of North America, and the Saxon and Bohemian areas, giving under each of these heads a list of species, with indications of their probable derivation. Mr. Etheridge commented on the importance of Dr. Nicholson's paper, and on the difficulties attending the study of the Graptolitidæ. The migration of these organisms appeared to him to be very difficult to establish, especially in connection with their extension both eastwards and westwards. Mr. Hughes believed that if we could discover the original of any species, we should see a small variety appearing among a number of forms not very different from it, and from which it had been derived; but when the variety had prevailed, so as to be the dominant form, we were far on in the history of the species; that it was a great assumption to fix upon any bed we now know as representing the original source of any group; that we know too little about the chronological order of the geological divisions referred to to reason with any safety on the migration of Graptolites from one era to another; that the term *Lower* Llandeilo, for instance, was very unsatisfactory as used in the paper; there was nothing lower than the Llandeilo Flags at Llandeilo; and where older beds occurred in Scotland and elsewhere, it was not at all clear that the equivalent of the Llandeilo Flags was present at all. He differed also altogether from the author as to the position of the Dufton Shales, and criticised the views of the author as to the range of some species. He thought that M. Barrande's theory of the colonies was borne out by the study of the Graptolites, but that we had not sufficient data to speculate as to the areas in which they made their first appearance, or the order of their geographical distribution. Prof. Duncan observed that at the present time there was, among other forms, quite as great a range for species as that of the Graptolites pointed out by the author. Having looked through all the drawings of Graptolites that he could meet with, he had found none whatever that were accurate; and he had moreover never in any specimens discovered such cups or *calices* between the serrations as were always attributed to these organisms. From all he had seen he was led to the conclusion that the projections on the Graptolites bore the same relation to the central stem as those of some of the Actinozoa. These latter also, like the Graptolites, seemed to prefer a muddy sea. Professor Duncan also suggested that the Graptolites were really the remains of the filiform polypiferous parts of floating Hydrozoa. Prof. Morris regarded the paper as mainly suggestive. It was on all hands agreed that there were in Britain two principal zones in which graptolitic life was most abundant; and the same held good in America. Both these seemed to be homotaxially related. M. Barrande had long since pointed out the probable emigration of many of the Bohemian species from the British area; and there could be no doubt of there being many species common to Europe, America, and Australia. This afforded strong evidence in favour of some such theory as that of migration. He cautioned observers as to taking careful notice of the manner in which Graptolites are presented in their matrix; for when seen from three different points of view, they exhibited such differences that three species might be made from one form of organism. Mr. Gwyn Jeffreys mentioned the wide distribution of marine Hydrozoa by means of winds and currents, as illustrative of the history of Graptolites, the dispersion of which might have arisen from similar cause, and not from migration. Mr. Prestwich commented on the uncertainty of our knowledge with regard to Graptolites, and consequently regarded speculation on the subject of their migration as premature. He instanced *Cardita planicostata*, which was formerly regarded as having originated in the Paris basin and come thence into England, but which had since been found in far earlier beds in Britain; so that the presumed course of its migration has been reversed. Mr. Hicks remarked that the rocks referred by the author to the Upper Cambrian were in reality the lowest of the Silurian series, and that the Graptolitidæ were exclusively a Silurian family. Mr. Hopkinson also made some remarks both on the distinction of different species of Graptolites and on their distribution. He regarded the Quebec area as that in which these forms had originated. The Chairman commented on the great want of accord among those who had studied Graptolites, not only with regard to their structure, but to their distribution in different horizons. He thought that the suggestion of the author, as to modification of form during migration having taken place, seemed to throw some light on the subject. He could not regard two districts now only separated by the Solway Firth as constituting two geographical areas so distinct that the occurrence of the same species in both could with propriety be held to be due to migration. The phenomena in the other cases seemed to him quite as much in accordance with distribution from some common centre as with migration along any line connecting two spots where Graptolites are now found. He thought that the recurrence of these forms on different horizons in Cumberland was to be accounted for by the fact that most of the rocks which intervened between the shales containing these organisms were merely sub-aërial volcanic beds, on which, after submergence, these muddy shales had been deposited.

Entomological Society, February 19.—Prof. J. O. Westwood, president, in the chair.—Drs. Ransome and Livett, and Messrs. Rothera and Jenner, were elected subscribers to the society.—Mr. F. Smith made some observations respecting the occurrence of two pupæ in one large common cocoon of *Bombyx mori* from China. The examples had been found amongst silk-waste in a London warehouse, and this waste had been attacked by mice, which fed upon the dead chrysalides. He further remarked that, occasionally, two or more swarms of wasps united in building a common nest, and also that broods of different species of wasps could be induced to act in concert, the result being that when these wasps used different building materials, a parti-coloured nest was produced.—Mr. Butler exhibited drawings of a large grub, apparently the larva of some species of Ichneumonidæ, which had emerged from the larva of the common "buff-tip" moth (*Pygæra bucephala*), which it nearly equalled in size.—Dr. Buchanan White communicated extracts from his note-book respecting the habits of a species of ant as observed at Capri in 1866, confirming Mr. Moggridge's recent observations as to the grain-storing habits of these ants. Mr. Horne had observed a similar habit in certain Indian ants.—Prof Westwood exhibited type-specimens and drawings of the animal from Madagascar, upon which Latreille founded his genus *Prosopistoma* as pertaining to the *Crustacea*; and made some remarks thereon connected with the assertion of a French entomologist, Dr. Joly, that these creatures, and "le Binocle" of the neighbourhood of Paris, described by Geoffroy, are in reality the earlier stages of species of *Ephemeridæ*. Prof. Westwood was scarcely able to believe that this association was founded upon facts, though he was not disposed to express any opinion as to their actual affinities.—Mr. Müller read some remarks on the habits of certain gall-producing saw-flies of the willow, which are said to avoid those portions of the trees that overhang water, and suggested a practical application of the theory to save choice fruit-trees from the attacks of insects, by surrounding them with glass at the base, it being well known that glass is often mistaken for water by aquatic insects.

Anthropological Institute, February 19.—Sir John Lubbock, Bart., F.R.S., president, in the chair. Messrs. C. Bowley, R. J. Nunn, Edward Harris, J. E. Price, and J. P. Steele, were elected members. Mr. H. H. Howorth read a paper entitled "Strictures on Darwinism. Part I.: Fertility and Sterility." After a brief statement of the evolutionary theory of Mr. Darwin, which was the old-fashioned theory of Malthus pressed to its utmost limits, viz., that in the struggle for existence which is always going on everywhere the weak elements go to the wall and are gradually eliminated whilst the strong survive, the author stated his intention in the present paper to confine his examination to one case in its concrete form. He criticised the argument that physical vigour, health, and strength had, in the struggle for existence, a tendency to prevail to the expulsion and eradication of weakness and debility, and he held that the reverse was the truth as regarded the large majority of cases, and the paradox was the same in substance as that maintained by Mr. Doubleday in his true Law of Population. It was shown that the gardener, who was an empirical philosopher, in his experience of cultivated plants, was fully aware of the truth of the principle advocated by the author, and a great number of instances were cited in illustration. Passing from the vegetable to the animal world, he showed how stock-keepers and breeders had accumulated much sound experience, which corroborated that of the gardener in regard to plants. It was a golden rule with them to keep their animals weak and in a state of depletion if they wished them to breed freely. Pure breeds were seldom very fruitful, they were notoriously pampered and highly fed; but when turned into coarse and scanty pastures their rounded sides became denuded of flesh and the animals bred more freely. The same principle obtained with man. It was in the crowded alleys and among half-starved or ill-fed populations that fertility was greatest. The author had high authority for stating that as a general rule convalescent persons—those recovering from prostrating diseases—were very fertile. On the other hand, with the rich and well-to-do, especially among families whose position for some generations had been prosperous, comparative sterility prevailed. Illustrations of that dictum were drawn from the writings of physiologists, from statistics, from the genealogical histories of the nobility and gentry, and were sustained by lengthened argument. National and ethnic tendencies to fertility or sterility were surveyed by the author, *e.g.*, among the Irish, various Black and savage peoples, Americans aboriginal and modern, the Slaves, and various Russian tribes. In conclusion, the arguments were thus summarised: that sterility is induced by vigorous health and by a plentiful supply of the necessaries of life, while fertility is induced by want and debility, and that this law acts directly against Mr. Darwin's theory, inasmuch as it is constantly recruiting the weak and decrepit at the expense of the hearty and vigorous, and is thus persistently working against the favourite scheme of Mr. Darwin, that in the struggle for existence the weak are always being eliminated by the strong.

Manchester

Literary and Philosophical Society, February 20.—Mr. E. W. Binney, F.R.S., president, in the chair. The president said that at the meeting of the society on the 9th of January last he alluded to the probability of the genus *Zygopteris* being found in the limestone nodules of the Foot Mine near Oldham. He had lately had an opportunity of inspecting the collection of Mr. James Whitaker of Watershedding, and he there recognised a specimen of the *Zygopteris Lacattii* of M. Regnalt. There was a difference between the Autun and Oldham specimens; for whilst the vascular bundles in the petiole of the former were shaped like a double anchor, in the latter they came nearly together and formed a circle; but he thought this difference scarcely sufficient to form another species.—Dr. J. P. Joule, F.R.S., described some experiments he had been making on the polarisation by frictional electricity of platina plates, either immersed in water or rolled together with wet silk intervening. The charge was only diminished one half after an interval of an hour and a quarter. It was ascertained both in quality and quantity by transmitting it through a delicate galvanometer. He suggested that a condenser on this principle might be useful for the observation of atmospheric electricity.—"On an Electrical Corona resembling the Solar Corona," by Prof. Osborne Reynolds.—"On the Electro-Dynamic effect, the induction of Statical Electricity causes in a moving body. The induction of the Sun a probable cause of Terrestrial Magnetism," by Prof. Osborne Reynolds.

Edinburgh

Royal Physical Society, February 28.—Dr. James M'Bain, president, in the chair. The following communications were read:—"On the Dentition of *Echinorhinus spinosus*," by Prof. Duns. Dr. Duns has obtained two specimens of this rare shark in the Firth of Forth, one in 1868 and another in 1871. The former is in the Scottish Natural Museum, the latter in the Museum of the New College. The specimens noticed by Yarrell were referred to, and the form of the teeth of the 1868 example shown. The remarks of Agassiz were quoted on the resemblance of the teeth of *Echinorhinus* to those of his genus *Goniodus*. It was shown, that while in other specific features the specimen of 1871 resembles those of that got in 1868, it differs very widely in the form of the teeth.—"On Garnetiferous Limestone, Balmoral," by Prof. Duns.—"On the Preservation of Compound Ascidians," by Mr. C. W. Peach. Mr. Peach stated that when living at Cornwall he was much struck by the beauty of the compound ascidians, so abundant on rocks, &c., between tide-marks there, and that he was perfectly aware that the beauty of the colours and flower-like systems of these lovely objects was always lost, whether they were preserved in spirits or any other fluid. He thought of Canada balsam—the great difficulty of contending with wet objects suggested itself. He, however, tried, and so far succeeded, by laying them on glass, (when detached from the rocks), after squeezing out as much as possible of the moisture by first laying them in cotton or linen rag between sheets of blotting paper, changing these as often as required, and doing all as quickly as possible, after taking the object from the sea. Thus dried, they were placed on glass covered with warmed Canada balsam, and covered with another similarly prepared plate of glass, on which sufficient balsam was melted to cover up completely the specimen. It is then allowed to cool under slight pressure, the superfluous balsam scraped off, and sealing-wax put round the edges to form a cell, and thus they were preserved. He exhibited several specimens—some preserved twenty-five years ago—of Leptoclinum, Botryllus, Didemnum, Paracidra, &c., in a beautifully preserved condition.—Mr. Peach exhibited a number of fossil plants he had collected last summer from the coal-fields of Edinburgh, Slamannan, Bathgate, and Devonside near Tillicoultry.—"On the Phosphate Deposits of South Carolina," by Prof. Pratt, Charleston, U.S.—Mr. John Hunter exhibited a series of fossils from the same region.

Dublin

Royal Irish Academy, February 12.—Rev. J. H. Jellett, president, in the chair. Dr Eugene A. Conwell read a paper on the identification of the ancient Cemetery at Loughcrew, Co. Meath.—Dr. W. Frazer read notes on several finds of silver coins lately made in Ireland.

Paris

Academy of Sciences, February 19.—The dispute concerning the accuracy of the results published by the Paris Observatory was carried on rather briskly by MM. Serret, Le Verrier and Delaunay.—A note by M. Zeuthen on the determination of the characteristics of the elementary systems of cubics was presented, with remarks by M. Chasles.—M. Ciotti claimed the originality of his researches on the employment of vibratory elastic laminæ as a means of propulsion.—M. Delaunay communicated some remarks on the experiments of M. Wolf, on the reflecting power of silvered glass mirrors.—Numerous reports on the aurora of February 4 were presented, and also a note by Marshal Vaillant on the phenomena which give rise to auroras, a note by M. H. Tarry on the origin of polar auroras, and a memoir by M. Silbermann on the facts from which we may deduce a theory of auroræ borealis and australis founded on the existence of atmospheric tides, and the indication, by means of auroras, of the existence of flights of meteors in proximity to the terrestrial globe.—Marshal Vaillant regarded auroras as produced by the reflection from the surface of the terrestrial atmosphere of the light produced by electrical or magnetic currents. M. Tarry ascribes to these phenomena a cosmical origin.—A note by M. J. L. Soret on the induction currents produced in the coils of an electro-magnet when a metallic mass is set in rotation between its poles was read.—M. H. Sainte-Claire Deville presented a note by M. E. Branley on the measurement of the polarisation in a voltaic element.—A note by M. Respighi on the spectral analysis of the zodiacal light was read, in which the author detailed some interesting observations on the spectral phenomena presented by the zodiacal light and auroras tending to indicate the identity of origin of the two phenomena.—M. Delaunay presented a note by MM. Loewy and Tisserand on the search for the last planet (99) Dike.—MM. J. Pierre and E. Puchot communicated some facts in the history of propylic alcohol, relating chiefly to the behaviour under distillation of the so-called monohydrate of that body.—M. G. Tissandier communicated a note on a new mode of producing anhydrous protoxide of iron by the action of carbonic acid upon iron heated to redness. The author describes the properties of the oxide thus prepared.—A memoir was read by M. E. Duclaux on iodide of starch, which he does not regard as a regular chemical compound.—A note by M. Blondlot on the alcoholic fermentation of sugar of milk was read. The author described the fermentation of milk when agitated from time to time, by means of a ferment apparently proper to it, and stated that this fermentation was continued by the addition of sugar of milk or glucose to the fluid after the cessation of the first fermentation. He obtained alcohol by the distillation of the fermented product, and regarded his results as favourable to the theory of fermentation of M. Pasteur.—M. Pasteur criticised the recent communications of M. Fremy on the subject of fermentation, discussing his experiments *seriatim*, and indicating objections to them.—M. S. de Luca presented some investigations upon the composition of the gases which are evolved from the fumaroles of the solfatara of Pozzuoli, upon which M. Boussingault made some remarks.—The processes for the preservation of wines by the application of heat formed the subject of notes by M. A. de Vergnette-Lamotte and by Dr. Bart.—M. E. Alix noticed the existence of the depressor nerve in the hippopotamus, and stated that it resembles that of the horse in arrangement, but is thinner coinciding with the small size of the primitive carotid.—M. A Béchamp presented some observations on a recent note by M. de Segnes upon microzymes.

February 26.—The following mathematical papers were read:—An exposition of a geometric theory of the curvature of surfaces, by M. A. Mannheim, presented by M. Serret; a note on some relations between the angular quantities of convex polyhedra, by M. L. Lalanne; and a determination of the characteristics of the elementary systems of cubics, by M. Zeuthen, communicated by M. Chasles.—M. de Saint Venant read a memoir on the hydrodynamics of streams.—M. Phillips presented a note on the governing spiral of chronometers, and M. de Pambour a second paper on the theory of hydraulic wheels, relating to the reaction wheel.—A letter from Father Secchi on the aurora of February 4, and on some new results of spectrum analysis, was read, containing a description of the appearances observed at Rome, with a notice of the phenomena presented by spectrum analysis, and a discussion of the supposed relation between auroras and the solar protuberances, which the author is not inclined to accept. In a postscript M. Secchi calls attention to the appearance of remarkably distinct bands and lines upon the planet Jupiter.—A communication was read from Prof. Piazzi Smyth, on the brilliant yellow band in the spectrum of auroras, which he stated to be of constant occurrence, and to fall always upon the line 5579.—M. A. Laussedat also presented a memoir on the aurora of February 4, and M. C. Sainte-Claire Deville a continuation of M. J. Silbermann's memoir on the theory of auroras, and on the indication by their means of the existence of flights of asteroids in proximity to the earth.—M. C. Sainte-Claire Deville also read a note on the probable application of quadruple, dodecuple, and tridodecuple symmetries, or of periods of 90, 30, and 10 days, to the mean returns of the electrical phenomena of the atmosphere, such as storms and auroras.—M. E. Becquerel presented a memoir by M. G. Planté, on the employment of secondary currents to accumulate or transform the effects of the galvanic battery, containing the description of improvements in the arrangements previously suggested by him.—M. H. Sainte-Claire Deville communicated a note by M. J. M. Gaugain on the electromotor forces developed by the contact of metals with inactive fluids, containing the discussion of results obtained with plates of platinum in distilled water.—The question of priority in the invention of the method of preserving wines by the action of heat was treated at some length by M. Balard, to whom M. Thenard replied.—M. Tellier forwarded a further communication on his system of producing cold by the evaporation of ether, assisted by compressed air.—M. Wurtz presented a note by M. E. Reboul on two new isomers of bromide of propylene.—M. J. Personne read a note on iodide of starch, in answer to one presented by M. Duclaux at the last meeting. M. Personne claims to have arrived six years ago at the conclusion that the so-called iodide of starch is not a chemical compound.—A note by M. Marey, on the determination of the inclinations of the plane of the wing at different moments of its revolution was read.—M. C. Bernard presented a third note by M. P. Bert on the influence which changes in barometric pressure exert upon the phenomena of life, in which the author described the effects produced by exposing small animals to various degrees of atmospheric pressure. He has found that up to a pressure of two atmospheres sparrows die when the air in the receiver contains 25 per cent. of carbonic acid, but that above this limit and below a pressure of 25 centims., this law does not apply. In the former case the birds perish partly by the toxical effects of an excess of oxygen, and in the latter by a privation of oxygen.—M. C. Bernard also communicated a note by M. N. Gréhant on the respiration of fishes, containing a statement of the curious fact that fishes in respiration can avail themselves not only of the oxygen dissolved in the water, but also of that held by the red corpuscles of the blood of other animals when these are mixed with the water.—A note by MM. L. Labbé and G. Guyon on the combined action of morphine and chloroform, was also presented by M. C. Bernard. The authors state that a state of perfect anæsthesia may be produced and sustained for a long time without the usual danger, by administering a subcutaneous injection of hydrochlorate of morphine about a quarter of an hour before the exhibition of chloroform.—M. A. Béchamp read a paper "On the Essential Nature of the Organised Corpuscles of the Atmosphere, and on the part which belongs to them in the phenomena of Fermentation."—M. S. Meunier presented a note on the existence of bauxite in French Guiana.

VIENNA

Geological Institution, February 6.—Dr. Neumayr, "On the Jurassic Provinces of Europe." The author stated the different development of the Jurassic strata in three regions of Europe. To the Mediterranean province belong the Jurassic beds of Spain, and of the Alpine and Carpathian districts; secondly, the middle European province is formed by the Jurassic beds of England, France, and Northern Germany; while to the third, the Russian province, belong the Jurassic beds of Russia, as well as those of Spitzbergen and Greenland. The only really important diversity between the Jurassic strata of these provinces is founded, as he shows, on differences in the zoological characters of their faunas. Thus, for instance, the most prevalent peculiarity of the Mediterranean province is the presence of Ammonites of the two genera, Phylloceras (*Heterophylli*) and Lytoceras (*Fimbriati*), which abound in almost all members of the Jurassic formation in the Alps and Carpathians, while they are almost entirely wanting in the middle European province. The Russian province, on the contrary, is chiefly characterised by the absence of reef-forming coral and some other peculiarities. It is impossible to account for this difference by the supposition of land having separated the Jurassic seas of the different provinces. The fact that along the line of separation between the Mediterranean and middle European provinces, from the South of France to the Crimea, strata of both provinces approach very near, even to a few miles, excludes this supposition. The only possible mode of explanation the author finds in accepting in the Jurassic period climatic differences in the zones from north to south. The strict separation of both faunas along the said line may be explained, he thinks, by a great stream of warm water, which produced similar effects to the Gulf Stream in our time.—Dr. G. Pilar, "On the Tertiary deposits in the valley of the Culpa, in the environs of Glina, in Croatia." Very instructive sections have been denuded in these deposits by the Culpa river. The marine beds, as well as the Sarmatic and the Congeria beds are developed; all abound with fossils.

DIARY

THURSDAY, March 7.

Royal Society, at 8.30.—On the Organisation of the Fossil Plants of the Coal Measures. III. Lycopodiaceæ, by Prof. W. C. Williamson, F.R.S.
Society of Antiquaries, at 8.30.—Exhibition of a large collection of Photographs and Drawings of Irish Architectural Remains anterior to the 12th Century, made by the late Earl of Dunraven, F.S.A., with Remarks by Miss Stokes.
Chemical Society, at 8.
Linnean Society, at 8.—Revision of the Genera and Species of Scilleæ: J. G. Baker.—Andræcium in Cochliostema: Dr. Masters.
London Institution, at 7.—A Vindication of our Monetary Standard, with an Exposition of its Internal Relations: J. A. Franklin.

FRIDAY, March 8.

Royal College of Surgeons, at 4.—On the Digestive Organs of the Vertebrata: Prof. Flower, F.R.S.
Astronomical Society, at 8.
Quekett Microscopical Club, at 8.
Royal Institution, at 9.—On the Effect of certain Faults of Vision on Painting, with especial reference to Turner and Mulready: R. Liebreich.

SATURDAY, March 9.

Royal Institution, at 3.—Demonology: M. D. Conway.

MONDAY, March 11.

Royal Geographical Society, at 8.30.
Royal College of Surgeons, at 4.—On the Digestive Organs of the Vertebrata: Prof. Flower, F.R.S.

TUESDAY, March 12.

Photographic Society, at 8.—Retouching, its Use and Abuse: Valentine Blanchard.
Royal Institution, at 3.—On the Circulatory and Nervous Systems: Dr. Rutherford.

WEDNESDAY, March 13.

Royal College of Surgeons, at 4.—On the Digestive Organs of the Vertebrata: Prof. Flower, F.R.S.
Society of Arts, at 8.—On the British Trade with France during the last Ten Years, in its relation to the General Trade of the United Kingdom: Leone Levi.

THURSDAY, March 14.

Royal Society, at 8.30.
Society of Antiquaries, at 8.30.
Mathematical Society, at 8.—Shall the Society apply for a Charter?
Royal Institution, at 3.—On the Chemistry of Alkalies and Alkali Manufacture: Prof. Odling, F.R.S.

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Errata.—P. 341, first col., line 32, for "and should be changed," read "and should not be changed." P. 338, first col., line 3, for "J. Murray" read "Tinsley Brothers."

NOTICE

We beg leave to state that we decline to return rejected communications, and to this rule we can make no exception. Communications respecting Subscriptions or Advertisements must be addressed to the Publishers, NOT *to the Editor.*

THURSDAY, MARCH 14, 1872

LA SEINE*

IN carrying out the great works for the improvement and embellishment of Paris under the late Empire, all incidental discoveries of objects relating to art, history, and science, were systematically investigated, recorded, and preserved, instead of being left to the chance and uncertain description of casual and independent observers. In a liberal and enlightened spirit the Municipality of Paris and the Préfet of the Seine (M. Haussmann) established a proper organisation and a staff (*Service des fouilles et des substructions*) to follow up such discoveries, to take plans of old works, to preserve all art treasures or objects of scientific value; to note, in fact, and to investigate everything of interest. Men eminent in several departments were consulted, and engaged to draw up reports with full illustrations of the discoveries. By these judicious measures, the knowledge of the topography, antiquities, and archæology of Old Paris has been greatly advanced. Works of the Roman, Gallic, and Mediæval periods have been brought to light, surveys and plans made, and the more important specimens preserved *in situ* or in the public museums.

To M. Belgrand, the eminent and able engineer for the water supply and drainage of Paris, was deputed the work of recording all the geological and some of the archæological facts discovered during the construction of the large works on which he was engaged.

Paris up to the last few years had been supplied with water from local sources (river, canal, and wells), but as these were found insufficient and of indifferent quality, it was determined to seek for other and better sources of supply at a distance, and some large springs in the chalk district, respectively distant sixty and eighty-four miles from Paris, were eventually selected by M. Belgrand, and their waters were brought to Paris by means of aqueducts on a high level. In carrying out this great work, M. Belgrand made himself intimately acquainted with the hydrography of the Basin of the Seine. He explored every valley, and determined the *régime* of every important river. The result of the first part of the inquiry appeared in a valuable series of tables, showing the connection between the rainfall and the discharge of each river—the extent and nature of the floods, and the geological character of the ground with reference to the range and extent of the permeable and impermeable strata, and which he illustrated by a specially coloured map. In connection with the construction of the aqueducts, M. Belgrand had also to ascertain the nature of the surface and the contours of the hills and great plains along which he carried them, and to examine the many pits whence the materials for construction were obtained. This geological investigation led to the discovery of many interesting specimens, and further suggested many theoretical inquiries relating to the origin of the present surface, and to the *régime* of the old Seine during the later geological periods. The result of the inquiry is embodied in the three handsome quarto volumes before us—one of 255 pages of text, with 105 pages of introduction, descriptive of the country and giving the theoretical views; a second containing plates of fossils, of flint implements, and pit sections; and a third with extended coloured sections and a monograph by M. Bourguingnat of the shells found in the Drift beds.

Paris stands on Tertiary strata, from beneath which, at a distance of some miles, the chalk crops out and forms a belt many miles in width. These formations constitute a table land having a height of 100 to 200 feet along the sea coast of Normandy, and rising from 500 to 600 feet inland in Champagne. This district is traversed by the Seine and its tributaries, flowing in comparatively narrow valleys cut deep into the table land; while, on the extended upland plains thus formed, there rise, here and there, ranges of hills of Fontainebleau Sands or other later Tertiary strata. The strike of these hills is in a direction entirely distinct from that of the hill slopes flanking the river valleys and forming part of the present river-system. The latter range in various directions—north, north-east, south, and south-east—in accordance with the direction of the tributaries of the Seine until they join that river, the main channel of which has, from Montereau to the sea, a general direction south-east to north-west. M. Belgrand found that the hills on the plains nearly all ranged in this one given direction, or approximately from south-east to north-west, with intervening valleys having the same direction. Numerous such ridges, none being of any great length and all narrow and having this definite trend, are found to extend over the whole plateau area uninfluenced by the more tortuous deeper river-valleys which intersect the same area at various angles to their course. The river-valleys are covered with gravel formed of the *débris* of the rocks through which the present rivers flow, while the plateau valleys and plains are free from such *débris*, but are covered with a uniform layer of red clay or loam. Whence M. Belgrand concludes that the two systems of valleys have a different origin. He contends that it is not possible to have a true river channel without having more or less drifted gravels formed by the constant action of running water and by floods, and therefore that these higher valleys could not have been formed by river action, while at the same time their rectilinear and special bearing indicates that their formation is due to one common and independent cause.

M. Belgrand considers that the only explanation which will account for the phenomena presented by these higher-level valleys and hills, is the rapid and transient passage of a large body of water over the surface; and as the excavation of these higher valleys took place after the formation of the Fontainebleau Sands and of the Calcaire de Beauce (Miocene), and before the Pliocene period (for the *Elephas meridionalis* of the valley of the Eure shows that the land had then emerged), and as also, according to M. Elie de Beaumont, the elevation of the main chain of the Alps took place at the same period, M. Belgrand connects the two events and supposes that the sea of the Pliocene deposits of the Alpine area was thereby displaced and that it swept over this northern portion of France, denuding the softer portions of the strata and leaving narrow ridges of the harder portions

* *Le Bassin Parisien aux Ages Antéhistoriques.* Par M. Belgrand, Inspecteur-Général des Ponts and Chaussées, Directeur des Eaux et des Egouts de la Ville de Paris. (Paris: Imprimérie Imperial.)

all trending south-east to north-west (or in the direction from the Alps), standing out, on the denuded high plains, as monuments of its passage. M. Belgrand points out that where the Tertiary strata have presented a resistance which the waters could not overcome, the high-level valleys formed by the diluvial waters are, in such cases, fronted in the opposite range of hills, against which the mass of waters impinged, by a deep bay cut by the current in those hills, and that the waters thus checked in their course were turned off at acute angles, until they reached the main channel of the Seine, tending thereby to form secondary or tributary valleys, which, when the deluge had passed, contributed, with the Seine valley, to form the present lines of river drainage. Such volumes of water as we have depicted would, he argues, have swept the higher channels and plains clear of *débris*, leaving the denuded area covered merely with the silt thrown down from muddy waters, and depositing the coarser *débris* in the middle and lower range of the deeper channels through which the present rivers afterwards took their course. In support of this hypothesis, he shows that, whereas the basin of the Seine is now drained by the one river and its tributaries, the diluvial waters held their course straight across that basin and debouched in five main channels—one, marked by the hills of Montmorency and Satory, took the course of the Seine below Montereau to the sea, but in a more direct and broader line; the second took the course shown by the hills of Villers-Cotterets, thence across the present valley of the Oise, along the valley of the Pays de Bray, to the sea at Dieppe; the third followed in part the course of the Aisne, and then by the line of the Somme valley to the sea; and the fourth and fifth by those of the valleys of the Aulthie and Cauche. M. Belgrand accounts for the radidity and force of this cataclysm in the belief, which he shares with M. Elie de Beaumont, that the elevation of the Alps took place rapidly and suddenly.

But there was a second elevation of the Alps, at a later geological period, and which, according to M. Belgrand, may have produced a second deluge, not by the displacement of the sea, for then there were only lakes on the north-western side of those mountains, but by the sudden melting of the snow on that great range; and our author again adopts the views of M. Elie de Beaumont on this subject. This distinguished geologist propounded in 1847 the theory that that last convulsion of the Alps was accompanied by an enormous disengagement of those gases to which has been attributed the formation of the Dolomites and Gypsum beds of that chain, and that this caused the accumulated snows to melt in a very brief period of time (*un instant*). At the same time, according to the same authority, the Pliocene lakes of "La Bresse" were raised and drained. Thus, suggests M. Belgrand, this second convulsion might have caused another diluvial wave to pass over the basin of the Seine—an hypothesis also advanced by M. Elie de Beaumont, who speaks of "the probable concourse in this off-throw flood (*déversement*) towards the north-west, of the waters of the great lake of La Bresse, in the production of the diluvial phenomena observed in the neighbourhood of Paris."

We are disposed to agree with our author in the opinion, which we have elsewhere expressed, that the original contour of the surface with its higher valleys and hills, is due to a cause different from that which excavated the present river valleys—that it preceded and is independent of it—but we cannot agree with him as to the nature of that cause. Without going far into the argument, we may mention that the well-known fact of the gravel found in each tributary of the valley of the Seine, consisting of the *débris* of those rocks only through which that tributary flows, while in the Seine valley are found the *débris* of all the tributaries, together with its own and no more, is, it seems to us, a conclusive argument against the passage of a body of water from one great basin to another—against the flow of such a body of water from the Alps across the Jura, the great plains of the Doubs and the Soane, the southern prolongation of the Vosges, and, over the separating water-shed formed by the lower hills of Burgundy, to the Seine basin, and so to sea on the northern shores of France. Such a cataclysm must surely have spread the *débris* of the strata destroyed in its course north-westward along the tract over which it flowed. Some remains of the rocks of Switzerland, of those of the Vosges and of Burgundy, must surely have been detected in the course of its passage. How can the author account for the large blocks and abundant *débris* of the Seine valley—which blocks and *débris* he considers as originally due to this cataclysmic action—and yet overlook the almost necessary consequence of the introduction of some foreign elements into the Seine Basin, whereas none such exist. Not only is the *débris* of each great asin restricted to its own rocks, but even each tributary river valley has its own special rock *débris* and no other. M. Belgrand remarks, it is true, of the Somme Valley, which lies on the line of his third great diluvial water channel, and which prolonged south-east passes across the Oise valley and up that of the Aisne, that some *débris* of the older rocks of the latter areas have been found in the chalk valley of the Somme. But we must confess we have never found a trace of such a mixture, and we have particularly examined the Drift of those areas with a view to the determination of this point. At the same time the water-shed between the two valleys is so low that their complete separation in old times appears to us more remarkable than their present independence, and we can quite conceive the possibility of the Oise waters, when that river flowed at its higher level, passing at periods of flood into the valley of the Somme, and so carrying some small amount of *débris* across the present water-shed, especially as so good an observer as M. Buteux is referred to as the authority for this fact. If there, however, it is evidently quite the exception, and may be accounted for as just suggested.

With regard to the ingenious suggestion of M. Belgrand that some south-east and north-west valleys of the table-lands are faced on the opposite side of intersecting river valleys by a bay in the hills due to the violence of the checked diluvial waters, such for example as the amphitheatre in the hills on the west of the River Ecolle between Milly and Moigny and again at Soissy, it is to be remarked that such amphitheatres exist equally on the opposite or lee side of the hills towards La Ferté-Aleps and Maisse; and, further, that, in the same Tertiary area beyond the intersecting range of hills between the Ecolle and the Essonne (which according to M. Belgrand's views should have acted as a breakwater), the south-east and north-west ridges again resume between the valleys of the Essonne and the Eure.

After the contour of the surface produced by this cataclysm, and by which M. Belgrand considers that all traces of any previous river courses must have been obliterated, the Seine and its tributaries began to flow at an elevation estimated by him of from 80 to 100 feet above the present level. This he proves, as we have already done, by the occurrence of the remains of land mammalia and of river and land shells in beds of Drift at that elevation above the Seine on some of the hills near Paris. This part of M. Belgrand's work is admirably illustrated, both by general and local sections, and contains valuable lists of the mammalian remains, in the determination of which he had the advantage of the high authority of the late M. Ed. Lartet. Here again we cannot, however, agree with him in his *modus operandi.* The great boulders of sandstone, meulière, granite, &c., found in the valley gravel of the Seine, are attributed by M. Belgrand in the first place to removal to the line of the Seine valley by diluvial action, and subsequently to their drifting along the valley channel by the river action during floods of the Quaternary period, and he gives some remarkable instances of the power of water to remove large blocks, and of the rate at which such blocks move. When, however, it is considered that the granitic rocks of the Morvan have been transported some 150 miles, and other rock boulders in proportion, that the angles of many of the large blocks of sandstone and of meulière constantly show little wear, and that they are dispersed irregularly and at various levels, some imbedded in soft clays, and others in sand or fine gravel and that these latter are often twisted and contorted, we can only explain the phenomena by the action of river ice and transport thereby.

M. Belgrand, on the other hand, shows that a prolonged and steady fall of rain, even if not very heavy, during the winter, now produces great floods—that such rivers as the Yonne and Cure flowing over impermeable strata are subject to sudden and great freshets after a heavy but short fall, whereas the Marne and Seine flowing over permeable strata have their floods retarded, but, at the same time, rendered more permanent by the rainfall having to pass through the strata and delivered in springs. He also shows that when the permeable strata become saturated by long-continued rains, they act as impermeable strata, and that the floods follow close on the rainfall besides being long maintained, so that in the remarkable and long wet winter seasons of 1658 and 1802 the Seine rose at Paris in the one case 29 feet, and in the other $24\frac{1}{2}$ above its ordinary low level, and the floods in the last case lasted three months. M. Belgrand considers that this state of things was a normal condition during the Quaternary period, and he sees reason to believe that the rainfall at that period must have been very much greater than at present.

The ordinary low-water discharge of the Seine at Paris is 75 cubic metres per second; but during these great floods it rose to 2,400 and 2,000 cubic metres. M. Belgrand gives a list of eight such floods in the last two centuries, during which the discharge was above thirty times greater than the ordinary low-water discharge. In rivers flowing over more impermeable strata the difference is still greater; and he mentions that in the Loire at Orleans it has amounted to as much as 400 times, or 25 : 10,000. We may take the width of the Seine valley during the high-level gravel period at six kilometres, and during the low-level gravel period at about two kilometres; and M. Belgrand estimates that the river in flood had in the first instance a sectional area of 60,000 square metres, and in the second of 40,000 metres; and, calculating the flow at a given rate per second, the discharge, as compared with that of the present river, would be as under:—

Discharge per second of the Seine at Paris in the present period and during floods in past periods:—

		Extreme rise of river. Metres	Discharge of river. Cubic Metres
Present River	low water	8·81	75
	flood-water		2,400
Old River during the Quaternary period	low level stage 20 / high level stage 13		27,000 to 60,000

Large as these Quaternary period quantities are, M. Belgrand thinks that there are cases of recent occurrence to prove that it is possible to realise them. He mentions a flood following on a heavy rainfall in the valley of the Armançon, a small river flowing over impermeable strata, with a basin of only 1,490 square kilometres, which had its discharge raised for a short time to 800 cubic metres per second; and he infers that under like conditions of rain and impermeability (by saturation and otherwise) the Seine, with its basin of 78,600 square kilometres, might have its discharge raised to 42,444 cubic metres, showing, that notwithstanding the size of the old river channels, the area drained during a period of greater rainfall would have sufficed for the necessary water supply.

In confirmation of this larger and more permanent supply of water, M. Belgrand instances the presence of the Hippopotamus, the remains of which are found at several places in the Seine basin as well as in that of the Somme, and which would have required for its existence larger and fuller rivers. He also derives a further argument in the presence of this animal, against a prolonged and severe winter cold, which he considers would have been fatal to it. M. Belgrand, nevertheless, argues that the presence of the Reindeer indicates the six summer months temperature of Scandinavia, not exceeding in the mean 8° centigrade; but with such a summer temperature we hardly see how he can avoid the three months' winter temperature of the same latitude or of 4·6 per cent. A still more extreme winter temperature is in fact indicated by the presence of the Musk Ox and the Marmot. It is to be observed also that the Reindeer at that time lived as far south as the Pyrenees, and that the physical condition of the drift deposits are, as we have before shown, strictly in accordance with a very low winter temperature. As the Hippopotamus is an extinct species, we do not know how far it may, like the extinct Elephants and Rhinoceroses, have been adapted to live in a severe climate. M. Belgrand's work is full of interesting details of the distribution of these and the other Quaternary animals, not only over the Seine Basin, but in some cases over the whole of France. He gives also a monograph with figures, by M. Bourguingnat, of all the mollusca of this age found in the Seine Basin. This well-known conchologist makes out that out of a total of 76 there are 38 new species which he considers as extinct, a conclusion which we expect English conchologists will hardly be prepared to agree with, as they have detected no extinct species in these deposits, and find only a few which are not local--a view in which we also believe most French conchologists join. The author considers that the same mammalian fauna is common to both the high-level and the low-level gravels. In one main point, however, do these gravels differ. In those of the high-levels of Montreuil and Bicêtre no Human remains, no Flint Implements, have been found, whereas, in those of the low-levels of Clichy, Grenelle, &c., above 5,000 flints, more or less worked, are stated to have been found by a single collector. Besides these works of early Man, M. Belgrand states that human bones, skulls, and entire skeletons, have been found in these lower gravels; but it seems to us that much of this evidence requires confirmation.

The Quaternary period of the Seine Basin is coeval, in M. Belgrand's opinion, with the Glacial period, and he considers that it was brought suddenly to a close with the low-level gravels. To this Quaternary period the peat deposits immediately succeed, owing, as the author ingeniously suggests, to the suddenly diminished rainfall leaving the rivers clearer and under conditions favourable for the growth of peat, which he shows never takes place in river valleys subject to frequent and heavy floods, but always in valleys where springs abound, and the floods are few and not turbulent.

The latter part of the work is occupied with a minute account of formation of gravel beds, and of the position of the Organic Remains, showing how all the features of those deposits are to be accounted for by ordinary river action, and that the mammalian remains are abundant precisely at those very places where a river with strong currents and numerous eddies would leave them. He endeavours to account also for the fact of all the bones of the larger animals being found in the coarser bottom beds of gravel, by the circumstance that these coarser beds were formed in those deeper water-channels along which only the larger carcases could have floated, and which were afterwards surmounted by those upper beds of sand and finer gravel, which he considers to be due to silting up (*alluvionnement*) of the channel where the river had changed its course to another channel. The brick earth or Loess is ascribed by him, as by English geologists, to river floods. But instead of considering it, as we do, to be produced by successive floods at all the various levels of the river, from the high to the low level, M. Belgrand admits but two levels, the high and the low, and that owing to a sudden elevation of the land, the excavation between these two levels was produced at once without intermediate stages. Consequently, he considers that the height of the Loess above these two levels marks in each case the rise of the flood waters. This, we think, is a weak point in his argument. According to his view, which he illustrates by a section, showing the range of the Loess up the hill slopes, he concludes that the floods of the low-level stage of the river rose, notwithstanding the width of the valley, to a height of 66ft., and during the high-level stage, to a height of 43ft., which give very much larger sectional areas for the river in flood than is otherwise necessary, and such as we conceive the area drained would have been insufficient to fill even with greatly larger rainfall. For, although the discharge of the Armançon may in a particular case of heavy rainfall have been so large as when multiplied by the whole area to give two-thirds of the required supply, still it is perfectly well known that the discharge by the main river never equals the sum of all its tributaries, and the discharge of the Seine at Paris on that occasion actually only rose to 1,250 cubic metres per second. There are besides beds of gravel on the slopes of Clichy towards Paris, and again on the slopes leading to Charenton distinct beds of gravel at intermediate levels, though of limited extent.

Thus, M. Belgrand ascribes the gravel beds and the Loess of the Seine Basin to old river action, referring the red loam alone of the higher plains above to diluvial causes, in opposition to the view usually received in France, according to which all these Drift beds are divided into the three diluvial deposits of *Diluvium gris*, *Diluvium rouge*, and *Limon* or *Loess*. As we have already expressed very similar views respecting the commonly accepted classification, we cordially agree with the author on this point.

The illustrations forming the second volume constitute a very interesting exhibition of the art of Photo-lithography. The execution varies a good deal, and there are plates which, though valuable for their truthfulness, are rather indistinct. Some of the representations of the Flint Implements are excellent. The work is somewhat large and costly; but as a copious record of facts, an ingenious statement of theory, and a reliable representation of specimens, this work of M. Belgrand will be greatly valued by all those who feel an interest in the remarkable phenomena connected with the present configuration of the country, the distribution of life during the Quaternary period, and especially with the evidence bearing on the Antiquity of Man.

J. P.

OUR BOOK SHELF

The Discovery of a New World of Being. By George Thomson. (Longmans, Green and Co., 1871.)

THE world discovered by this psychological Columbus is the "world of spirits," although he "disclaims all connection with so-called Spiritualists—a sect of modern times," whom he somewhat ungenerously "believes to be either dupes or knaves." Mr. Thomson believes that man consists of two "personalities," an animal personality or body, and a personality he calls spirit, which is the "knowing and conscious we," and which he believes to be as distinct from and as capable of being at almost any moment abstracted from the former as steam is from a steam-engine. Indeed, this latter phenomenon takes place every time the body "goes to sleep," to use the vulgar phrase; for Mr. Thomson believes that the "animal life never sleeps, and cannot sleep, and that to say or think that it, or any other life, can sleep, in the popular sense of the word, is the most glaring absurdity that ever has had possession of the human mind." "What is meant properly by sleep," he goes on to say, "is simply the abstraction or withdrawal of the influence of a being, a spirit, from a being, an animal, the leaving of a servant to itself, from the influence of its lord and master." Mr. Thomson explains the phenomenon of dreaming to be the struggles of this "being, a spirit," to get out of and back into the house of its servant, the body. The frequently unpleasant consequences of a late supper might have led Mr. Thomson one step further, and suggested to him the probable habitat of the spirit when embodied. How brimful of meaning to Mr. Thomson, then, must be Shakespeare's well-known utterance—"We are such stuff as dreams are made of." The particular merit which he claims for himself as a discoverer is, that he has realised to himself this spirit-world "predicted of old to be in existence," become conscious of himself as a "spirit in the world of spirits," clearly distinct, "in rounded belief," as he puts it, from that other entity, the body; and he declares that any one may make this awful discovery for himself if he only has "faith," shuts himself off from the outer world, and ponders long enough and with sufficient intensity. If our author is really in earnest—and we cannot but think he is—in trying to fathom the mystery of life and of consciousness, we recommend him to approach the subject unprejudicedly from the side of physiology; for so long as a psychologist concerns himself with the phenomena of his "inner consciousness" alone, and neglects the facts of his "outer man," his work is less than half done, and he is as likely to succeed in arriving at the whole truth as Columbus would have been in discovering America, had he contented himself with studying charts and staring longingly across the Atlantic for forty years.

On the Elevation of Mountains by Lateral Pressure; its Cause, and the Amount of it, with a Speculation on the Origin of Volcanic Action. By Rev. O. Fisher, M.A., F.G.S., &c. (From the Trans. of Camb. Phil. Soc. Vol. xi. part iii.)

THIS paper is of considerable interest as bearing upon the question of the internal condition of the earth. Mr. Fisher is of opinion that the elevation of mountain chains and the phenomena of volcanoes can both be accounted for on the hypothesis that the earth is solid. He conceives that "if a sufficient loss of heat has happened since the stratified rocks were formed, to cause a slight diminution in the volume of the earth, then the outer layer will have become too large, and will have had to accommodate itself to the reduced spheroid; and the lateral pressure caused by the resulting failure of support will have given rise to those foldings which have produced mountain ranges;" and an attempt is made by the author to "estimate the lateral pressure which would arise in the outer strata of the earth under such circumstances." Referring to the results obtained by Archdeacon Pratt in India, which seem to show that the density of the earth's crust beneath mountain chains is less than in other places, the author thinks this is only what might have been expected upon the supposition that the elevation of these mountains is due to lateral pressure; for it is evident that the strata would to some extent be supported by the lateral pressure which upheaved them. Here then, he thinks, may be the origin of volcanoes:—Diminished vertical pressure will enable the interior layers of the crust to pass into a state of fusion, and, "if from an independent cause a partial passage towards the surface is opened for molten rock containing highly heated water, the fluid will convey to a level where the resistance is less the pressure existing at a lower depth, and the force necessary to complete a passage to the surface may be furnished by the pressure of the molten rock and by the steam contained within it." But, although Mr. Fisher believes that the elevation of mountain chains and the phenomena of volcanoes are both of them the result of the same fundamental causes, yet, he thinks, it would certainly be a mistake to regard elevation as the consequence of volcanic action. He does not see how subterraneous lakes of molten matter can account for the elongated form which trains of volcanoes like those of the Andes affect; nor how such lakes should have shifted about from one region to another at different geological epochs. His theory, however, offers an explanation of the elongated form assumed by chains of volcanoes—the shifting of volcanic activity to different regions at successive periods—the spasmodic character of volcanic action, and other volcanic phenomena. 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.*]

The Placental Classification of Mammals

A REMARK made by Prof. Allen Thomson on this subject in a late number of NATURE induces me again to draw attention to some objections I offered to the placental classification in a review of Prof. Rolleston's "Forms of Animal Life" (NATURE, vol. i., p. 81). If this system fails to satisfy so sound a critic and so accomplished an anatomist as Dr. Thomson, there must be some serious deficiencies in it. No doubt De Blainville did good service in calling attention to the wide distinction of Marsupials and of Monotremes from other mammals; but his names, *Ornithodelphia* and *Didelphia*, are inappropriate, and even misleading, and the skeletal characters of these two groups furnish quite as important, and far more available, means of diagnosis.

It admits of question whether the divisions of the higher mammals, according to the same system, are the most natural, even if the placenta were the best organ by which to define them. It is true, as Prof. Huxley observes, that the singularities which ally the elephant with the Rodentia have been a matter of common remark since the days of Cuvier, but the placental classification requires us to find still more singular ties between the elephant and the Carnivora. On the other hand the Carnivora lead down by the seals to the true Cetacea, a line of connection broken by the placental arrangement; which is equally opposed to the more doubtful analogy of the whales with the Ruminants. And the third order with deciduous zonary placentation, the isolated genus *Hyrax*, whatever may be thought of its relations to Rodentia on the one hand and to Ungulata on the other, has at least more likeness to either than to elephants and cats. Again, the different placentation of Edentata may be held only an additional proof of the looseness of an order held together chiefly by negative characters, but if we break it up, shall we obtain a more natural or convenient arrangement by placing the sloths with the Ruminants, *Manis* with Cetacea and Perissodactyla, and *Orycteropus* with Primates?

No doubt embryological characters are justly regarded as the most important for revealing true affinities between animals. But the tenacity of hereditary transmission, which gives them this value, does not appear to belong to placental structure. The placenta is more a maternal than a fœtal organ, especially as to its deciduate or non-deciduate character, and should rather rank with organs like the mammæ than with the yelk-sac and the amnion.

There are, moreover, many practical objections to the placental classification. The opportunities of obtaining knowledge on the subject are few, the investigation is not always easy, and it cannot be readily verified by subsequent observers.

But the most important objection to De Blainville's system is, that the perishable nature of the structures on which it is based renders it impossible to apply the criterion to fossil animals. It will probably be long before we shall have any notion of what a Sirenian placenta is like; it is only lately that we have learnt what is the real placentation of so common a creature as the rat, but we shall certainly never have the remotest idea of that of a megatherium, a zeuglodon, or a rhytina. So that if it be admitted—and surely no one will deny—that any classification of animals which is to be more than a mere aid to the memory, must include all known forms, recent or fossil, it follows that neither placenta, nor brain, nor any other soft part, can be of more than subordinate value in classification. On the other hand, it may be fairly maintained that there is no group of mammals, and scarcely one of the other Vertebrata, of undisputed importance, which cannot be completely defined by the characters of the skeleton.

It is, I venture to think, rather the authority of such illustrious names as Gegenbaur and Huxley than its own merits which have recommended the placental classification of mammals. If we regard the object of classification to be the setting forth of true genetic relationships, all characters must be included, and among them the placenta has no claim to be a primary index of affinity. And if we only seek for the most practically convenient way of arranging Mammalia, it is to the bones and teeth, rather than to the maternal organs of generation, that we must look.

P. H. Pye-Smith

Potential Energy

While on the subject of Thomson and Tait's Natural Philosophy, I should like to call attention to the definition of Potential Energy, given in Art. 273, p. 189.

I think it will be found that this definition gives the wrong sign, because the potential energy in any configuration is the amount of work the forces of the system perform in *returning* to the zero configuration, the ideal position of stable equilibrium.

Thus when a spring is stretched or compressed the potential energy is measured by the kinetic energy which is generated by the work done by the elastic force of the spring by the time the spring has returned to its unstretched condition. With this change of sign the definition now agrees with that given in Art 484.

Infinite distance being taken as the zero configuration, the potential energy is a positive quantity for such forces as electric and magnetic forces.

With this zero the potential energy for gravitating particles is negative, which is expressed by saying that the exhaustion of potential energy is positive, because as the particles approach their kinetic energy increases, and their potential energy suffers exhaustion and diminishes.

In Art. 485 we read, "The potential at any point, due to any attracting or repelling body or distribution of matter, is the mutual potential energy between it and a unit of matter placed at that point. But in the case of gravitation, to avoid defining the potential as a negative quantity, it is convenient to change the sign. Thus the gravitation potential at any point, due to any mass, is the quantity of work required to remove a unit of matter from that point to an infinite distance."

Although the gravitation potential has had its sign changed, nevertheless the potential at any point P for gravitation and for electric and magnetic forces, is defined in the same way as the sum of the quotients of every portion of the mass divided by its distance from P.

This is the Potential Function of Green, usually called by the name given by Gauss, the Potential, and is the function which satisfies Laplace's equation.

The gravitation potential is the old force function of Sir W. Hamilton and Jacobi, such that its rate of increase in any direction is the resolved part of the force in that direction on the unit of mass.

The potential, defined as the potential energy in the unit of mass is of opposite sign to the free function; its rate of decrease in any direction is the component force in that direction.

These perplexing changes of sign arise from the fact that in gravitation we have only one kind of matter, the particles of which naturally attract; hence the potential energy is negative, or it diminishes as the particles approach; it is, therefore, convenient to make a change of sign.

In the general case of which electrical and magnetical phenomena may be taken as the type, like particles repel, unlike attract, and the potential energy increases as the particles approach.

These definitions and conventions of signs are, of course, in accordance with those given by Thomson and Tait; the proper signs and names are given also in Briot's "Théorie Mécanique de la Chaleur," but in all the other French books there is great confusion; for instance, in the "Théorie Mécanique de la Chaleur" of Verdet, the potential goes by Green's name, the potential function, but has its sign changed, while the potential energy is called the potential, after Clausius. This also seems to be the nomenclature adopted by the Germans.

It is very necessary that all doubt as to the meaning and value of these important functions should be set at rest; the system adopted in Thomson and Tait's "Natural Philosophy" leaves nothing to be desired.

A. G. Greenhill

St. John's College, Cambridge, March 6

Development of Barometric Depressions

I leave to those who are equal to it the task of reconciling and discussing "J. K. L.'s" propositions in reference to Indian meteorology, which appear to be these:—1, "The rainfall in the Himalayas" (instanced by him in proof that rainfall is not the cause of depression), "probably causes a very great depression" (meaning, I now suppose, the great Asiatic depression really due to the rarefaction of the air in Central Asia); 2, "but certainly not any currents such as I have described" (viz., currents in accordance with Buys Ballot's Law, having the lowest pressure on their left); 3, "the circuit of the wind in the region of the Himalayas is, so far as we know, in exact accordance with Ballot's Law."

My complaint was that the critic had ignored, not, of course, Part II. of my book, but certain propositions in Part I., as "distinctly enunciated" as those on which he comments, and inseparable from them, though not yet fully discussed.

I will now close, as far as my part is concerned, a discussion, for the opening of which I was responsible, but which has, contrary to my intention, become rather personal than scientific. The question, however, really at issue between us I believe to be one of some interest in meteorology. "Does the fact that precipitation in certain cases, and especially in the warmer regions of the globe, fails to produce baric depression, disprove, or render improbable, the theory (based on substantial evidence) that the depressions which occur in Western Europe are results of precipitation?"

March 10 W. Clement Ley

A Safety Lamp

The article in this week's Nature on "Foul Air in Mines, and how to live in it" calls to mind a contrivance made use of by the watchmen of Paris in all magazines where explosive or inflammable materials are stored, and suggests the idea that the same may possibly be of service to our miners.

The Paris *Figaro* says, "Take an oblong vial of the whitest and clearest glass, put in it a piece of phosphorus about the size of a pea, upon which pour some olive oil, heated to the boiling point, filling the vial about one-third full, and then seal the vial hermetically. To use it, remove the cork, and allow the air to enter the vial, and then re-cork it. The whole empty space in the bottle will then become luminous, and the light obtained will be equal to that of a lamp. As soon as the light grows weak its power can be increased by opening the vial and allowing a fresh supply of air to enter. Thus prepared the vial may be used for six months."

4, Moreton Place, S.W. B. G. Jenkins

Beautiful Meteor

I enclose a description of meteor, apparently of unusual brilliancy, recently seen by my assistant at Parsonstown, thinking that it may perhaps be interesting to some of your readers.

Carlton Club, London, March 12 Rosse

"Observed an intensely brilliant meteor. It was first seen in the region about Lepus, whence it moved with a slow and steady motion across the heavens to the S.E. horizon, where it gradually disappeared in a bank of cloud at about $9^h\ 5^m\ 19^s$, Greenwich mean time, having occupied seven or eight seconds in moving over 50° of a great circle. The time given may be a few seconds wrong, as it was noted by an ordinary watch. The head was intensely brilliant, of a bluish white colour, and lighted up the whole sky.

"Its brightness was maintained during its entire visibility, and may have been as great as the moon at quadrature. Apparent diameter of the head 42′. It was followed by a very narrow tail about 3° in length and of a reddish hue. It did not leave any phosphorescent train behind it, but at the latter part of its course it threw out some reddish luminous masses, that gradually faded away. Its apparent course was in a great circle through β Canis Majoris to a point near the S.E. horizon, in azimuth S. $28\frac{1}{2}°$ E., and altitude $8\frac{1}{2}°$. For β Canis Majoris the azimuth was S. 20° 52′·4 W., and altitude 16° 43′·3.

"Observatory, Birr Castle, March 8"

While travelling last night, at about twenty minutes to nine o'clock, as we were descending a tolerably high hill, about 5 miles from this city, our road leading S.S.W., I found myself very favourably circumstanced for seeing a beautiful meteor which was visible for probably forty seconds. It appeared first as if approaching from the W.S.W. about 40° or 50° above the horizon, unusually large and bright, and leaving a long train of bright spots behind. After a few seconds it seemed extinguished, but in a moment or two flashed out again still brighter, apparently passing due E., at a height of about 25° or 30°, through Eridanus, Lepus, Canis Major, and Argo, and much slower than at first. While passing under Orion two protuberances burst out, giving it the appearance of an arrowhead, or rather a bird flying, as it appeared to have a tail which at the end was a fine smoke colour: it now occupied the space of 1½° or 2°. Passing behind a cloud below Regulus it disappeared.

Waterford, March 9 JAMES BUDD

"Whin"

CAN you or any of your readers furnish a probable etymology of the word *whin?* Over all the north of England and south of Scoland basalt is so called. Here we have the *whin*-sill or stratiform basalt—*whin*-dykes, or geological fissures filled with basalt. The vocabularies in treatises on geology give no derivation of this prevalent mining term. In Scotland *whin* seems to typify the hardest mineral known. Burns makes Death say in "Hornbuik," "I micht as weel hae tried a quarry o' hard *whin* rock." Surely a satisfactory root for the word in question can be found in Celtic, Old Norse, Danish, or Anglo-Saxon! The Old Norse "fors" is found in the names of several local waterfalls, as for instance "High Fórce" in Teesdale. At this "force" the river Tees is precipitated over a whin-stone cliff, 80ft. high.

WM. R. BELL

Laithkirk Vicarage, Mickleton, March 12

CUCKOO AND PIPIT

SEVERAL well-known naturalists who have seen my sketch from life of the young cuckoo ejecting the young pipit (opposite p. 22 of the little versified tale of which I send a copy)* have expressed a wish that the details of my observations of the scene should be published. I therefore send you the facts, though the sketch itself seems to me to be the only important addition I have made to the admirably accurate description given by Dr. Jenner in his letter to John Hunter, which is printed in the "Philosophical Transactions" for 1788 (vol. lxxviii., pp. 225, 226), and which I have read with pleasure since putting down my own notes.

The nest which we watched last June, after finding the cuckoo's egg in it, was that of the common meadow pipit (Titlark, Mosscheeper), and had two pipit's eggs besides that of the cuckoo. It was below a heather bush, on the declivity of a low abrupt bank on a Highland hill-side in Moidart.

At one visit the pipits were found to be hatched, but not the cuckoo. At the next visit, which was after an interval of forty-eight hours, we found the young cuckoo alone in the nest, and both the young pipits lying down the bank, about ten inches from the margin of the nest, but quite lively after being warmed in the hand. They were replaced in the nest beside the cuckoo, which struggled about till it got its back under one of them, when it climbed backwards directly up the open side of the nest, and hitched the pipit from its back on to the edge. It then stood quite upright on its legs, which were straddled wide apart, with the claws firmly fixed half-way down the inside of the nest among the interlacing fibres of which the nest was woven; and, stretching its wings apart and backwards, it elbowed the pipit fairly over the margin so far that its struggles took it down the bank instead of back into the nest.

After this the cuckoo stood a minute or two, feeling back with its wings, as if to make sure that the pipit was fairly overboard, and then subsided into the bottom of the nest.

As it was getting late, and the cuckoo did not immediately set to work on the other nestling, I replaced the ejected one, and went home. On returning next day, both nestlings were found, dead and cold, out of the nest. I replaced one of them, but the cuckoo made no effort to get under and eject it, but settled itself contentedly on the top of it. All this I find accords accurately with Jenner's description of what he saw. But what struck me most was this: The cuckoo was perfectly naked, without a vestige of a feather or even a hint of future feathers; its eyes were not yet opened, and its neck seemed too weak to support the weight of its head. The pipits had well-developed quills on the wings and back, and had bright eyes, partially open; yet they seemed quite helpless under the manipulations of the cuckoo, which looked a much less developed creature. The cuckoo's legs, however, seemed very muscular, and it appeared to feel about with its wings, which were absolutely featherless, as with hands, the "spurious wing" (unusually large in proportion) looking like a spread-out thumb. The most singular thing of all was the direct purpose with which the blind little monster made for the open side of the nest, the only part where it could throw its burthen down the bank. I think all the spectators felt the sort of horror and awe at the apparent inadequacy of the creature's intelligence to its acts that one might have felt at seeing a toothless hag raise a ghost by an incantation. It was horribly "uncanny" and "grewsome." J. B.

The University, Glasgow

* "The Pipits," illustrated by Mrs. Hugh Blackburn (Glasgow: Maclehose, 1872).

DR. G. E. DAY

IN a former number, under the date of February 8, we had the painful duty of announcing the death, at the age of fifty-six, of Dr. George Edward Day, F.R.S., Emeritus Chandos Professor of Medicine in the University of St. Andrews, which took place at Torquay on January 31, 1872. Most of his earlier friends had probably heard of the sad accident which reduced him to a state of bodily helplessness, and which darkened his latter years; but few of those who remembered him only as the genial witty Cantab, overflowing with life and spirits, and as the brilliant medical student at Edinburgh, carrying everything before him in class-room and debating hall, or later, as the active untiring President of the Medical Examinations at St. Andrews, would have supposed him capable of the cheerful resignation with which he submitted to his enforced exclusion from all participation in active, professional, and social life.

The story of Dr. Day's life is a sad record of brilliant expectations suddenly wrecked, and long continued struggles against irreparable calamities.

As the eldest son of a wealthy country gentleman of good position, his fortune seemed assured from his birth; but the failure of the Swansea Bank in 1825, when he was scarcely ten years old, ruined his father, and led to his removal to the house of a widowed grandmother.

In 1834, after some preparation under a private tutor, he went up to Cambridge with the reputation of an able mathematician, and a good classical scholar. At the University he worked splendidly by fits and starts, but the period between 1834 and 1837 does not belong to the working era of Cambridge, and George Day's natural love of fun and the fascination of his manner combined to render his society especially attractive to his comrades, and the result was, that he came out as low as twelfth among the wranglers of his year.

On leaving Cambridge he resolved to adopt medicine as his future profession, and went to Edinburgh, where he at once took his place among that brilliant band of young men who reckoned John Goodsir, Edward Forbes, and many others of similar promise amongst their ranks. On leaving Edinburgh he at once came to London, and taking a house at the West End, attempted to establish himself as a pure physician. During these eight or nine years of his London life, Dr. Day laboured on with unwearying industry and patience, lecturing at the Middlesex and other metropolitan medical schools, writing for reviews, translating from German, and turning his versatile talents and his special knowledge of physiological chemistry to account in every way. The result of this heavy strain was a threatening of brain disease, which, according to the verdict of his medical advisers, could only be warded off by complete rest and cessation from the cares in which he was immersed.

At that moment the death of an old friend, Dr. John Reid, opened the prospect to him of obtaining the Chair of Medicine at St. Andrews. His success in this probably saved his life, for the removal from the turmoil of a struggling London career to the comparative ease of the Scottish University arrested the threatenings of disease, and enabled him to recover some of his old vigorous tone. During the 13 years that Dr. Day held the Chair of Medicine at St. Andrews, from 1850 to 1863, he made it his special duty to promote the honour and further the interests of the University by raising the character of medical degrees; and so successfully did he accomplish this task, that the discredit which had belonged in former days to the M.D. degree of St. Andrews was completely effaced under his presidency of the Examining Board. A new system of stringent *vivâ voce* and written examinations was then inaugurated, which justified those who graduated in his time in regarding their attainment of the M.D. degree of St. Andrews as a professional honour of which any man might be proud.

In 1857 Dr. Day's prospects of a more prosperous future than he had as yet been able to look forward to were completely destroyed by the accident to which we have already referred, and which befell him in the course of a vacation tour in the English Lake District. On a bright morning at the end of the August of that year, he had set forth from his hotel at Patterdale in full vigour and strength, bent on "learning a new wrinkle about Helvellyn," as he himself expressed it, by making his way to the summit along a recently opened path. He made the ascent as he had designed, but instead of returning by the same track, he struck off in the direction of the white lead mines; and while walking along what he mistook for a miner's path, the ground gave way under him, and he fell into what proved to be a horizontal chimney or culvert, constructed to carry off the sulphurous, arsenical, and other gases, whose deposits had proved injurious to the sheep grazing on the hill side. He was rescued after three hours of anxious suspense, but the proximate results of that accident were dislocation of the right elbow and two fractures of the same arm, the upper one in the surgical neck of the bone of the humerus, which never united. The subsequent effects were the complete destruction of his general health, which obliged him in 1863 to give up the Chair of Medicine at St. Andrews and retire from active life. A removal to the milder climate of Torquay had little effect in arresting the train of symptoms which year by year marked the progress of disease, and were, it is conjectured, the result of a jar to the spine sustained by his accident on Helvellyn, which had, in truth, proved to him the beginning of the end.

And such was the checkered career of this man of brilliant promise, unflinching bravery of spirit, clear judgment, and tender heart. Disappointed again and again, he always met his troubles manfully, and turned them to good account for himself or others. We have given no list of the various honours which he attained in his profession, or of his literary works, for the detailed reports of these particulars are contained in the various obituary notices which have appeared of Dr. Day in the medical and other journals, to whose pages, as well as to our own, he was a frequent contributor.

OCEAN CURRENTS

A NEW interest seems now to be taken in Ocean Currents, and much is being said and written upon the subject. In the investigation of this subject it is very important that we should understand well all the forces and agencies concerned in the production and maintenance of the currents, and that we should consider well all the principles, and theories based upon hypothetical forces, which have come down to us from preceding generations, however plausible and however much sanctioned by high authority they appear to be. As in the case of the winds, so also in ocean currents, the modifying force arising from the earth's rotation has a very important bearing, and should be well understood. There are certain erroneous views in connection with this force, which have come down to us from preceding generations, and which are contained in text-books, and are being taught in colleges and schools, which are liable to have, and do have, a mischievous bearing upon this subject. These are the more dangerous because they appear to have received at least the tacit sanction of past ages, so that almost any one is liable to adopt them without much consideration. Prof. Colding has in this way been unsuspectingly let into error in his recent paper on ocean currents. We are all familiar with the usual explanation of the trade-winds contained in text-books, which assuming that a particle of air at the equator, at rest relatively to the earth, and consequently having a lineal velocity in space of about 1,000 miles per hour, is forced to move toward the pole, it will, on arriving at the parallel of latitude where the earth's surface has a velocity of only 900 miles, still have its velocity of 1,000 miles per hour in the case of no friction, and consequently have a relative velocity of 100 miles per hour, and on arriving at the parallel of 60°, will still have its initial velocity of 1,000 miles, and consequently have a relative velocity of 500 miles per hour. But this is at variance with a fundamental and well-established principle in mechanics. The force in this case is a central force, or at least the compound perpendicular to the earth's axis can be neglected, since it can have nothing to do with any east or west motion. This being the case, the principle of the preservation of areas must be satisfied, and consequently the particle of air, when it arrives at the parallel where the earth's surface has a velocity of 900 miles, must have a velocity of more than 1,000 miles, and a relative velocity of more than 200 miles per hour, and on arriving at the parallel of 60°, where the earth's surface has a velocity of 500 miles, it must have a velocity of 2,000 miles, and consequently a relative velocity of 1,500 miles, instead of 500 miles per hour. Adopting thoughtlessly, and very naturally, the erroneous principle which is usually taught, that a particle of air or of water in moving toward or from the pole, tends to keep its initial lineal velocity relative to space, Prof. Colding estimates the amount of deflecting force due to the earth's rotation, eastward when the particle is moving towards the pole, and westward when moving from the pole, and the result is, that his force is just one half of what it really is. Consequently, all the results based upon his estimated amount of this force should be doubled. Prof. Colding has also entirely neglected one component of the force due to the earth's rotation. It has been shown by Prof. Everett, and also by the writer, that when a body moves east or west, there is also a similar deflecting force due to the earth's rotation, exactly equal to the former. Prof. Colding has, therefore, taken into account only the one-fourth part of the whole force. If he had taken in this latter component of the force also, and resolved it in the direction of the line of motion and perpendicular to it, as he did the former, he would have found that the parts in the direction of motion, arising from both components, exactly cancel one another in all cases, and that the resultant of both components is a force perpendicular to the direction of motion. This force then tends only to change the direction of the motion, and never to accelerate or retard it, in whatever direction it may be. Prof. Colding's result, therefore, that the velocity of the current is accelerated by the earth's rotation, when moving in certain directions, and retarded in others, is erroneous.

It is known that there are two theories with regard to the cause of Ocean Currents: the one, that they are caused by the winds acting upon the ocean, the other, advocated by Dr. Carpenter, that they are caused by a difference of density of the ocean between the equator and the poles, due to a difference of temperature. The tendency of both theories is in the same direction, and the currents, no doubt, are in some measure due to the forces belonging to each theory. The history of the former theory, and the high authority which can be appealed to in its support, are well known, but we have reason to think that the forces, and the effects of them, in the former theory, are quite subordinate to those of the latter. The well-known explanation of the Gulf Stream by the former theory assumes that there is a heaping up of the water of the ocean in the Gulf of Mexico by the action of the trade winds, sufficient to change the sea-level enough to cause the observed current passing through the Strait of Florida. But the trade winds cannot have much effect in causing a heaping up of the water on the coast of Mexico, since the force is applied to the surface merely, and tends to produce only a surface current, while all the great body of the water, except a little of the surface, is free to flow back. It is true there must be a slight change of sea-level to give rise to a force sufficient to overcome the resistances to this under tow, but these are extremely small since the velocity of this under tow, including all the great depth of the ocean, except the superficial westward current, is very small. That the merely superficial part of the equatorial current is mostly caused by the trade-winds may be true, but the Gulf Stream, which is not directly acted upon, except by the very gentle south-west winds, and which is not merely a surface-current, must be mostly accounted for by the other theory. Let us now see what can be learned upon this subject from observation. Instances of a great change of water-level in shallow canals have been cited to show the influence of the wind in causing a heaping up of the water at the one end; but the water in these cases being very shallow, the force may be regarded as applied somewhat to the whole body of the water, and the under counter-current is thus prevented, but the case is very different in a deep ocean. It is well known from the discussion of tidal observations that the influence of the wind in changing the sea-level is very small. If the force of the trade winds causes a higher sea-level in the Gulf of Mexico, we know that the west winds in higher latitudes must cause a similar rise of sea-level on the west coast of Europe, for the sum of the moments with reference to the earth's axis, of the forces, west between the tropics and east in higher latitudes, must exactly balance each other. If the explanation of the Gulf Stream requires that the level of the Gulf of Mexico should be raised about twelve feet, as shown by Prof. Colding, then there must be about an equal change of level on the west coast of Europe, if these changes are caused by the winds; for although the extent of coast receiving the west winds may be greater than that receiving the east winds, yet this is counterbalanced by the circumstance that the force of the west winds acts at a less distance from the earth's axis, which requires that they should be stronger. If, then, the west winds cause a change of sea-level on the coast of Europe, say of ten feet, then any change in the force of these winds at different seasons must cause a very perceptible change of sea-level. Now, we know that the force of the west wind on the Atlantic Ocean is considerably greater in the spring than the autumn. There should therefore be a corresponding difference in the mean level of the sea, and this mean level on the coast of Europe should be greatest in the spring. But the discussion of the tidal observations made at Brest, shows that the mean level of the sea, after being corrected for the barometer and a very small astronomical term affecting the mean level, is about four inches lower in the spring when the winds are strongest than in the autumn when they are weakest. (Proceedings of the American Academy of Sciences and Arts, vol. vii. p. 32.) The discussion, likewise, of the tides of Boston Harbour gives a similar result, except that the range of the monthly means is still less, being less than three inches. (U.S. Coast Survey Report for 1868.) These results should receive the attention of those who maintain that great changes of sea-level are caused by the winds.

In a paper by the writer, published in *Silliman's Journal* (second series, vol. xxxi. p. 45) there are several pages given to the subject of ocean currents, in which it is maintained that the principal agency in their production is difference of temperature of the sea-water between the equator and the poles. The principal effects of the earth's rotation are there given, which are too numerous to be recited here. In addition to the results there given, the following additional thought may be given here as being perhaps new. As the surface-water flows toward the poles the deflecting force of the earth's rotation presses it toward the east. In like manner as the water below flows toward the equator, there is a similar force pressing it toward the west. These forces are small, but they must nevertheless cause a gradual rising of the cold water at the bottom on the American coast, and this, perhaps more than the Greenland current, causes cold water there. The Gulf Stream of warmer water cuts its way through this cold water gradually rising from the bottom, and hence the cold walls observed by the U.S. Coast Survey.

Mr. Croll seems committed to the wind theory, and is unwilling to admit that the theory advocated by Dr. Carpenter can have even a subordinate effect. His principal argument is based upon an experiment of M. Dubuat. I know not under what circumstances this experiment was made, but of course it was with a comparatively shallow canal or stratum of water, and the result is no doubt correct for the depth of water with which the experiment was made. A much less force on each particle of a large body of water is sufficient to overcome the cohesion of the particles, and produce motion than upon a small one, just as a small drop of water remains suspended to a twig, while the same force of gravity causes a large one to drop off. The case therefore of the ocean is very different from that of a shallow canal. As Mr. Croll insists that Dr. Carpenter's experiment, to be applicable to the case, should have been made with a canal 120 feet long, and only one inch deep; so it might be insisted that M. Dubuat's experiment, to be applicable to Mr. Croll's case, should be made with a canal or body of water three or four miles deep. But there is no necessity for us to make any such experiments, for nature is performing the experiment regularly every six hours, and all that we have to do is to observe. The attraction of the moon changes the level due to the attraction of the earth alone, and puts the ocean, as it were, upon an inclined plane with a gradient of about two feet in the distance of a quadrant, and the water slides down, causing a rising of the tide at one place and a falling at another; and in six hours this gradient is reversed, and the reverse motion of the water follows, thus causing the regular ebbing and flowing of the tides. If M. Dubuat's experiment were applicable to the ocean, the moon could not cause a tide at all unless its mass were about fifteen times greater.

Cambridge, Mass., U.S.A. W. FERREL

*FERGUSSON'S RUDE STONE MONUMENTS**

IN Mr. Fergusson's "Handbook of Architecture," published in 1854, one chapter of about fifty pages is devoted to Megalithic, or, as he prefers to call them, Rude Stone, Monuments. Ever since that period he has been collecting materials on this interesting subject, and the result is now before us, in the work which forms the subject

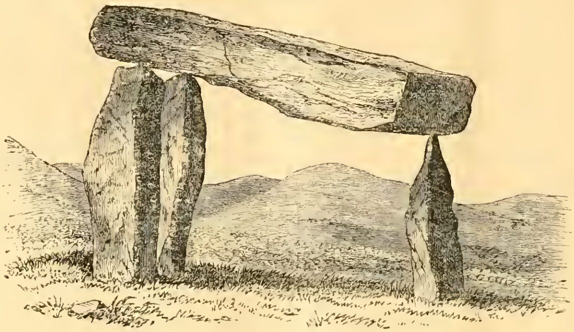

FIG. 1.—Dolmen at Castle Wellan, Ireland. From a drawing by Sir Henry James.

of this notice. In it he confines himself to the classes of monuments indicated in the title, omitting all reference to hut circles, Pict's houses, brochs, and other buildings composed of smaller stones; not because he doubts that they belong to the same period, "but because their age being doubtful also" it would only complicate the

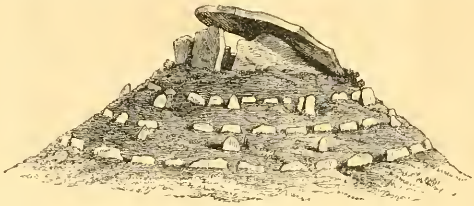

FIG. 2.—Dolmen de Bousquet. From a drawing by E. Cartailhac.

argument to introduce them. He limits himself therefore to tumuli, menhirs or stone pillars, stone circles, avenues, and dolmens. All these we find sometimes singly, sometimes in combination, the tumulus containing a dolmen, being surrounded by one or more stone circles, and surmounted by a menhir. Fig. liii., representing the celebrated

FIG. 3.—Nine Ladies, Stanton Moor. From a drawing by L. Jewitt.

tumulus of New Grange, near Drogheda, gives a good idea of the large barrows; it was originally surrounded by a circle of stones, most of which, however, have disappeared. Fig. 3 represents the stone circle, known as the Nine Ladies on Stanton Moor.

The typical "Dolmen" may be described as a massive

FIG. 4.—Long Barrow, Kennet, restored by Dr. Thurnam. *Archæologia*, xii.

stone resting on three supports; the celebrated Kits Coty House, near Maidstone, may be regarded as a typical example. Fig. cvii. represents one at Halskov, in Denmark, raised on a small mound, and surrounded by a circle of stone. Fig. 1, representing a Dolmen at Castle Wellan, Ireland, and Fig. 6, one at Grandmont, in Bas Languedoc, are more ex-

* "Rude Stone Monuments." By James Fergusson, D.C.L., F.R.S. (London: John Murray, 1872.)

ceptional types. Dolmens are sometimes covered by a mound of earth (like the Gib Hill example, excavated by Mr. Bateman), sometimes free, as in the preceding figures. That all the earlier ones were covered, says Mr. Fergusson, "is more than probable, and it may since have been originally intended to cover up many of those which now stand free ; but it seems impossible to believe that the bulk of those we now see were ever hidden by any earthen covering."

The tumuli which contain megalithic chambers closely resemble the dwellings even now used by many northern nations, the Siberian Yurt, for instance, consists of a central chamber, generally sunk a little below the surface, built of stones or timber, and heaped over with earth, so as to form a mound. The Tchutski huts are very similar. "They are," says Captain Cook, "sunk a little below the surface of the earth. One of them which I examined was of an oval form, about twenty feet long and twelve or more high. The framing was composed of wood and the ribs of whales, disposed in a judicious manner, and bound together with smaller materials of the same sort. Over this framing is laid a covering of strong coarse grass, and that, again, is covered with earth, so that, on the outside, the house looks like a hillock supported by a wall of stone three or four feet high, which is built round the two sides and one end."

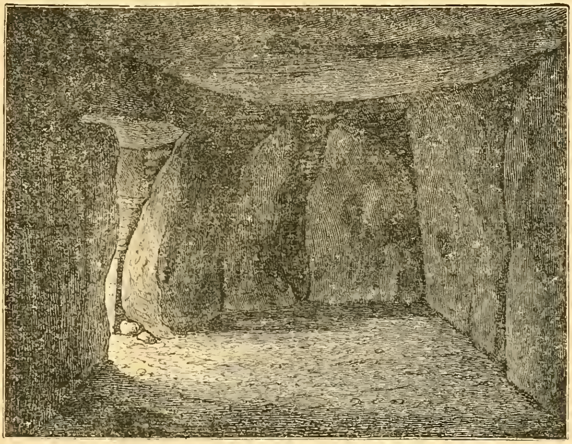

FIG. 5.—View of Interior of Chamber at Uby. From Madsen.

FIG. 6.—Dolmen of Grandmont.

The huts of the Esquimaux and Lapps are built on the same model, and have generally a longer or shorter covered passage leading to the door, the object of which is to keep the cold out of the central chamber. Round the walls of the latter are ranged seats for the inmates, and part of the space is often separated off by partitions. So closely do many of our Northern tumuli correspond to these descriptions, that Nilsson long ago suggested many of them having been originally used as dwelling places, and converted subsequently into tombs. Fig xi., for instance, represents the chamber of a tumulus near St. Helier, in Jersey. Here we have the central room, with partitions, and the passage leading to the door. In some few cases the dead have been found sitting round the sepulchral chamber, with their arms and implements by their side, just as they may be supposed to have sat during life. Fig. 5 represents the chamber of a tumulus at Uby in Denmark. Stonehenge itself (Fig. 8) seems to be constructed on the same model: the mound, however, being absent, or only represented by the encircling ring of earth.

In determining the date of particular tumuli, Mr. Fergusson seems to me to attach too much importance to objects found on, or near the surface, and which often have no doubt been accidentally dropped, or belong to secondary interments. Thus he refers to the two objects of iron found at Gib Hill, as if they justified us in ascribing that interesting tumulus to the iron age. But Mr. Bateman, by whom that mound was opened, expressly states that the objects of iron were not found in the central cist, but they belonged to a secondary interment. They throw, therefore, no more light on the date of Gib Hill itself than the fragments of ginger-beer bottles which abound in the area of Stonehenge do on the period to which it belongs. This is a consideration which is of great importance; because the history of these megalithic monuments, the race by whom, and the date at which they were constructed, are most interesting questions of archæology. Although few now regard Stonehenge as a Druidical temple, still archæologists are almost unanimous in regarding it as very ancient; while the class of megalithic monuments they consider to have begun in pre-historic times, and to have continued in out-of-the-way parts down to a comparatively recent period. Mr. Fergusson, on the contrary, is of a different opinion. He endeavours to show that these monuments belong to one period, and to comparatively recent times:—

"However this may be," he says, "I trust that this work may lay claim to being, in one respect at least, a contribution to the cause of truth regarding the much-disputed age and use of these rude stone monuments. It states distinctly, and without reserve, one view of the mooted question, and so openly, that any one who knows better can at once pull away the prop from my house of cards and level it with the ground. If one thing comes out more clearly than another in the course of this investigation, it is that the style of architecture to which these monuments belong is a style, like Gothic, Grecian, Egyptian, Budhist, or any other. It has a beginning, and middle, and an end; and though we cannot yet make out the sequence in all its details, this at least seems clear—that there is no great hiatus; nor is it that one part is pre-historic, while the other belongs to historic times. All belong to the one epoch or the other. Either it is that Stonehenge and Avebury, and all such, are the temples of a race so ancient as to be beyond the ken of mortal men, or they are the sepulchral monument of a people who lived so nearly within the limits of the true historic times that their story can easily be recovered."

FIG. 7.—Dolmen at Pullicondah.

As already mentioned, the latter is Mr. Fergusson's view. Almost alone among English archæologists, he considers that Stonehenge is part Roman, and believes it to have been erected by Ambrosius, between the years 466 and 470 A.D., in memory of the British chiefs treacherously slain a few years previously. This theory I have discussed in "Pre-historic Times," and, as I have little to alter in, or add to, what is there said, I will not here repeat my arguments.

As regards Abury, the second in importance—if, indeed, it be the second and not the first of these monuments Mr. Fergusson says:—"I feel no doubt that it will come eventually to be acknowledged that those who fell in Arthur's twelfth and greatest battle were buried in the ring at Avebury, and that those who survived raised these stones and the mound at Silbury, in the vain hope that they would convey to their latest posterity the memory of their prowess" (p. 89). In fact, Mr. Fergusson refers to this period all the similar monuments in England, a conclusion which seems to me in itself most improbable, and which becomes still more so if we consider the similar remains of other countries. The Irish examples he considers to be somewhat earlier; the Moytura remains, for instance, being perhaps as early as the first century B.C. As regards the North, he regards the celebrated tumulus of Maes Howe as probably the "tomb of Havard, or of some other of the Pagan Norwegian Jarls of Orkney;" while the Stones of Stennis can hardly, he thinks, "be carried back beyond the year 800," to which period he refers all the megalithic remains in those islands. In short he regards these monuments, whether in Britain, Scandinavia, Germany, France, Spain, Algeria, or India, as post-Christian in date, and in many cases not more than a few hundred years old. Such a conclusion seems to me entirely inconsistent with architectural history. Thus in more than one case we know of early churches, probably belonging to the 10th or 11th centuries, which are constructed over dolmens.

Mr. Fergusson admits that the great tumulus near Sardis (Fig. 1, p. 31) is rightly identified as the tomb of Alyattes, was erected in the sixth century, B.C., and was described by Herodotus; that some of the tumuli on the eastern shores of the Mediterranean are certainly "as old as the thirteenth century, B.C.: that the practice of burying in tumuli must have existed for many centuries before such tombs could have been constructed; and that the age in which they "were erected was essentially the age of bronze: not only are the ornaments and furniture found in the Etruscan tombs generally of that metal, but the tombs at Mycenæ and Orchomenos were wholly lined with it;" a fact which is the more interesting when we remember that all the metallic objects found in the tumuli round Stonehenge were of bronze.

Again, let us consider the class of monuments which consist of a free dolmen standing on a mound, and surrounded by one or more stone circles. This type is very widely distributed. A Danish example has already been given, Fig. 5. Fig. 4 represents the long barrow at Kennet, near Marlborough, after Dr. Thurman; Fig. 2 is the Dolmen de Bousquet in the Aveyron; lastly, Fig. 7 is a similar monument at Pullicondah, near Madras. These tumuli, though differing in detail, are identical in all essential

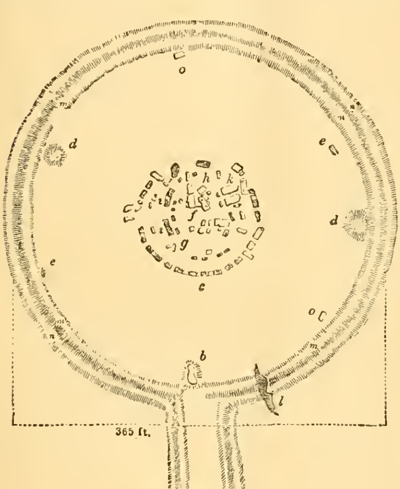

FIG. 8.—General Plan of Stonehenge, from Knight's "Old England."

points. If these monuments all belong to post-Christian times, they must have been erected by very different races of men. Mr. Fergusson, indeed, admits that they are the work of very different races; how then does he account for the remarkable similarity existing between them? He denies that the Celts, Scandinavians, or Iberians were themselves naturally "rude stone builders," and endeavours to remove the difficulty by an explanation which is most important, because it seems to me to involve the practical abandonment of the conclusion, which, as he told us in the preface, is the central feature of his work. This style of art, he says, "seems to have been invented by some pre-Celtic people, but to have been adopted by Celts, by Scandinavians, by British, and Iberian races."

But if Europe was once occupied by a pre-Celtic, megalithic-monument-building race, surely some of our megalithic monuments must be ascribable to that time and race, and we come back therefore to the general opinion of archæologists, that our megalithic monuments belong to very different periods and people, and *not* all to one race or one epoch.

I cannot now enter into the consideration of the dates to which Mr. Fergusson ascribes individual monuments; I doubt whether any belong to so recent a period as he supposes: and can only express my surprise at the certainty and confidence which he feels in his own opinions—a certainty sometimes, however, oddly expressed, as, for instance, when he tells us, speaking of the crosses at Katapur, which he considers to be Christian and contemporaneous with a group of neighbouring dolmens, that "their juxtaposition and whole appearance render escape from this conclusion apparently inevitable."

But while I cannot accept Mr. Fergusson's peculiar theories, I cannot conclude without thanking him for the labour and care with which he has brought together a great number of illustrations, and a vast mass of facts, on this most interesting subject. In a review, one naturally dwells on points of difference, but every one must accord to Mr. Fergusson the credit which, in the following passage from his preface, he claims for himself; though I would venture to add that the unintentional self-criticism in the latter sentence seems to me not inapplicable. "I have," he says, "spared no pains in investigating the materials placed at my disposal, and no haste in forming my conclusions." His conclusions are, I think, in some cases, hasty and untenable; some seem inconsistent with one another; but no one can deny to his work the merit of being a rich and trustworthy storehouse of facts.

JOHN LUBBOCK

THE STUDY AND TEACHING OF MECHANICS

A LECTURE on this subject, being one of the series of lectures at the College of Preceptors on the Teaching of Physical Science, was given by Prof. W. G. Adams, of which the following is the substance:—

Mechanics treats of the laws of equilibrium and of motion of bodies, and in its widest sense, as the science of energy, must include all branches of Physics, for the solid, liquid, and gaseous states of bodies are determined by the more or less free motion of their molecules, and heat, light, electricity and magnetism are all different forms of motion. The study of the laws of equilibrium and of visible motion is important, both for their practical applications and because on them are founded the principles of thermo- and electro-dynamics. Before entering on a study of mechanics, students should have a knowledge of algebra and geometry, and on account of the importance of accurate measurement, the elements of trigonometry should also be studied. By a proper method of teaching geometry, boys can be taught to think, and the exact definitions and proofs of Euclid's Elements are better fitted to train the judgment and the reasoning powers than any less exact system of geometry. The way to teach geometry (and the same remark applies to mechanics) is not to expect boys to get up their Euclid from a book, and to say it off by the aid of a book of figures (a system which has been practised in many schools), but to explain the meaning of and illustrate every proposition, so that boys may understand it. The true method of teaching mechanics is illustrated by the way in which Galileo established the first principles of dynamics, and placed them before his pupils. Due weight should be given both to experimental and to rational mechanics, and the best way of bringing the subject before students is to have parallel but distinct courses of experimental and theoretical lectures attended by students at the same time. The practical applications of the subject are important, and some of them of great simplicity. The "Triangle of Forces" may be employed to build up diagrams to represent the thrusts on a jointed framework; so that by "Diagrams of Forces" the conditions of stability of loaded structures, and the form and tensions of suspension bridges, could at once be determined, by measurement of these diagrams or by calculation from them. Of the variety of text-books on the subject of mechanics, the teacher should reject books that profess to be adapted for examinations, as well as those which contain gross errors on the laws of friction, or on the inertia of matter and the laws of motion, or on the subject of dynamical units and should select from those which are not liable to such objections.

Competitive examinations may be useful if they are made tests of thorough knowledge; but too often they injure the student who is preparing for them by narrowing his mind, and create a class of dabblers in science, and are worthless for the purpose for which they are intended. Test examinations given to a class on the subject of their lectures are the best tests of the knowledge and progress of the student.

In teaching the laws of equilibrium of liquids and of gases, the same method must be followed as in teaching the laws of equilibrium and of motion of solids; and in addition to lectures and ordinary teaching students should have the opportunity of making experiments and measurements in these subjects in a physical laboratory. Some knowledge of other kindred sciences is necessary before a student can be said to have an intelligent knowledge of the principles of mechanical science. Accurate investigation and experiment show that near the melting and the boiling points the special properties and laws of solids or of liquids are no longer true, and Dr. Andrews has pointed out the existence of a border-land between the liquid and the gaseous states, and has shown that there is no breach of continuity between them. Taking a model, of which three rectangular edges shall represent the pressure, volume, and temperature, the upper surface will represent the state of the substance, and will explain in what way it is possible to pass from the liquid to the gas without change of state or any sudden change of volume. The ease with which we can conceive of the state of a gas under different circumstances, when we have such a model before us, shows the importance of employing figures and models to give a boy clear ideas of the propositions of mechanics.

Regarding Mechanics in its wider sense as the Science of Energy, there are three great principles—the Conservation, the Transformation, and the Dissipation of Energy, which have been established, and these principles are illustrated in the conversion of water into steam, in winding up a watch, in the diffusion of gases, in the conduction of heat, in the friction of the tides on the earth, and in the rushing of water down a mountain side. This latter source of energy has been employed in piercing the Mont Cenis tunnel.

The accuracy of the calculations by which the axes of the two tunnels on opposite sides agreed so completely with one another shows the importance of accurate measurement, and of the correct application of theoretical principles to practice.

These principles of energy tell us that in raising the waters of the ocean to the mountain tops, as much energy must be expended as can be expended by those waters in their return to the ocean, and the atmosphere, acted upon by the solar heat, is the vast air-engine by which these changes are accomplished.

NOTES

At the last meeting of the Royal Society the names of the candidates for election into the Society were read, in accordance with the statutes, as follows:—Andrew Leith Adams, Surgeon-Major; William Grylls Adams, M.A.; William Aitken, M.D.; ? Alexander Armstrong, K.C.B., M.D.; Edward Middleton Barry, R.A.; John Beddoe, B.A., M.D.; Henry Bowman Brady, F.L.S.; Frederick Joseph Bramwell, C.E.; James Brunlees, C.E.; Edwin Kilwick Calver, Capt. R.N.; Alexander Carte, M.A., M.D.; William Chimmo, Commander R.N.; Prof. Arthur Herbert Church, M.A.; Fredk. Le Gros Clark, M.R.C.S.; Prof. John Cleland, M.D.; Herbert Davies, M.D.; Henry Dircks, F.C.S.; August Dupré, Ph.D.; Michael Foster, jun., M.A., M.D.; Peter Le Neve Foster, M.A.; Wilson Fox, M.D.; Arthur Gamgee, M.D.; Prof. Thomas Minchin Goodeve, M.A.; Townshend M. Hall, F.G.S.; Edmund Thomas Higgins, M.R.C.S.; Rev. Thomas Hincks, B.A.; Rev. A. Hume, LL.D.; Henry Hyde, Lieut.-Col. R.E.; Prof. William Stanley Jevons, M.A.; Edmund Charles Johnson, F.R.G.S.; George Johnson, M.D.; Prof. Thomas Rupert Jones; John Leckenby, F.G.S.; Clements R. Markham, Sec. Geog. Soc.; William Mayes, Staff-Comm. R.N.; Edmund James Mills, D.Sc.; Thomas George Montgomerie, Major R.E.; Robert Stirling Newall, F.R.A.S.; Edward Latham Ormerod, M.D.; Francis Polkinghorne Pascoe, F.L.S.; Prof. Oliver Pemberton; Rev. Stephen Joseph Perry; John Arthur Phillips, F.C.S.; Bedford Clapperton T. Pim, Captain R.N.; William Overend Priestley, M.D.; Charles Bland Radcliffe, M.D.; Edward John Routh, M.A.; George West Royston-Pigott, M.D.; William Westcott Rundell; William James Russell, Ph.D.; Osbert Salvin, M.A.; Harry Govier Seeley, F.L.S.; Alfred R. C. Selwyn (Geol. Survey, Canada); Peter Squire, F.L.S.; George James Symons, V.P. Met. Soc.; Edwin T. Truman, M.R.C.S.; Wildman Whitehouse, C.E.; Henry Woodward, F.G.S.; Archibald Henry Plantagenet Stuart Wortley, Lieut-Col.

The Earl of Lonsdale, whose death is recorded this week, was the father of the Royal Society, his election having taken place sixty-two years ago, in 1810. This honour now devolves on Sir Henry Holland, elected in 1815.

The death is announced, on the 3rd inst., of Dr. A. B. Granville, F.R.S., at the age of 88. He was one of the oldest Fellows of the Royal Society, having been elected in 1817, and was member of a large number of foreign learned societies.

We are very glad to be able to state that intelligence has just been received from Prof. Huxley that his health has already been greatly renovated by the pure air of Upper Egypt. He wrote from Thebes, and was then contemplating a visit to Assouan, from which he would probably have returned to Thebes before this.

Sir William Thomson has accepted the office of President of the Geological Society of Glasgow.

The Radcliffe Travelling Fellowship at Oxford has been awarded to Mr. F. H. Champneys, B.A. of Brasenose College. This Fellowship is of the annual value of 200*l*., and tenable for three years, provided the Fellow does not spend more than eighteen months within the United Kingdom.

The President of the Quekett Microscopical Club will hold a *soirée*, on Friday evening, March 15, at University College.

Dr. Liebreich, the eminent ophthalmist, of St. Thomas's Hospital, delivered a lecture at the Royal Institution on Friday evening last, on certain faults of vision, with special reference to Turner and Mulready. The later "aberrations" of Turner's style he attributed to a physical change in the refractive power of the eye, by which illuminated points were converted into illuminated lines. The change of manner in Mulready's later pictures he accounted for, in like manner, by increasing yellow degeneration of the crystalline lens. We hope in a future number to give a report of the lecture.

THE Royal Academy of Sciences of Belgium offers prizes on the following subjects for Essays to be sent in during the year 1873 :—(1) A simplification of the theory of the integration of equations of the two first orders ; (2) On the disturbing causes which influence the determination of the electro-motor force and of the interior resistance of an element of the electric pile ; (3) On the relations of heat to the development of flowering plants, especially with regard to the periodic phenomena of vegetation, and on the dynamical influence of solar heat on the evolution of plants ; (4) On the mode of reproduction of serpents ; (5) On the composition and mutual relations of albuminoid substances ; (6) On the coal fields of the basin of Liége. A gold medal of the value of 1000 fr. will be given for the first, fifth, and sixth questions, and of 600 fr. for the second, third, and fourth. The essays must be written in Latin, French, or Flemish. For 1874 the subjects are :—(1) On uric acid and its derivatives, especially in relation to their chemical structure and synthesis ; (2) On the polymorphism of the Mucedineæ, their real nature, and the conditions of their development ; (3) On the question whether the fungi of fermentation can, under certain circumstances, become changed into the higher fungi, with positive proof of the fact or the contrary.

Harper's Weekly states that Uriah F. Boyden, of Boston, U.S.A., has deposited with the Franklin Institute, of Philadelphia, the sum of one thousand dollars, to be awarded as a premium to any resident of North America who shall determine by experiment whether all rays of light and other physical rays are or are not transmitted with the same velocity. The conditions of the premium limit the applicants to those living north of the southern boundary of Mexico, and including the West India Islands. Applications must be made before the 1st of January, 1873, at which time the judges, appointed by the Franklin Institute, shall examine the memoirs and decide whether any one is entitled to the premium.

WE are desired by Colonel Grant to say that the botanica collection from Tropical Africa, referred to at p. 339, was not made in conjunction with Captain Burton, but during the journey of Captain Speke and himself in 1860-3, from Zanzibar to the great central Lake Region. The results will shortly be published in the Transactions of the Linnean Society; it will be illustrated by 100 (not 600) 4to plates, and the descriptions will be in great part drawn up by Prof. Oliver. We are glad to hear that Mr. W. O. Livingstone, who is accompanying the Livingstone Search Expedition, has considerable botanical knowledge, and is intending to bring home a collection.

IN reference to the hairy tapir of the South American Andes (*Tapirus Roulini*), the acquisition of skeletons of which by the Smithsonian Institution was spoken of in our last number (p. 370), we are informed that a fine series of skins and skeletons of the animal has recently been obtained by Mr. Buckley in Ecuador. Some of these are now in the British Museum ; the others have been purchased by Mr. Edwd. Gerrard, jun., of Camden Town. At the last meeting of the Zoological Society a paper was read by Dr. Gray, describing the specimens acquired by the British Museum, and referring them to a new species, *Tapirus leucogenys*. But we are informed that there are no valid grounds for separating them from Roulin's Tapir of the U.S. of Colombia.

WE desire to call attention to the Annual General Meeting of the Iron and Steel Institute, which will be held in Willis's Rooms, King Street, St. James's, London, commencing on Tuesday, March 19, under the presidency of Mr. Henry Bessemer. The programme of proceedings will be found in our advertising columns. It is expected that on Tuesday evening, March 19, a paper, by Mr. I. Lowthian Bell, "On the Conditions which Favour and those which Limit the Economy of Fuel in the Blast Furnace for Smelting Iron," will be read and discussed at the meeting of the Institute of Civil Engineers, Great George Street, Westminster. The Council have kindly promised to issue invitations to members of the Iron and Steel Institute, to attend on this occasion.

OUR readers will have noticed in our advertising columns the list of subscriptions at present received to the "Priestley Memorial Fund." The object is worthy of the attention of all who are able and disposed to assist in so meritorious an object.

AN important letter, by M. Berthelot, appears in the *Moniteur Scientifique* for February, in which this eminent *savant* insists on the reconciliation of the scientific worlds of France and Germany, pointing out that the united action of France, Germany, and England, in the advancement of civilisation and science, is necessary for the progress of the world.

IT is stated that shocks of earthquake were felt at Dresden, Pirna, Schandau, Chemnitz, Rodenbach, Weimar, and Rudolstadt, between three and four o'clock on the afternoon of the 6th inst. They continued to recur during an hour, and in some cases several hours.

THE return of Professor C. F. Hartt, of Ithaca, from his late expedition to Brazil, has been already announced in the papers ; and we are glad to learn, from *Harper's Weekly*, that he succeeded in making many important discoveries in natural history and the geography of the country, and especially the languages of the native tribes. By his researches in this latter direction he has already become quite an authority, and, we presume, will before long begin to publish his linguistic results. In the course of his expedition Professor Hartt took occasion to examine the great Kjoekkenmoedding, near Santarem, referred to by various travellers, which, however, yielded him only a few fragments of coarse pottery and a few bones. He was very fortunate in the opportunity of excavating the sites of a number of Indian villages on the edge of the bluffs bordering the Amazon and the Tapajos, in the angle made by the two rivers. Here he found an immense quantity of broken pottery, often highly ornamented, idols, stone implements, &c., probably derived from the Tapajos, now extinct as a tribe, or merged into the mixed Indian population of the Amazon. In an ancient burial-place on the Tapajos he dug up a number of burial-pots ; none, however, containing complete skeletons. An examination of the mounds of the island of Marajo was to be made by some of his associates who remained behind.

THE Royal Horticultural Society has just issued an exceedingly comprehensive and valuable series of meteorological observations made at their gardens at Chiswick from 1826 to 1864, and analysed by Mr. James Glaisher. The number of tables is nearly sixty, including the mean temperature of every day, and the extremes of mean temperature for every day in each month during the year specified, the excess or deficiency above or below the average of the mean temperature of every day, month, and year ; the daily ranges of temperature on every day of the year, and the daily falls of rain in each month. Comparisons are made with the series of observations taken at Greenwich ; general conclusions are deduced, and the introductory observations are of value and interest to all meteorologists.

WE understand that the Meteorological Committee have resolved to issue lithographed illustrative charts of the Daily Weather Report, which will be delivered in London, within a reasonable distance from the office of the printer in Lincoln's Inn Fields, between 1 and 2 P.M., or posted in time for the evening mails. Up to the 31st of March these charts will be supplied gratuitously.

AURORA AUSTRALIS

ON Sunday the 4th instant, at 9h. 28m. P.M., my attention was suddenly called to a "fire." Looking in the direction indicated, I saw at S.S E., about 15° above the horizon, a glare of reddish light. Curious to know whereabout the supposed fire was, I kept my eyes upon that part of the heavens. Presently, similar patches of light broke out on either side of the first, and in a few seconds I could see, on the assumption made, that there must be several fires blazing away over a wide range, for the sky was here and there lit up with a peculiar dark red light over an extent of at least 70° of the horizon. My attention being now aroused, I had recourse to various conjectures, which were speedily abandoned. The idea of an aurora had occurred almost at the outset; but as I had never, with certainty, seen one in Mauritius, and never heard or read of any having been observed there by others, I felt some reluctance to admit the fact that I was actually witnessing one My doubts, however, were soon dispelled. I noticed that patches of cloud floating across the illuminated portions of the sky reflected no light, and on one or two occasions, a faint flickering, like lightning, was seen among the upper cirrus clouds. These and other facts, coupled with the knowledge that the magnets had been occasionally disturbed to a considerable extent on Friday and Saturday, and on the morning of Sunday, left no doubt on my mind.

Hastening to the house, I immediately mounted a portable inclinometer and declinometer, and took all the measures I could to observe what might take place, dividing my time and attention between the instruments, which I put up in a verandah facing the south, and the aurora right in front of me.

The needle of the inclinometer did not give the slightest indication of a disturbance, but the declinometer magnet was affected to the extent at times of 9'.

It was 9h. 48m., or 20m. after I saw the luminosity supposed to have been caused by a fire, that I began to observe the aurora systematically, and I append a copy of the notes which I took from that time up to 1h. 20m. A.M.

What struck me particularly was the apparent quietness of the whole scene. Unlike the "merry dancers," which I have often seen and admired in Scotland, rapidly changing shape and colour, and rushing in variegated columns and bands in different directions with great velocity, thereby conveying an impression of energy and violence, the display of Sunday night was calm and serene, giving one an idea of peace and repose. Except shortly after I first saw the phenomenon, I could not make out any motion of the arches, segments, or luminous bands. They appeared and disappeared without change of locality, the intensity of the light increasing or decreasing without any flickering. I could see no shooting, darting, or rushing of the bands or beams. Each made its appearance and disappearance simultaneously along its whole length, as if the action was vertical.

The spectacle presented by the beams from 11 P.M to 11.20 P.M. was at once grand and lovely beyond description. Almost from the extreme left to the extreme right, and from as low down as I could see up to a meridional altitude of about 72°, the sky was furrowed with alternate white and dark bands, all of which, so far as I could judge, were parallel to each other and to the magnetic meridian. They were generally at unequal intervals, sometimes crowded together, and sometimes considerably apart; but in this respect I could only judge of those near the meridian. At times they presented the appearance of graceful folds and convolutions, but the action seems to have been performed so gently and imperceptibly as to convey no idea of motion. They presented the same colour during the whole time, namely, a steel grey to a silver white.

The arches and segments were of a blood, cherry, or Indian red, and every now and then, when the intensity of the light increased, the stars twinkled like gems seen through a delicate pink curtain or veil placed before them. Occasionally one could fancy that he was looking at the Southern Cross through very transparent glass or crystal of an exquisite ruby tint into an inner chamber lit up with light of a similar colour.

The light was never very strong. I saw no part of the landscape lighted up by reflection. It is to be borne in mind, however, that I was occupied with the instruments, and that much may have escaped my attention.

During some parts of the night black clouds passed over the field of view, and I believe, although I could not see them, except on one or two occasions, that they were light cirrus and cirro-stratus clouds in the upper regions, as had been the case throughout the day.

The wind was light from E. by S. throughout, and the barometer was ·100 inch below the mean for the season.

After 1 A.M. the aurora speedily died away. At 3 A.M. I could see nothing; but looking out at 4.30 A.M. I saw a red glow in the southward, which at first I took for aurora, but which turned out to be cirrus clouds lit up in the early dawn.

Throughout Monday the magnets were quiet. A great many cirri appeared, which, in the evening, assumed at eastward and westward a dark red colour, very much resembling that of the aurora.

The Magnetic Observatory, which had barely commenced operations, may be said to have been inaugurated on Sunday night, and it is possible that its future records will show, amongst other things, that aurora is not so unfrequent in Mauritius as is supposed, although such a display as that which has just occurred may not be seen for many years to come. In the end of August and beginning of September, 1859, aurora was observed over a considerable portion of both hemispheres, and on one night during that period I saw a reddish glow in our southern sky, which may have been the Aurora Australis. Probably the present display has been seen over a great part of the globe. Has any unusual solar activity been observed? On Friday a chain of spots stretched over nearly the whole of the sun's disc, and a large group occupied another part of it. On Monday the chain had disappeared. Any one who may have made observations in the colony or at sea on Sunday night would oblige me by sending them to the Observatory. It would be interesting to know the height of the aurora.

Aurora Australis seen at Mauritius on the 4th to 5th February, 1872.

9.48 P.M —An irregular convex arch of dark red light extending over about 60° of the horizon, and having its vertex in the line of the magnetic meridian. Brightest below the Southern Cross.

9.58 P.M.—An arch of a dark red colour having a cord of about 70°. Vertex in or near the magnetic meridian. Patches of black cloud passing over the coloured segment from E. by S., but they reflect no light.

10.1 P.M.—The segment is of a more intense dark red colour. Its eastern limit is about 3° east of the Cross, and its brightest portion from 1° to 2° above the Cross. It is broken off towards the west, and extends in that direction to about only S. by W.

10 4 P.M —No segment now seen, but patches of Indian or cherry red on either side of the magnetic meridian at a distance of 30° to 40° from it.

10.8 P.M.—The whole has almost disappeared.

10.19 P.M.—An intense blood-red patch at S.S.E. having its centre 2° to 3° below the Cross. The stars shining through it with subdued light.

10.20 P.M.—The red light all gone, but a broad conical space of an ash-grey colour, with a slight green tinge, low down on the horizon, and apparently bisected by the magnetic meridian. Resembling early dawn.

10.22 P.M.—A dimly defined arch of a smoky red colour extends from about E.S.E. to S.W. by W. The height of its vertex is about 40° above the horizon.

10.24 P.M.—All gone.

10.25 to 10.30 P.M.—Appearing and disappearing. Some faint streaks of whitish light seen low down

10.34 P.M.—Six bands of faint whitish light near horizon at S. by E.

10.37 P.M.—A bright meteor of first magnitude travelled slowly from α Centauri towards N. by E. It had a train of light and left sparks behind it. Colour white with a yellow tinge. The auroral bands brighter and higher.

11 P.M.—Sixteen luminous bands of a steel grey to a silver white colour, extending from as low down as I can see to within 20° of the zenith. The extremity of one of them is close to Canopus. Light of the Great Magellan cloud enfeebled. No apparent convergence of the beams; they seem to be quite parallel.

11.6 P.M.—The parallel bands are still seen. They cover the greater part of the hemisphere, extending (at the meridian) to about 72° above the horizon. On their eastern and western extremes there are patches of blood-red light, but none in the intermediate space. Some of the bands appear to be folded in a direction from west to east.

11.7 P.M.—Dying away.

11.15 P.M.—A deep red glow from E. to W. by S. along the horizon. Fourteen parallel bands of a silvery colour, with dark bands between them. They lie south and north, occupying nearly the whole southern hemisphere as far as the eye can reach, and are flanked at east and west by patches of blood and cherry red.

11.24 P.M.—The bands have disappeared. There is a deep red glare at E.S.E. and a lighter one at W.S.W.

11.28 P.M.—A few faint bands on either side of Canopus. A red light on their western, but none on their eastern side.

11.31 P.M.—A dark red glow at W.S.W., about 12° above the horizon.

11.33 P.M.—Clouds gathering in the lower regions of the atmosphere.

11.37 P.M.—Two parallel faint beams of whitish light 2° to 3° east of Canopus. A faint red glow at W.S.W., about 10° above the horizon.

11.42 P.M.—Two broad bands of faint whitish light to westward and three to eastward of Canopus. A patch of red light still at W.S.W. near horizon.

11.46 P.M.—Clouds gone. Aurora gone.

11.49 P.M.—A faint red glow at W.S.W. about 10° above the horizon, and a band of faint greyish light about 2° west of Canopus.

11.51 P.M.—The glow at W.S.W. is brighter and higher.

11.58 P.M.—Much fainter.

0.34 A.M.—A segment of dark red light from S.E. by S. to W.S.W., and rising at its middle to about 45° above the horizon.

1.20 A.M.—A bright red glow from S E. to S.W. Intensest below the Centaur. Soon died away. J. MELDRUM

Royal Alfred Observatory, Mauritius, February 6

GEOLOGY

Supposed Legs of Trilobites*

MR. HENRY WOODWARD, of the British Museum, in a reply to the paper by the writer in vol i., p. 320, of the present series of this Journal, supports the view that the supposed legs are real legs. He says that the remark that the calcified arches were plainly a calcified portion of the membrane or skin of the under surface is "an error, arising from the supposition that the matrix represented a part of the organism." But Prof. Verrill, Mr. Smith, and myself, are confident that there is on the specimen an impression of the skin of the under surface, and that this surface extended and connected with the arches, so that all belonged distinctly together.

Moreover the arches are exceedingly slender, far too much so for the free legs of so large an animal; *the diameter of the joints is hardly more than a sixteenth of an inch outside measure; and hence there is no room inside for the required muscles.* In fact, legs with such proportions do not belong to the class of Crustaceans. Moreover the shell (if it is the shell of a leg instead of a calcified arch) is relatively thick, and this makes the matter worse.

We still hold that the regular spacing of these arches along the under surface renders it very improbable that they were legs. Had they been closely crowded together, this argument would be of less weight; but while so very slender, they are a fourth of an inch apart. Mr. Woodward's comparison between the usual form of the arches in a Macrouran and that in the trilobite does not appear to us to prove anything. We therefore still believe that the specimen does not give us any knowledge of the actual legs of the trilobite. Mr. Woodward's paper is contained in vol. vii., No. 7, of the *Geological Magazine*.

J. D. DANA

PHYSIOLOGY

Blood Crystals

AN interesting volume has just been published by M. W. Preyer on Blood Crystals. The literature of this subject, which dates no farther back than 1840, is already extensive, no less than 143 authors being quoted in the "Bibliography," some of whom, as Böttcher, Hoppe-Seyler, Kühne, Lehmann, Rollett, Valentin, and M. Preyer himself, have written many separate essays on points bearing more or less directly upon the crystallisation of the blood. Though blood crystals were first observed by Hünefeld, the merit of discovering them is due to Reichert, who first recognised their nature. The fact of the crystallisation of a complex organic substance like blood was first received with some amount of incredulity, but the corroborative testimony of many microscopists soon cleared away all doubt, and a variety of methods were suggested by which the crystals could be obtained. The best plan for obtaining them is thus given by M. Preyer. The blood is received into a cup, allowed to coagulate, and placed in a cool room for twenty-four hours. The serum is then poured off, and a gentle current of cold distilled water passed over the finely divided clot placed upon a filter until the filtrate gives scarcely any precipitate with bichloride of mercury. A current of warm water (30°—40° Cent.) is now poured on the clot, and the filtrate received in a large cylinder standing in ice. Of this a small quantity is taken, and alcohol added drop by drop till a precipitate falls from which an estimate may be made of the quantity required to be added to the whole *without* producing a precipitate. The mixture, still placed in ice, after the lapse of a few hours, furnishes a rich crop of crystals. The forms of the crystals obtained from the blood of different animals do not vary to any great extent, and are all reducible to the rhombic and hexagonal systems. The vast majority are rhombic prisms, more or less resembling that of man. The squirrel, however, with several of the Rodentia, as the mouse and rat, and the hamster, are hexagonal. The hæmoglobin of several corpuscles is required to form a single crystal. All blood crystals are double refracting. The animals whose blood has been hitherto examined and found to crystallise, are—man, monkey, bat, hedgehog, mole, cat, lion, puma, fox, dog, guinea pig, squirrel, mouse, rat, rabbit, hamster, marmot, ox, sheep, horse, pig, owl, raven, crow, lark, sparrow, pigeon, goose, lizard, tortoise, serpent, frog, dobule, carp, barbel, bream, rudd, perch, herring, flounder, pike, garpike, earthworm, and nephelis. The spectrum of blood-colouring matter when oxidised with its two absorption striæ between D and E of Fraunhofer's lines or in the yellow part of the ordinary spectrum, and the single band of deoxidised hæmoglobin, are now well known. M. Preyer states he has not been able to obtain a spectrum from a *single* blood corpuscle, but that the characteristic bands are visible where certainly only a very few are present. The specific gravity of dry hæmoglobin he gives at about 1·3—1·4. The solubility of the crystals obtained from different animals varies considerably. Those of the guinea-pig and squirrel dissolving in water with great difficulty. Hæmoglobin is insoluble in absolute alcohol, æther, the volatile and fixed oils, in benzole, turpentine, chloroform, and bichloride of carbon. It is easily soluble in alkalies; acids rapidly decompose it. He calculates out for it the fearful formula of $C_{600}\ H_{960}\ N_{154}\ Fe,\ S_3\ O_{179}$, as agreeing very accurately with the percentage results of its analysis. Its equivalent is 4444, 4. Many pages of M. Preyer's work are occupied with an account of the action of various reagents upon it. The plates contain the forms of the principal crystals, and thirty-two spectra lithographed in colours. He describes five crystallisable products of the decomposition of hæmoglobin, namely, hæmin, hæmatosin, hæmatoidin, hæmatochlorin, and hæmatolutein, and several uncrystallisable, such as methæmoglobin, hæmatin, and hæmathion.

H. P.

* From the *American Journal of Science and Arts* for March 1872.

SCIENTIFIC SERIALS

Annalen der Chemie und Pharmacie, September 1871.—Kochlin has continued his researches on "compounds of the camphor group." By the action of nitric acid on camphor the author has obtained a new body, $C_9H_{12}O_5$, which he calls camphoronic acid, and which has the property of forming salts in which H_2 and H_3 are replaced by metals. By distillation with potassic hydrate, butyric acid is produced; with bromine in presence of water camphoronic acid is oxidised, yielding oxy-camphoronic acid; this acid forms salts, in which H_1, H_2, and H_3 are replaced by metals.—An important physiologico-chemical paper follows by Hlasiwetz and Habermann on "Proteids," and a paper by Naumann on the length of time for the evaporation and condensation of solid bodies," which, however, do not possess much general interest.—Bender contributes a paper on the "hydrate of magnesic oxychloride." This substance, however, does not appear to be very stable, or to have very marked properties.—Mulder has experimented on allantoin and bodies derived therefrom; by the action of nitric acid two substances are obtained, allanic and allanturic acid.—An interesting paper on a new series of aromatic hydrocarbons, by Zincke, follows; by heating together benzol, benzyl-chloride and zinc powder, or finely divided copper, a reaction sets in with the evolution of hydrochloric acid gas, and the partial formation of a metallic chloride; the principal reaction seems to be, however, $C_7H_7Cl + C_6H_6 = C_{13}H_{12} + HCl$. Benzyl-benzol is a solid crystalline body, melting at 26°—27°, and boiling at 261°—262°; by oxidation it is transformed into $C_{13}H_{10}O$, a crystalline body belonging to the monoclinic system, which fuses at 26°—26·5°. Benzophenon, however, has the same composition, but crystallises in the rhombic system, and fuses at 48°—49°; the body obtained above is therefore an isomeric benzophenon, it, however, easily passes into the modification fusing at 48°—49°. The composition of benzyl-benzol will therefore probably be $C_6H_5—CH_2—C_6H_5$.—This number concludes with the translations of two papers by Messrs. Friswell and Armstrong respectively, which have already appeared in the English journals.

THE *Geological Magazine* for January (No. 91) opens with a paper on a subject connected with an important branch of geology which is too much neglected in this country, and, indeed, has but few cultivators anywhere, namely, the microscopic structure of the so-called igneous rocks. This is Mr S. Allport's notice of the microscopic structure of the pitchstones of Arran, the appearance of the sections of which under the microscope is, as described by Mr. Allport, exceedingly beautiful; and it is to be hoped that this paper and the illustrations accompanying it may induce others to enter upon this most interesting and important line of research.—The Rev. O. Fisher contributes a note on "Cirques and Taluses," with reference to Mr. Bonney's paper in the December number of the magazine Mr. Fisher ascribes an essential part in the excavation of cirques to glacial action.—Mr. D. Forbes communicates a severe criticism of some remarks made by Mr. A. H. Green in his account of the geology of part of the county of Donegal.—"The Age of Floating Ice in North Wales" is the subject of a paper by Mr. D. Mackintosh; and Mr. James Geikie publishes the second part of his "Memoir on Changes of Climate during the Glacial Epoch."—The number includes the usual notices and reviews.

Mémoires de la Société des Sciences Naturelles de Cherbourg. Tome xv. (Deuxième Série, Tome v.) 1870. "On the Swell and Roll of the Sea," by W. Bertin.—"Notes on the Comora and Seychelles Archipelagos," by M. Jouan. These islands were visited in 1850; a very brief list of the flora and fauna is appended. The list of birds has been apparently overlooked in the Zoological Record for January 1870.—"Notes on the Tubercles met with in *Callitriche autumnalis*," by MM. Karelschtikoff and Rosanoff, with a plate.—"On the *Lophobranchs*," by M. Dumeril.—"Notes of a Visit to Aden, Point de Galle, Singapore, and Tché-fou," by M. Jouan.—"On the Influence of Climate on the Growth of some Resinous Trees," by M. Békétoff.—"Geological Essay on the Department of La Manche," by M. Bonissent. "Supplementary notes to a paper on the Swell and Roll of the Sea," by M. Bertin.—Works received by the Society from July 1869 to August 1870.

Proceedings of the Natural History Society of Dublin, for the Session 1869-70, 1870-71, vol. vi., part i. (Dublin 1871) contains the following papers by Dr. A. W. Foot:—1, Notes on Irish Lepidoptera; 2, On Goitre in Animals; 3, On the Breeding of some Birds from the Southern Hemisphere in the Dublin Zoological Gardens; 4, Notes on Animal Luminosity; 5, Notes on Entomology; 6, Notes on Irish Diptera; 7, On some Irish Hymenoptera; and the following by Mr. William Andrews:—1, On the Inhabitants of the Rock-pools and caves of Dingle Bay, records, as new to the fauna of Ireland, *Aiptasia couchii*, *Stomphia churchiæ*, *Balanophyllia regia*, *Capnea sanguinea*, and "a deep-water species of stony coral, formed by hydroid animals, and related to the Tabulate Madrepores, which is nearly allied to, and indeed considered identical with, *Millepora alcicornis* of Linnæus;" 2, Ichthyological Notes; 3, On *Orthagoriscus oblongus*, with two plates; 4, On some rare Crustacea from the south-west of Ireland; 5, On the Ichthyology of the south-west of Ireland; 6, Notes on Hymenophylla, especially with reference to New Zealand species; 7, On some Irish Saxifrages; also papers by Prof. Macalister, on the mode of growth of Discoid and Turbinated shells; by G. H. Kinahan, on the Ferns of West Connaught and the south-west of Mayo.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 7.—"On the organisation of the Fossil Plants of the Coal-measures.—Part III. *Lycopodiaceæ.*" By Prof. W. C. Williamson, F.R.S. An outline of the subject of this memoir has already been published in the Proceedings in a letter to Dr. Sharpey. In a former memoir the author described the structure of a series of Lepidodendroid stems, apparently belonging to different genera and species. He now describes a very similar series, but all of which, there is strong reason for believing, belong to the same plant, of which the structure has varied at different stages of its growth. The specimens were obtained from some thin fossiliferous deposits discovered by Mr. G. Grieve, of Burntisland, in Fifeshire, where they occur imbedded in igneous rocks. The examples vary from the very youngest half-developed twigs, not more than $\frac{1}{12}$th of an inch in diameter, to arborescent stems having a circumference of from two to three feet. The youngest twigs are composed of ordinary parenchyma, and the imperfectly developed leaves which clothe them externally have the same structure. In the interior of the twig there is a single bundle, consisting of a limited number of barred vessels. In the centre of the bundle there can always be detected a small amount of primitive cellular tissue, which is a rudimentary pith. As the twig expanded into a branch, this central pith enlarged by multiplication of its cells, and the vascular bundle in like manner increased in size through a corresponding increase in the number of its vessels. The latter structure thus became converted into the vascular cylinder so common amongst Lepidodendroid plants, in transverse sections of which the vessels do not appear arranged in radiating series. Simultaneously with these changes the thick parenchymatous outer layer becomes differentiated. At first but two layers can be distinguished—a thin inner one, in which the cells have square ends, and are disposed in irregular vertical columns, and a thick outer one consisting of parenchyma, the same as the epidermal layer of the author's preceding memoir. In a short time a third layer was developed between these two.

When the vascular cylinder had undergone a considerable increase in its size and in the number of its vessels, a new element made its appearance. An exogenous growth of vessels took place in a cambium layer, which invested the pre-existing vascular cylinder. The author distinguishes the latter as the vascular medullary cylinder, and the former as the ligneous zone. The newly-added vessels were arranged in radiating laminæ, separated from each other by small but very distinct medullary rays. At an earlier stage of growth traces of vascular bundles proceeding from the central cylinder to the leaves had been detected. These are now very clearly seen to leave the surface of the medullary vascular cylinder where it and the ligneous zone are in mutual contact; hence tangential sections of the former exhibit no traces of these bundles, but similar sections of the ligneous zone present them at regular intervals and in quincuncial order. Each bundle passes outwards through the ligneous zone, imbedded in a cellular mass, which corresponds, alike in its origin and in its direction, with the ordinary medullary rays, differing from them only in its larger dimensions. At this stage of growth the plant is obviously identical with the *Diploxylon* of Corda, with the *Anabathra* of Witham, and, so far as this internal axis is concerned, with the *Sigillaria elegans* of Brongniart. The peculiar medullary vascular cylinder existing in all these plants is now shown to be merely the developed vascular bundle of ordinary Lycopods, whilst the exogenous radiating ligneous zone enclosing that cylinder is an additional element which has no counterpart amongst the living forms of this group.

Though the central compound cellulo-vascular axis continued to increase in size with the general growth of the plant, it was always small in proportion to the size of the stem. The chief enlargement of the latter was due to the growth of the bark, which exhibited three very distinct layers,—an inner one of cells with square ends, and slightly elongated vertically and arranged in irregular vertical rows, an intermediate one of prosenchyma, and an outer one of parenchyma. These conditions became yet further modified in old stems. The exogenous ligneous zone became very thick in proportion to the medullary vascular cylinder, and the differences between the layers of the bark became yet more distinct. These differences became the most marked in the prosenchymatous layer; at its inner surface the cells are prosenchymatous, but towards its exterior they become yet more elongated vertically, their ends being almost square, whilst numbers of them of exactly equal length are arranged in lines radiating from within outwards. These oblong cells often pass into a yet more elongated series with somewhat thickened walls, which become almost vascular, constituting a series of bast-fibres. In the transverse sections these prosenchymatous cells are always arranged, like the vessels of the ligneous zone, in radiating lines. Yet more external is the sub-epidermal parenchyma passing into leaves composed of the same kind of tissue. The petioles of the leaves have been long, if not permanently, retained in connection with the stem, a character of Corda's genus *Lomatophloios*.

Where young twigs branch, the vascular medullary cylinder divides longitudinally into two parts; the transverse section of this cylinder now resembles two horse-shoes pointing in opposite directions. The break in the continuity of each half of the cylinder occasioned by the division is never closed by new vessels belonging to the cylinder; but when the stem develops exogenously, the cambium-layer, from which the new growths originated, has endeavoured to surround these openings in the cylinder, and, by closing them, once more to separate the medullary from the cortical tissues. Some beautiful specimens have been obtained, which exhibit these new exogenous layers in process of formation. The vessels of the young layers are not half developed. At first they meander vertically through masses of delicate cellular tissue; but they soon arrange themselves in regular radiating vessels and cells, becoming mere outward prolongations of the woody wedges and medullary rays of the older part of the stem. At this stage of their growth, the walls of the vessels are deeply indented by the contiguous cells, as if the plastic issues of the former had been moulded upon the latter structures. As the new vessels enlarge, the superfluous intervening cells disappear, until each medullary ray finally consists of a single vertical pile of from one to a small number of cells, arranged in as many Conifera. The exceptional cases are those where vascular bundles pass outwards to the leaves; these bundles have protected the contiguous cells above and below them from the pressure of the enlarging ligneous vessels and limited their absorption. Both these and the smaller ordinary rays pass outwards in horizontal and parallel lines. The evidences of an exogenous mode of growth afforded by these young, half-developed layers of wood is clear and decisive.

The Burntisland deposits are full of fragments of strobili, especially of torn sporangia and of macrospores. Several fine *Lepidostrobi* have been obtained, like those to which the fragments have belonged, and which the author believes to have been the fruits of the stems described. The structure of these strobili is very clear and of interest; the primary branches from the central axis subdivide, so that each sporangium rests upon a separate bract, from the upper surface of which a vertical lamina arises, and, extending the entire length of the sporangium, ascends far into its interior, where it bifurcates. The cellular walls of the sporangium blend with the bract along each side of this sporangiophore. The microspores occupy the upper part of the *Lepidostrobus*, and are usually triplospores, sometimes tetraspores. The macrospores occupy the lowermost sporangia, are of large size, and are very remarkable from having their external surfaces clothed with numerous projecting caudate appendages, each one of which is slightly capitate at its extremity. So far as the author is aware, this is an undescribed form of macrospore.

Two new forms of *Lepidodendron* are described from the Oldham beds, in one of which the medullary axis attains to an unusually large size, even in the young shoots; whilst the other is remarkable for the magnitude of its leaves. It is obvious that the plant which is the chief subject of the memoir is a true example of Corda's genus *Diploxylon*, so far as its woody axis is concerned; whilst its bark and leaves are those of a *Lomatophloios*, and its slender twigs are *Lepidodendra*. The author also points out the probability that the plant had a true Stigmarian root.

The structure of these fossil types is compared with that of recent *Lycopodiaceæ*. The vascular medullary cylinder is shown to be an aggregation of the foliar vascular bundles, so that the vascular connection between the leaves and the stem is maintained exclusively by means of these vessels, which thus correspond most closely with the central vascular axes of living Lycopods. On the other hand, the exogenous layers do not communicate directly with the leaves in any way; but, on the other hand, they are homologous with the corresponding layers in the Stigmarian root, in which latter they receive the vascular bundles from the rootlets. The medullary cylinder does not enter the roots, but appears to terminate at the base of the stem, though the pith is prolonged through them. Hence it seems probable that the nutritive matters were taken up from the soil by the Stigmarian rootlets, that it ascended into the Diploxyloid stem through the exogenous layer, but that, in order to reach the leaves, if conveyed by the vessels, and not by the cellular tissues, it had to be transferred by endosmosis to those of the medullary cylinder. The bark of the fossil plants is compared with those of *Lycopodium chamæcyparissus*, and *Selaginella Martensii*, which two combined represent the former.

These discoveries necessitate some changes in generic nomenclature, since the several parts of the plant not only represent the three genera above mentioned, but also several others. Meanwhile some other errors require correction. Corda erroneously defined his genus *Diploxylon* as having no medullary rays, and Brongniart relied upon this distinction in separating *Diploxylon* from *Sigillaria*; but no difference exists between the ligneous structures of the two genera, so far as *Sigillaria* is illustrated by Brongniart's *S. elegans*. Corda, Brongniart, and King all agree in regarding *Diploxylon* (which is identical with Witham's *Anabathra*) as a Gymnospermous Exogen. The necessity for abandoning this separation of the plants in question from the *Lycopodiaceæ*, urged in the author's previous memoir, is now made more obvious than before, the distinctions upon which the great French botanist relied in his classification being now shown to be such as mere differences of age can produce. The author concludes from his own observations that the genera *Diploxylon*, *Anabathra*, *Lomatophloios*, and *Leptoxylon* must be united. Brongniart had already brought into one generic group Corda's genera *Lomatophloios*, *Leptoxylon*, and *Calamoxylon*, Göppert's genus *Pachyphyllum*, and Sternberg's genus *Lepidophloios*, giving the latter name to the whole. Hence no less than six obsolete generic names are disposed of. The author finally follows Brongniart in adopting the term *Lepidophloios*, and *temporarily* assigns to the plant described the trivial name of *L. brevifolium*. The further relations of this genus to more ordinary forms of *Lepidodendron* require further investigation.

Linnean Society, March 7.—Mr. G. Bentham, president, in the chair. "Revision of the genera and species of *Scilleæ*," by J. G. Baker. This paper contained technical details of the new groups and genera proposed of this difficult tribe of Liliaceæ in continuation of papers already presented to the society.—"On the Andrœcium in *Cochliostema*," by Dr. M. T. Masters. In this singular genus of Commelynaceæ, from the Amazon region, the staminal arrangement is different to anything else observed in the vegetable kingdom. There are three petaloid stamens, all arranged on the posterior side of the pistil, within which are three spiral bodies constituting the anthers. Within these are three staminodes, one of which is not developed till a considerably later stage than the other two; they do not appear to have any physiological value. The mode of fertilisation is obscure; the stamens and styles are both so completely obscured that self-fertilisation seems impossible.—"On a supposed hybrid between *Vaccinium Myrtillus* and *V. Vitis-Idæa*," by Mr. Gardner. In the discussion which followed, the prevalent opinion was that the plant was but a variety of *V. Vitis-Idæa*.—"A list of the Marine Algæ of St. Helena," by Dr. Dickie. These are twenty-one in number, all dwarf, and, notwithstanding the remarkable peculiarity of the terrestrial vegetation, only one species is peculiar to the island.—"Catalogue of new *Leguminosæ* from Western India," by N. A. Dalzell.

Chemical Society, March 7.—Prof. Williamson, F.R.S., vice-president, in the chair.—In the course of the ordinary business of the society, the proposed changes in the officers and council of the society for the ensuing year were announced.—Dr. Debus, F.R.S., then read a paper "On the reduction of ethylic oxalate by sodium amalgam." In 1864 Dr. Friedlander described, as the result of this reaction, the production of the sodium salt of a new acid, which he named glycolinic acid. Although the author has carefully repeated Dr. Friedlander's experiments, and varied the details of the process in different ways, he has been unable to obtain glycolinic acid, the only acids formed being glycollic and tartaric. A comparison of the crystalline form of a specimen of sodium glycolinate, prepared by Friedlander, with that of sodium glycollate, would seem to indicate that it is identical with the latter.—Two other papers were read, one "On metastannic acid, and the detection and estimation of tin," by A. H. Allen; and the other, "Note on the quantity of cæsium contained in the water of the hot springs found in Wheal Clifford," by Colonel Philip Yorke, F.R.S., from which it appears that a gallon of this water contains 26 grs. of lithium chloride and one million parts 1·7 of cæsium chloride, or more than ten times as much of the latter as the Dürkheim water, in which, it will be remembered, that element was first detected by Kirchhoff and Bunsen in 1860.

Zoological Society, March 5.—Mr. John Gould, F.R.S., vice-president, in the chair. Mr. Howard Saunders exhibited and made remarks on specimens of *Falco barbarus* and *Cypselus pallidus*, obtained in Southern Spain, being the first recorded occurrences of these species on the continent of Europe.—A letter was read from Mr. Walter J. Scott, of Queensland, giving some further information respecting the supposed existence of an undescribed large carnivorous animal in that colony. This letter was accompanied by drawings of the impression of the foot of the animal.—Mr. A. H. Garrod read some notes taken on the dissection of an ostrich, recently living in the Society's menagerie. The examination of this bird proved that its death was due to copper poisoning, a number of copper coins and pieces of coin in a much worn state having been found in its stomach.—Mr. E. W. H. Holdsworth read a paper containing a catalogue of the birds found in Ceylon, with remarks on their localities and geographical distribution; and gave a description of two new species, which were proposed to be called *Zosterops ceylonensis* and *Arrenga blighi*. The total number of Ceylonese birds included in Mr. Holdworth's list was 323, of which 36 were stated to be peculiar to the island.—A communication was read from Dr. Hermann Burmeister, containing a list of the species of the Lamellirostral birds of the Argentine Republic, with remarks on their habits and times of occurrence.—A communication was read from Dr. W. Peters, containing a list of a collection of small mammalia recently made by Mr. J. J. Monteiro in Angola.—Dr. J. E. Gray communicated some notes on a new species of tapir (*Tapirus leucogenys*) from the snowy regions of the Cordilleras of Ecuador, recently obtained by Mr. Buckley; to which were added some observations on the young spotted tapirs of Tropical America.

Society of Biblical Archæology, March 5.—Dr. Birch, president, in the chair.—Mr. J. W. Bosanquet read a paper "Concerning Cyrus, son of Cambyses, grandson of Astyages, who took Babylon; as distinguished from Cyrus, father of Cambyses, who conquered Astyages." In this paper, the learned chronologist endeavoured to show that, contrary to the received opinion of historians, Cyrus, son of Cambyses, though leader of the Medes as early as the year B.C. 535, was contemporary with the early part of the reign of Darius Hystaspes; having taken the throne of the Persian Empire after the death of his father. This view he believed to be consonant with the results of recent discoveries, and afforded a satisfactory explanation of the confessedly difficult chronology of Ezra and the Chaldee writers. Mr. Bosanquet summed up his argument as having proved:—(1) that Cyrus, father of Cambyses, who conquered Astyages, neither conquered Babylon nor reigned in Babylon, as Ptolemy assumes in his Babylonian Canon; (2) that Cyrus, son of Cambyses, King of Persia, grandson of Astyages, twice conquered Babylon; but did not reign over Babylon till after his father's death in B.C. 518; (3) that Ptolemy's Canon rests upon no sound authority, either historical or astronomical, as regards placing the reign of Cyrus at Babylon before the reign of Cambyses; (3) that the alternative reckoning deduced from Demetrius is to be preferred to that of Ptolemy, as resting upon the dates of three solar eclipses.

Anthropological Institute, March 4.—Mr. G. Harris, vice-president, in the chair.—Mr. Charles F. Tyrrwhitt Drake was elected a member.—Captain Richard F. Burton read his third paper "On Anthropological Collections from the Holy Land." It contained accounts of the Hamath Inscriptions, facsimiles of which were exhibited, and of skulls from Siloam. An interesting discussion was raised on the high antiquity of the Hamath Inscriptions. Dr. Carter Blake described the human remains brought by Captain Burton from Siloam, and by M. Ganneau from the "Tomb of Jesus," near that place; the former were stated to be undoubtedly Jewish, and the latter of modern Turkish origin. Mr. J. Gould Avery read a paper "On Race-characteristics as related to Civilisation."

BOOKS RECEIVED

English.—Dr Pereira's Elements of Materia Medica: Edited by Bentley and Redwood (Longmans).—Sir John Herschel's Outlines of Astronomy, 11th edition (Longmans).—Science Primers: Chemistry, by Prof. H. E. Roscoe; Physics, by Prof. Balfour Stewart (Macmillan).—Astronomy and Geology compared: Lord Ormathwaite (J. Murray)—The Higher Ministry of Nature: J. R. Leifchild (Hodder and Stoughton).

Foreign.—Annuaire de l'Académie Royale de Belgique, 1871. (Through Williams and Norgate.)—Lehrbuch der Botanik: Dr. O. W. Thomé, 2te Auflage.

DIARY

THURSDAY, March 14.

Royal Society, at 8.30.—Contributions to the History of the Opium Alkaloids.—IV.: Dr. C. R. A Wright.—Further Investigations of Planetary Influence on Solar Activity: W. De La Rue, F.R.S., B. Stewart, F.R.S., and B. Loewy.—The Decomposition of Water by Zinc in connection with a more Negative Metal: Dr Gladstone, F.R.S., and A. Tribe.

Society of Antiquaries, at 8.30.—Stone Altar and Thurible from Syria: Capt. Burton. Further Facts in the History of the Discovery of Australia: R. H. Major, F.S.A.

Mathematical Society, at 8.—Shall the Society apply for a Charter?

Royal Institution, at 3.—On the Chemistry of Alkalies and Alkali Manufacture; Prof. Odling, F.R.S.

FRIDAY, March 15.

Royal College of Surgeons, at 4.—On the Digestive Organs of the Vertebrata: Prof. Flower, F.R.S.

Royal Institution, at 9.—The Alphabet and its Origin: J. Evans, F.R.S.

SATURDAY, March 16.

Royal Institution, at 3.—Demonology: M. D. Conway.

Association of Medical Officers of Health, at 7.30.—Mr. Stansfeld's Public Health Bill: Dr. A. W. Barclay.—On the Criminal Deaths of Infants, as shown by the Records of the Coroner's Court of Liverpool: F. W. Lowndes.

MONDAY, March 18.

Royal College of Surgeons, at 4.—On the Digestive Organs of the Vertebrata: Prof. Flower, F.R.S.

Anthropological Institute, at 8.—Comparative Longevity of Man and Animals: George Harris.—Physical Condition of Centenarians: Sir Duncan Gibb, Bart., M.D.

TUESDAY, March 19.

Royal Institution, at 3.—On the Circulatory and Nervous Systems: Dr. Rutherford.

Zoological Society, at 9.—Report on additions to the Society's Menagerie in February, 1872: The Secretary.—On a specimen of the Broad-fronted Wombat (*Phascolomys latifrons*): Prof. Macalister.

Statistical Society, at 7.45.

WEDNESDAY, March 20.

Royal College of Surgeons, at 4.—On the Digestive Organs of the Vertebrata: Prof. Flower, F.R.S.

Geological Society, at 8.—On the Wealden as a fluvio-lacustrine Formation, and on the relation of the so-called "Punfield Formation" to the Wealden and Neocomian: C. J. A. Meyer, F.G.S.—Notes on Atolls or Lagoon Islands: S. J. Whitnell.—On the Glacial Phenomena of the Yorkshire Uplands: J. R. Dakyn.—Modern Glacial action in Canada: Rev. W. Bleasdell, M.A.

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NOTICE

We beg leave to state that we decline to return rejected communications, and to this rule we can make no exception. Communications respecting Subscriptions or Advertisements must be addressed to the Publishers, NOT *to the Editor.*

THURSDAY, MARCH 21, 1872

THE HISTORY OF THE ROYAL INSTITUTION*

NO other Institution has been so closely associated with the greatest discoveries of Chemical and Physical Science during the present century as that which has its abode in the well-known building in Albemarle Street. The names of Rumford, Banks, Young, Davy, Faraday, Tyndall, will always add lustre to its annals; nor will it be forgotten that in its laboratory were made the most famous discoveries of Davy and Faraday. Dr. Bence Jones gives us in this very interesting volume a sketch of the history of the Institution, derived from its own record of proceedings, interspersed with biographical notices of its founder, Count Rumford, and its most eminent professors, Garnett, Young, and Davy. Of Faraday we hear comparatively little, Dr. Bence Jones having sketched his life in a separate biography; and with regard to the eminent men whose present connection with the Institution is adding fresh popularity to its courses of lectures, he is altogether silent.

Probably few of the visitors who now attend the lectures at the Royal Institution, or who crowd to its fashionable Friday evening *réunions*, are aware of the object with which it was originally founded, as shown in the prospectus drawn up by Count Rumford in 1799, from which the following are extracts:—

"Proposals for forming by subscription, in the metropolis of the British Empire, a public Institution for diffusing the knowledge and facilitating the general introduction of useful mechanical inventions and improvements, and for teaching by courses of philosophical lectures and experiments the application of science to the common purposes of life:—

"The two great objects of the Institution being the speedy and general diffusion of the knowledge of all new and useful improvements, in whatever quarter of the world they may originate, and teaching the application of scientific discoveries to the improvement of arts and manufactures in this country, and to the increase of domestic comfort and convenience, these objects will be constantly had in view, not only in the arrangement and execution of the plan, but also in the future management of the Institution.

"As much care will be taken to confine the establishment within its proper limits as to place it on a solid foundation, and to render it an ornament to the capital and an honour to the British nation.

"In order to carry into effect the second object of the Institution, namely, 'Teaching the application of science to the useful purposes of life,' a lecture-room will be fitted up for philosophical lectures and experiments, and a complete laboratory and philosophical apparatus, with the necessary instruments, will be provided for making chemical and other philosophical experiments."

This basis was adhered to, and these eminently practical objects were steadily kept in view, as long as the management remained with the original founders of the Institution; but it soon passed into the second stage of its existence. Count Rumford had fixed his residence abroad during the latter years of his life, the eminent men whom he had collected around him, headed by his intimate friend and ally, Sir Joseph Banks, withdrew from its active management, and its control passed into the hands of others, whose chief aim was to recruit its exhausted funds by making the Royal Institution one of the most fashionable places of resort in London. In this they succeeded; but their success was mainly due to the extraordinary interest which centred round the remarkable discoveries of young Davy which signalised the early years of the century. When we read the history of these discoveries, following one another in quick succession—the determination of the true constitution of the alkalies and alkaline earths, potassa, soda, lime, magnesia, the decomposition of ammonia—each a link in the chain of investigation which produced a complete revolution in chemical philosophy, we are disposed to query whether future diligent workers in the fields of science will ever again be rewarded by discoveries of similar gigantic importance.

The sketch of the life of Sir Benjamin Thompson, Count Rumford of the Holy Roman Empire, as presented by Dr. Bence Jones, shows a character full of strange contradictions. A native of North America, during the War of Independence an ardent Royalist, and throughout his life imbued with aristocratic principles of the strongest tinge, he yet spent all his energies in physical discoveries and mechanical inventions calculated to ameliorate the condition of the masses, and to promote the health and comfort of their lives. It was indeed for the purpose of forwarding this object mainly, as we have seen, that he projected the establishment of the Royal Institution. A man of the warmest affections, he yet compelled his second wife (Lavoisier's widow), to seek relief from domestic unhappiness in a judicial separation. With a remarkable power of attracting around him, and moulding to his views, the most eminent men in various branches of science, there were yet few whom he did not estrange from him by his morbid jealousy, and by the eccentricity of his conduct. The littlenesses of his character will, however, be forgotten in the noble aims and great results of his life.

We are glad to have recalled to us in this volume the career of so disinterested a student of Science as Dr. Thomas Young, and to find a full recognition of his eminent position as the *avant-courier* of Davy and Faraday. Born in Somersetshire in 1773, he showed in his school-boy days that precocity of intellect and power of acquiring knowledge in almost any subject presented to him, which does not always mark the future genius. After spending the years from fourteen to nineteen as a private tutor, he became in 1793 a student at St. Bartholomew's Hospital, presented during the same year a paper to the Royal Society on the "Structure of the Crystalline Lens," and in 1794, at the age of twenty-one, was elected a Fellow of that body. From 1799 to 1801 Dr. Young was carrying on his remarkable series of experiments on Sound and Light, and in the latter year was appointed Professor of Natural Philosophy to the Royal Institution. His lectures however were not considered sufficiently popular for the audiences that then frequented it, and his connection with it terminated in 1803. During the next twenty years of his life he practised as a physician in London, being connected with St. George's Hospital. In 1818 he was appointed

* "The Royal Institution: Its Founders and its First Professors." By Dr. Bence Jones, Honorary Secretary. (London: Longmans and Co. 1871.)

superintendent of the "Nautical Almanack" and secretary of the Board of Longitude, and in 1827, on the resignation of Sir Humphry Davy, was spoken of as a probable successor to his office of President of the Royal Society, Davies Gilbert, however, being chosen. He died in 1829, at the age of 56, and his character was thus drawn by his intimate friend Sir Humphry Davy:—"A man of universal erudition and almost universal accomplishments. Had he limited himself to any one department of knowledge, he must have been first in that department. But as a mathematician, a scholar, and a hieroglyphist, he was eminent; and he knew so much that it was difficult to say what he did not know."

Sir Humphry Davy's brilliant career, and especially that portion of it which contributed so greatly to the fame and success of the Institution with which he was connected, is drawn in detail by his biographer; and the failings in his character and in his life which obscured its lustre to his contemporaries are in no way concealed. The following contrast of the characters of Davy and of his pupil and successor, Faraday, will be read with interest:—"Whenever a true comparison between these two nobles of the Institution can be made, it will probably be seen that the genius of Davy has been hid by the perfection of Faraday. Incomparably superior as Faraday was in unselfishness, exactness, and perseverance, and in many other respects also, yet certainly in originality and in eloquence he was inferior to Davy, and in love of research he was by no means his superior." As early as 1804, when Davy was only twenty-six, Dr. Dalton consulted him as to the best mode of preparing his lectures, and described him as "a very agreeable and intelligent young man, the principal failing in whose character as a philosopher is that he does not smoke;" and within two or three years from that time he had made the discoveries which have immortalised his name.

Dr. Bence Jones does not carry down the history of the Royal Institution beyond 1814, when it became as closely associated with Faraday's career as it had previously been with Davy's. We have seen what were the primary objects for the promotion of which the Institution was founded; and we know also the great work which it effected during the first ten years of its existence. These special purposes soon gave way to the effort, as our author expresses it, after striving to be fashionable; and the fashionable element has continued to be the most prominent feature in its subsequent life to the present day. Something is, no doubt, gained by making scientific subjects one of the ordinary topics of conversation in West End *salons*; the continuation of the History of the Royal Institution, which will have to be written twenty years hence, will show whether this object is compatible with the carrying on of original investigations which will add to the sum of our knowledge of the laws of Nature.

OUR BOOK SHELF

Une Expérience relative à la Question de la Vapeur Vésiculaire. Par M. J. Plateau. (Brussels: F. Hayez.)

THE elder Saussure, and after him De Luc, considered it to be an established fact that clouds are formed of little hollow globules, which Saussure designated vesicular vapours, or vesicles. These vesicles, having a structure similar to the soap bubble, were assumed to be capable of floating in the atmosphere and of remaining suspended in it so long as their physical condition was unaltered. When they became resolved into drops of water they formed rain. The same structure was assigned to the cloud formed by the condensation of the vapour of boiling water in air colder than itself. M. Plateau has endeavoured to put this view of the vesicular constitution of vapour to the test of experiment. With this view he has taken advantage of a method devised by M. Duprez, for inverting a wide tube (20mm. in diameter) full of water, so that the water may remain suspended in the tube. By means of a narrow tube drawn out at one end, so as to present an orifice of 0·4mm. in diameter, he succeeded in obtaining small hollow globules of water of less than a millimetre in diameter, and transporting them under the free surface of the water, suspended in the wide tube. As soon as contact was established with that surface, the little bubble became detached, and the air which it contained penetrating into the liquid, mounted through it. The experiment, on being several times repeated, gave always the same result. M. Plateau has applied this method to the cloud formed when water is boiled in free air. "Let us imagine," he says, "that at a certain distance from the surface of the water suspended in the wide tube, a current of visible vapour of water arises. If this vapour is composed of vesicles, each of them which comes into contact with the liquid surface must introduce into the water a microscopic bubble of air, which will immediately begin to ascend, so that the whole will form in the water of the tube a cloud which will rise slowly in it, and alter its transparency." In making the experiment, no cloud was produced, and M. Plateau concludes, in conformity with the view now generally held by physicists, that the vesicular state of vapour has no real existence. He discusses objections which may be raised to his experiment, such as the possible solution of the bubbles of air in the water, the bursting of the bubbles at the surface of the water and the escape of the air contained in them, or their rolling under the surface of the water till they reach the margin of the tube and thus get away; and shows satisfactorily that these objections do not invalidate the result at which he has arrived.

Chemical Notes for the Lecture Room, on Heat, Laws of Chemical Combination, and Chemistry of non-Metallic Elements. By Thomas Wood, Ph.D., F.C.S. Pp. 181. (London: Longmans, Green, and Co.)

ON reading this volume the author's intention is plainly manifest; the book has been written principally for the use of students preparing for the matriculation examination at the University of London. It has been written as concisely as possible, rendering the task of "cramming" the subject more easy of attainment. For such a purpose we certainly can recommend this book; but for beginners who wish to study chemistry we think it has several faults. One of them is that such a comparatively large amount of the book is devoted to the subsidiary subject, Heat, almost a quarter of the text being thus occupied. The article on thermometers, for instance, occupies no less than nine pages, which strikes us as being rather out of proportion to the remainder of the book. A second fault is the almost complete absence of any such details as would enable a student to repeat the experiments mentioned in the text. This we think is a fault which would tend to make the beginner get up his subject parrot-like, a method which is certainly not to be desired. The chemistry of the non-metallic elements only occupies eighty-five pages of this volume; the definitions and laws of chemical combination occupy another thirty-eight pages. The explanations, in the majority of instances, are clearly expressed, the facts of the case being stated in as few words as possible. A few of the definitions can scarcely be considered good; one, in particular, is "that a compound of any non-metal with a metal is a salt of a metal." This would, of course, include such bodies as antimonetted and arsenetted hydrogen, hydride of copper, and so on. The definitions of acids and bases, too, are weak. It may almost be inferred that such is the case, by the manner in which the author uses the term acid; N_2O_3 is called nitrous acid; I_2O_5 iodic acid, and, in the same line, $HBrO_3$ bromic acid; B_2O_3 boracic acid, and so on. There is one thing which the author tells us which is a curiosity in chemical history. On page 38 it is stated "some few of the elements receive their symbols *from the names given to them* by the ancients—*e.g.* Iron (Fe.) from *Ferrum*, Sodium (Na.) from *Natrium*." We certainly were under the impression that Sodium was discovered in 1807 by Sir Humphry Davy. A number of questions are appended to the book which will be found very useful to those employed in teaching.

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

SURELY Mr. Ferrel must have misapprehended my arguments, or he would not have advanced the case of the tides against me. Undoubtedly the ocean will sink to its old level when the lifting force of the moon is withdrawn, even though the height to which the waters are raised may not exceed an inch. I agree also with what he says in regard to the improbability of ocean currents being caused by the heaping up of the waters by the winds. I believe that this erroneous view of the matter has done more real mischief to the wind theory than all the arguments advanced by the advocates of the gravitation theory put together. The notion that because the winds are applied to the surface of the ocean they can produce only surface drift is an error of a similar character.

I shall shortly refer to an important point bearing on the influence of rotation overlooked both by Dr. Colding and Mr. Ferrel. In my last paper in the *Phil. Mag.*, October 1871, p. 266, there is a trifling mistake to which I shall also refer.

Edinburgh JAMES CROLL

Science Stations

ALLOW me to say a few words in reply to your editorial of Feb. 29. It does seem to me to be a pity to "run the risk of spoiling a good work" by multiplying suggestions and urging counter claims. It is not quite fair that when biologists start a proposal for obtaining a necessary but costly aid to their studies, the devotees of other sciences should exclaim, "Oh, we must have one, too!" If all speak at once in this way no one will be heard, and we shall get no stations of any sort. Probably the writer of the article is not aware of the expense and requirements of a zoological station, otherwise he would not propose to increase the difficulty by thrusting a meteorological and astronomical observatory on the backs of its promoters, and then observe that "the outlay need not be heavy." It is notorious that there are meteorological and astronomical observatories in almost every part of the globe; but there is nothing of the kind for zoology. Under these circumstances it is to me a disappointment that the suggestion for zoological stations meets with what looks like a somewhat selfish criticism, in place of unqualified support, at the hands of physicists.

As to the station in England, I do not gather from Dr. Dohrn's article that he proposes to separate teaching entirely, or even partially, from the stations. He leaves it alone. "Teaching" can come or go just as those who deal in it may please; but that instruction in rudimentary zoology should be a chief object of the station is a proposal of the same nature as would be that to make use of Greenwich Observatory for giving lessons in the outlines of astronomy, and is not entertained by him for a moment. It no doubt would be a very good thing that students from Cambridge and Oxford and London should spend some time in a zoological station; and it would also be good for others of them to work in a lead or copper mine, or pass a few nights in an astronomical observatory; but we cannot urge the wants of these particular students as any reason for the maintenance of these three things. The primary object for which zoological stations will be erected—one for which it is to be hoped that the Universities, as well as scientific societies and private individuals, will be ready to subscribe money—is the prosecution of science.

We claim for biology now a place of far higher importance in the scheme of human knowledge than she has occupied hitherto. She has proved her claim to the respect and most serious attention of men by the discovery of the principles and detailed laws of evolution—a discovery which has more widely influenced human thought than has any other product of modern science, and must continue long so to do. We are no longer content to see biology scoffed at as "inexact," or gently dropped as "natural history," or praised for her relations to medicine. On the contrary, biology is the science whose development belongs to the day. At this moment she is deserving of more attention, more material aid, more assistance in her young growth, than any other human science. Her youthful performances, her hopeful stride onwards, promise more abundant results from pecuniary aid given to her than can be hoped for from her older sisters, who have "settled in life." If biology requires "stations," she ought to be gladly supplied with them.

I must protest against the notion—urged in your article only, I imagine, as a joke—that without "teaching" (whatever that may mean) there would be danger of a zoological station becoming the home of a narrow zoological clique. The connection is not explained, and I do not think any of your readers will see it. Are observatories the homes of narrow astronomical cliques? Are telescopes without professors liable to become the resort of ambitious young persons, anxious chiefly to discover hydrogen flames where nobody had found them before? I do not believe a bit in the narrow clique suggestion. Teaching bodies breed them much more rapidly and naturally than do working bodies. And as to the privat-docent, anxious to discover a notochord, or the amateur astronomer hunting for hydrogen flames, I would most gladly see them multiplied exceeding abundantly. Would that we could obtain the institution of "privat-docents" in English Universities; by simply erecting a zoological station, would that we could infuse some of their kind of ambition—one of the best a man can have—into English students.

Naples, March 4 E. RAY LANKESTER

[*** The article to which our correspondent refers was written by a distinguished biologist.—ED.]

The Etymology of "Whin."

THE following is from Jamieson:—"*Quhyn*, *Quhin-Stane*, s. i. Green-stone; the name given to basalt, trap, &c. . . . Isl. *hwijna*, resonare, *hwin*, resonans, q. 'the resounding stone.'" "Whin, whinstane, *s.* Ragstone or toadstone."

Whin or gorse, the name given to *Ulex europæus*, common furze, is from a different root, being traced to Welsh *chwynyn* = weed. A. HALL

YOUR correspondent, Mr. W. R. Bell, will find a derivation given for "Whin" in Jamieson's "Scottish Dictionary," where, under the name *Quhyn* or *Quhin*, it is referred to the "Islandic *hwijna*, resonare," "*hwin*, resonans, q. the resounding stone," probably from the resonance emitted on its being struck. It is in all likelihood the same as the word *whine*, and the root is present in both Celtic and Teutonic tongues, *e.g.*:—

Welsh	*Cwyno*, to complain
Irish	*Cuinead*, mourning (?)
Islandic	*hwijna* (as above)
Danish	*hvine*, to whistle
German	*weinen*, to weep

Compare also the Latin *hinnio*, to neigh.

F. DE CHAUMONT

Oakland, Woolston, March 15

WEBSTER, in his Dictionary (9th edition, 1862), says in explaining this word, which is known all over England, that it means *weeds, gorse, furze, waste growth*, from the Welsh *Chwyn*. That it is "a provincial name given to basaltic rock, and applied by miners to any kind of dark coloured and hard unstratified rock which resists the pick."

There is also "*whin-axe*," an instrument for extirpating whin from land.

The Scotch form of whin is *quhyn.*

March 16 JOHN JEREMIAH

The Aurora of February 4

THIS Aurora was seen throughout Europe, including Russia and Constantinople, in Egypt, in the Mauritius, and in India.

May not all auroras pervade the atmosphere around the entire globe and be visible wherever night prevails with a sufficiently clear sky? And so may not the southern and northern aurora belong to one and the same universal aurora?

GEORGE GREENWOOD

Alresford, March 16

I SEE notices in the English papers of a great aurora seen in all parts of Scotland, England, and even as far south as Alexandria in Africa. It may be interesting for your readers to know that it was visible here on the same evening—Sunday, February 4. I saw it first at 6.30 P.M., and at various times after that until 10.30, after which I did not look out of doors. There were no streamers, and the peculiarity of the appearance was that it was in all directions, and less in the north than in the west and east. It presented the appearance of a dull red fog, in shifting masses, and more like the haze I observed here in 1861, when the earth was said to have passed through the tail of the comet of that year. Auroras are very rare in this latitude, but we have had four or five displays in fifteen months: one so bright as to excite the alarm of fire, and to call out the fire department.

GEORGE S. BLACKIE

Nashville, Tennessee, U.S., Feb. 27

Barometric Depressions

BY the introduction of parenthetical sentences between words, which do to some extent represent my meaning, though they are not mine, as the inverted commas would imply, and by the omission of the main point of his own argument, Mr. Ley has presented as mine certain propositions which may well appear to him and to every one who reads them, not only irreconcileable, but sheer nonsense. As these parenthetical interpolations are Mr. Ley's own, and as the point in his argument to which I took exception was not the application of Buys Ballot's Law, but his proposition—shortly stated—that revolving storms are caused by heavy rain, I conceive that his version of my views, which may be funny but is certainly incorrect, is scarcely worth the serious attention of any one.

As to the rest, it is a great thing, in any branch of science, to establish points beyond the reach of further argument or doubt. The depression of the barometer in summer over a great part of Asia has hitherto seemed one of the most curious and difficult problems in Physical Geography. We now know all about it. There is no more room for doubt. It is "really due" to the rarefaction of the air. Mr. Ley says so. What, how, why, when, or where, are details far too commonplace for him to enter upon.

The whole subject of barometric changes, and their relation to strong winds or storms, is one of extreme difficulty; and, in the present state of our knowledge, we can do little more than guess at or discuss the probable solution of the many questions that arise out of it. From the off-hand way in which Mr. Ley disposes of them, or wishes them disposed of, it would appear that he has not yet arrived at even an appreciation of their difficulty. This is the real point on which we are at issue; the range of his study has been too confined. A more general application of his industry will, I hope—should he again meet me in my capacity of critic—relieve me of the necessity of making remarks unpleasant for him to read, or for me to write.

J. K. L.

The Meteor of March 4

I HAVE been looking out for some corresponding notice of a meteor seen here on March 4, but hitherto in vain. At first I hoped that the interesting accounts from Ireland, published in the last number of NATURE, might have referred to the same phenomenon; but I soon found that the dates were discordant, and I now beg to forward the following brief notice of the earlier one:—

On the above-mentioned evening, about 7h. 40m. P.M. railway time, a brilliant meteor was noticed by my gardener Thomas Wood. According to his account it appeared about 20° or 30° above the N. horizon as a ball of red fire passing rapidly from W. to E., about one-third as large as the full moon, with a tail seven or eight times its diameter in length, the portion nearest the head being reddish; but changing at about one-third of its length to green, which was especially distinct towards its tapering point. The head seemed to be surrounded by some sparks. It threw such a light upon the ground as to show all the growing wheat in the field through which the spectator was passing. The course was rather descending, and it went out suddenly without coming down to the horizon. I have heard of only one other person in the neighbourhood who saw the light cast by the meteor, and who described it as extremely brilliant. It is singular that it has not been more generally noticed. The especial interest attached to it is the fact that, in common with the one observed only four days later in Ireland, its course was in the unusual direction of the earth's motion.

Hardwick Vicarage, Hay, March 18 T. W. WEBB

THEODOR GOLDSTÜCKER

FOR the following particulars of the career of the late Prof. Goldstücker we are indebted mainly to the *Academy* and *Trübner's Oriental Record*:—

By the death of Theodor Goldstücker, at the early age of fifty-one, philology has lost one of its greatest scholars, and society, what it can still less afford to lose, one of the noblest and most disinterested of men. Born at Königsberg, in Prussia, he began the study of Sanskrit, for the profound knowledge of which he has since become so famous throughout the world, under Prof. Peter von Bohlen, at the University of that town. He continued this study under Profs. August Wilhelm von Schlegel and Christian Lassen at Bonn. He afterwards resided for some time at Paris, where he enjoyed the friendship of men of the greatest distinction, such as Burnouf, Letronne, &c. He then resided at the University of Berlin, where he began soon to display great scholarly activity. Alexander von Humboldt formed already at that time a very high estimate of the capacities of the young scholar, whose aid, in several very difficult questions of Indian philosophy, he gratefully acknowledged in his "Kosmos."

After the reaction of 1848-9, Goldstücker came over to England for the purpose of assisting Prof. Wilson in the preparation of a new edition of his Sanskrit Dictionary. For this new edition no material whatever existed save the dictionary itself in its printed form. Goldstücker, nevertheless, undertook its revision single-handed; and the immense proportions which under his hand the first six parts assumed (480 pp. without getting to the end of the first letter) rendered the completion of the work by one man or in one generation impossible. Many thousands of notes and references for this and other works, the result of an unremitting study of the MSS. treasures at the India House, &c., are left behind; and we are glad to learn from the *Academy* that the report in some of the newspapers that the deceased had left directions in his will for their destruction is without foundation.

The earliest work undertaken by Goldstücker was the translation into German of the "Prabodha Chandrodaya," a theologico-philosophical drama, by Krischna Miçra, to which Professor Rosenkranz wrote a Preface. In 1861 he published, as an Introduction to a Fac-simile Edition of the "Manava-Kalpa-Sutra," an investigation of some literary and chronological questions, which may be settled by a study of Panini's work, under the title of "Panini, his place in Sanskrit literature." Goldstücker also edited the text of the "Jaiminîya-nyâya-mâlâ-vistara," of which work 400 pages in large quarto are in type.

For the last two years he has been engaged in carrying through the press, for the Indian Government, a photolithographic edition of the "Mahâbhâshya," of which 300 pages still remain to be done. By his decease, what may be called the "traditional" school of Vedic criticism, which gives to the interpretations of native tradition the preference over those derived from comparative philology, ceases to have a European representative. His manuscript of a Sanskrit grammar has long been finished, and it is hoped that this work, which is likely to revolutionise the teaching of Sanskrit in many respects, may be allowed to see the light. The great psychological value as an educational instrument which he attached to the Sanskrit language, if properly taught, was well known to his friends; and it was through his advocacy that a committee of the professors of University College, London, was appointed to report on the desirableness of making Sanskrit an integral part of all the degree examinations in the University of London.

Of the philosophical literature of India, the "Mîmânsa," from its close connection with grammatical researches, engaged his chief attention; some fruit of his labours in this field is a nearly finished edition, prepared for the Sanskrit Society, of Mâdhava's "Jaiminîya-nyâya-mâlâ-vistara" (1865).

It was however Goldstücker's thorough familiarity with the legal and ceremonial literature of the Hindus which rendered his advice of so much value to the Indian Government. A paper recently published by him "On the Deficiencies in the Present Administration of Hindu Law" (Trübner, 1871), contains an exposure of the frequent failures of justice arising from the misunderstandings of native codes, which disgrace our Indian administration.

Besides some papers in the *Reader* and the *Athenæum*, Goldstücker contributed an excellent essay on the "Mahâbhârata" to the *Westminster Review* in April 1868; and among his papers will be found a copy of the great Eastern epic collated with the best European MSS. His library is, we are glad to hear, to be kept together.

Dr. Goldstücker was Professor of Sanskrit in University College, London, President of the Philological Society, a member of the Council of the Asiatic Society and of the Association of the Friends of India.

REPORT OF THE ASSOCIATION FOR THE IMPROVEMENT OF GEOMETRICAL TEACHING

AT the Second Annual Meeting of this Association, held at University College, London, on January 12, Dr. Hirst, the president of the association, delivered the following address:—

In opening the proceedings of this, the Second Annual Meeting of the Association for the Improvement of Geometrical Teaching, I am glad to be able to congratulate you on the decided progress which has been made during the past year towards the realisation of your views. The discussions recorded in English journals, and the reception given to recently published text-books on geometry, unquestionably indicate that public opinion is far more inclined now than it was a few years ago to entertain the notion of an improved exposition of the elements of geometry. We are no longer warned that to touch that edition of Euclid to which, for more than a century, we have paid such literal homage, would be to ruin the teaching of geometry. On the contrary, it is now generally admitted that, without departing from the admirable exactitude and geometrical purity of Euclid's elements, we ought to be able, by judicious revision and extension, to bring them more into harmony with the scientific methods and the habits of thought of our own day. I alluded last year to the retrograde step that had been taken in Italy on this question of the teaching of geometry. The announcement excited much interest in England, though the true purport of the Italian movement was, I fear, slightly misunderstood. I have, therefore, thought it my duty to procure original documents, to make inquiries into the success of the Italian movement of 1867, and also to ascertain the present aspect of geometrical instruction in that country. I hold in my hand the historically interesting document which was issued by the Italian Government in 1871. It contains instructions and programmes relative to the teaching of mathematics in their *Ginnasi* and *Licei*.* Before quoting it I may observe that the *Ginnasio* is essentially a classical school, mathematics being studied only in its fifth or highest class, and then only for five hours a week; and that in the *Liceo* the instruction is still to a great extent classical, though less exclusively so. Here, as the pupil advances through its three classes, mathematics, physics, natural history, and philosophy become more and more prominent as subjects of study. The instructions, as already observed, relate solely to the teaching of mathematics in these classical schools; nevertheless, the following introductory remarks on the objects of mathematical study are, I venture to think, applicable to all schools in which the foundation of a truly liberal education is to be secured: "Mathematics should not be looked upon as a mere collection of intrinsically useful propositions or theorems of which boys ought to acquire a knowledge in order to be able to apply them subsequently to the practical purposes of life. The study should be regarded principally as a means of intellectual culture, directed towards the development of the faculty of reasoning, and to the strengthening of that just and healthy judgment which serves as the light whereby we distinguish truth from that which has but the semblance thereof."

After describing the course of instruction in arithmetic and algebra best suited to the end in view, the document before me proceeds thus:—"In order to give to the instruction in geometry its maximum intellectual efficacy, and at the same time to bring the subject-matter within reasonable limits, it will suffice to follow, in our schools, the example of English ones by returning to the elements of Euclid, universally admitted to be the most perfect model of geometrical rigour." It would be a grave error to suppose that it was the good results on geometrical teaching of our adherence to the elements of Euclid that induced the Italians to return to them. Although England is made, in some measure, responsible for the step taken, we know from sources alluded to in my address last year that the main object in taking it was to purge from Italian schools the many worthless books which private enterprise had succeeded in introducing, and by no other means than the one adopted could the Italian Government, in the opinion of their advisers, have achieved this end with sufficient promptitude and impartiality.

The real motive of the order issued in 1867 is a little more apparent in the following passage from the Instructions, wherein allusion is made to the practice, then prevalent, of striving after a deceptive facility of treatment by the introduction of algebraical processes in place of geometrical reasoning: "The instruction in geometry is to extend to the first six, and to the eleventh and twelfth, books of Euclid, and to be followed by lessons on the most essential propositions of Archimedes relating to the measure of the circle, of the cylinder, of the cone, and of the sphere. Taught by the method of the ancients, geometry is easier and more attractive than the abstract science of number; hence, instead of postponing geometry to algebra, one part of the subject (the first book) is assigned to the fifth class in the *Ginnasio*, and another (the second and third book) to the first class of the *Liceo*. The teacher is recommended to adhere to the method of

* Instruzione e Programmi, per l'Insegnamento della Matematica nei Ginnasi e nei Licei, approvati con R. Decreto, 10 Ottobre, 1867.

Euclid. as the one best fitted to establish in the youthful mind the habit of thoroughly rigorous reasoning; above all, he is not to impair the purity of the ancient geometry by transforming geometrical theorems into algebraic formulæ, that is to say, by substituting in place of concrete magnitude—such as lines, angles, superfices, volumes—their respective measures; on the contrary he is to accustom his pupils to reason always on the magnitudes themselves even when their ratios are under contemplation. It is only after the propositions of Euclid and of Archimedes, mentioned in the programme, have been mastered that formulæ are to be deduced for practically determining the areas of rectilineal figures, the area of the circle, the length of its circumference, and the magnitudes of the surfaces and volumes of prisms, pyramids, cylinders, cones, and spheres."

The measures taken by the Italian Government in 1867 have, I am informed, fully answered the expectations of the mathematicians who recommended them. A taste for rigorous and purely geometrical methods has been revived, and the ground has been cleared for further advances. That such advances were contemplated from the first is obvious from the following passages, with which the Professors Betti and Brioschi—two of the most distinguished mathematicians of Italy—concluded their preface to the new edition (based on that of Viviani) of the Elements of Euclid, with which classical schools were supplied in 1867: "Profoundly convinced that it is only through the eminent qualities of precision and clearness which distinguished Euclid's Geometry that we can hope, in seeking to promote the intellectual development of our youth, to secure those results at which all civilised nations aim when they give to geometrical instruction so important a place in public instruction, we have undertaken the publication of an edition of the elements with the fixed intention of improving it whenever new foreign publications and the experience gained in our own schools shall have shown that improvements are desirable. We trust that professors in *Licei* will help us in this work. We shall gratefully accept their observations and suggestions."

Experience, however, has gone further than was here anticipated; already there appears to be a demand for something beyond a revision of Viviani's edition of Euclid's Elements. In the *Gazzetta Ufficiale* of the kingdom of Italy, published at Rome, I find that on the 2nd of December last an announcement was made by the authority of the Minister of Public Instruction, to the effect that in 1873 a prize of 2,500 lire (about 100*l*.) would be given to the author of the best "Treatise on Elementary Geometry which shall adhere rigorously to the method of Euclid, and contain, besides the subject-matter in the programme of 1867, those portions of the science, developed since Euclid's time, which are now to be found in all elements of geometry adopted as text-books in the classical schools of the most cultured nations." I forbear to attempt to determine what would be the rank of England amongst cultured nations if she were judged by this standard of the introduction of post-Euclidean matter into school text-books. I prefer to see in the announcement merely an encouragement to proceed with our self-imposed task of endeavouring to bring up the teaching of geometry and the text-book we employ to the level of the science of our day. In Italy this can be done more promptly than in England. Our Government cannot, with a stroke of the pen, alter the entire character of the instruction given in English schools. With us improvements are of slower growth, and it is by operations less surgical in their character that obstructions to their growth have first to be removed. It is, in fact, the function of associations like our own to endeavour to remove unreasonable prejudices against changes in the English habit of teaching geometry by bringing prominently forward the defects which we find to exist, and the improvements which we desire to see introduced. Let me now turn, therefore, to the work done by this association during the past year. You will recollect that members were invited to prepare programmes and syllabuses of text-books on geometry in accordance with their own views. The primary object in making this request was to ascertain what amount of unanimity at present prevails amongst teachers. The invitation was accepted by many, and the syllabuses received were referred to two committees, one meeting at London and the other at Rugby. Although the occupations of many of us, and our distances asunder, rendered it very difficult to secure concerted action, a report has at length been prepared, and will be this day submitted to you. With respect to the resolutions and recommendations embodied in this report, I will for the present confine myself to the statement that the main object they are intended to further is a practically useful degree of conformity amongst teachers during the present transitional state of matters. No attempt has been made to prepare any detailed scheme or programme of elementary geometrical study. This last difficult task, however, although postponed, is not, as you will hereafter see, abandoned.

Although the assertion may partake of the character of a truism, it cannot be too often insisted upon, that however necessary it may be to have good text-books, it is far more necessary to have good teachers; that, in fact, good text-books are useful principally by the aid they render in forming good teachers and in furnishing students with an accurate record of what they have been taught. In teaching, one might say, there is *vis viva*—actual energy; whereas in a text-book, however good it may be, the disciplinal energy is at most potential. The text-book, indeed, to be properly used, should always be subordinated to the teaching; but to do this it is absolutely essential that the teacher should, by his own study, have risen not merely up to, but above, the level of the text-book he employs. Until he has so mastered the subject that it has become plastic in his hands, his teaching must necessarily remain defective; for geometrical truth, it must be remembered, has, like all other truth, many sides, and no text-book can present all, or necessarily the one which, to individual pupils, is the most accessible. Alternative methods of demonstration, inquiries into the interdependence of propositions, judicious variation of data, and just discrimination between the contingent and necessary properties of figures; these and numerous other matters, all essential to geometrical culture, can only be properly supplied by the teacher; no text-book could be weighted with them. Above all, it is to him that we must mainly look for the cultivation of that scientific method of inquiry under whose guidance solely problem-solving can be raised in character above what has been termed "exalted conundrum guessing," and acquire its full educational value.

The interdependence of geometrical propositions above alluded to, as one of the subjects to which teachers should habitually direct the attention of their pupils, is mainly logical in character, but nevertheless most essential to geometrical culture. Every one will admit the primary importance of habituating the student to extract its full logical significance from every proposition he establishes, to recognise each proposition readily under different, although logically equivalent forms of enunciation, and thus to discriminate accurately between the cases where mere *logical* deduction from antecedent propositions is requisite, from those which require the introduction of further *geometrical* considerations. Obvious as this may be, it is rarely sufficiently attended to by teachers, and even in approved text-books, ancient as well as modern, we not unfrequently find remarkable instances of the absence of the discrimination to which I refer. The ninth proposition of the third book of Euclid is now a well-known case of the kind. Geometrical apparatus is there employed to demonstrate, indirectly, what had virtually been already proved in the seventh proposition. Having proved that *from a point which is not the centre three equal straight lines cannot be drawn to the circumference of a circle* (Prop. 7), it was wholly unnecessary to prove that *the point from which three equal straight lines can be drawn to the circumference must be the centre of the circle* (Prop. 9).

The two theorems are, in fact, contra-positive forms, one of the other; the truth of each is implied, when that of the other is asserted, and to demonstrate both geometrically is more than superfluous; it is a mistake, since the true relation between the two is thereby masked. There can be no better proof of this than the fact that the above defect in exposition remained undetected for centuries. Another, though less striking, example of the same kind is presented by the 16th and 27th propositions of the first book. Few intelligent boys fail on first reading the 27th to note the oddity of giving to two parallel lines a dagger-like shape in order to prove indirectly that "if a straight line falling on two other straight lines make the alternate angles equal to each other, these two straight lines shall be parallel." It is certain, however, that few of them ever discover that the proposition has virtually been proved before, that it is in fact the contra-positive form of the 16th, since the latter is obviously susceptible of being thus enunciated: "If two straight lines meet one another, a straight line falling on them will not make the alternate angles equal."

The late Prof. de Morgan, to whose keen penetration we owe the detection, not merely of the above defects in Euclid, but of many others, strongly and justly insisted upon the necessity of a more logical study of the elements of geometry.

I do not advocate the introduction of more *formal* logic into elementary geometry, but simply the cultivation of a logically severer habit of thought, and the more frequent application of those simple rules of reasoning by means of which tedious reiteration may be so often obviated, and, as a consequence, clearness of insight promoted. As an instance of such a rule I may mention that very useful one according to which "the converse of each of a series of demonstrated theorems is necessarily true if of their several hypotheses, as well as of their predicates, it can be said that one must be true, and that no two of them can be so at the same time." A conviction of the general validity of this rule is readily imparted, even to your pupils, by first selecting familiar instances and then generalising; and, once imparted, they are put in possession of the instrument whereby converse propositions in geometry are most frequently and satisfactorily established.

In conclusion, I may observe that it is chiefly by the aid of general rules, such as those just alluded to, that the mechanical details of demonstration become sufficiently subordinated to allow a complete grasp of the whole subject to be acquired; they serve, in fact, as the thread on which the isolated propositions of geometry, like beads, have to be strung before they can be properly viewed.

THE YELLOWSTONE PARK

THE following, reprinted from the "Reports to Congress" of the United States, will serve to show the zeal displayed by the American Government for the improvement of the people. We regret that we are unable to reproduce the accompanying maps:—

"The Bill now before Congress has for its object the withdrawal from settlement, occupancy, or sale, under the laws of the United States, a tract of land fifty-five by sixty-five miles, about the sources of the Yellowstone and Missouri Rivers; and dedicates and sets it apart as a great national park or pleasure-ground for the benefit and enjoyment of the people. The entire area comprised within the limits of the reservation contemplated in this Bill is not susceptible of cultivation with any degree of certainty, and the winters would be too severe for stock-raising. Whenever the altitude of the mountain districts exceed 6,000ft. above tide-water, their settlement becomes problematical unless there are valuable mines to attract people. The entire area within the limits of the proposed reservation is over 6,000ft. in altitude, and the Yellowstone Lake, which occupies an area 15 miles by 22 miles, or 330 square miles, is 7,427ft. The ranges of mountains that hem the valleys in on every side rise to the height of 10,000ft. and 12,000ft, and are covered with snow all the year. These mountains are all of volcanic origin, and it is not probable that any mines or minerals of value will ever be found there. During the months of June, July, and August, the climate is pure and most invigorating, with scarcely any rain or storms of any kind; but the thermometer frequently sinks as low as 26°. There is frost every month of the year. This whole region was in comparatively modern geological times the scene of the most wonderful volcanic activity of any portion of our country. The hot springs and the geysers represent the last stages—the vents or escape-pipes—of these remarkable volcanic manifestations of the internal forces. All these springs are adorned with decorations more beautiful than human art ever conceived, and which have required thousands of years for the cunning hand of nature to form. Persons are now waiting for the spring to open to enter in and take possession of these remarkable curiosities, to make merchandise of these beautiful specimens, to fence in those rare wonders so as to charge visitors a fee, as is now done at Niagara Falls, for the sight of that which ought to be as free as the air or water.

"In a few years this region will be a place of resort for all classes of people from all portions of the world. The geysers of Iceland, which have been objects of interest for the scientific men and travellers of the entire world, sink into insignificance in comparison with the hot springs of the Yellowstone and Fire-Hole Basins. As a place of resort for invalids it will not be excelled by any portion of the world. If this Bill fails to become a law this session, the Vandals who are now waiting to enter into this wonderland will, in a single season, despoil beyond recovery these remarkable curiosities which have required all the cunning skill of nature thousands of years to prepare.

"We have already shown that no portion of this tract can ever be made available for agricultural or mining purposes. Even if the altitude and the climate would permit the country to be made available, not over fifty square miles of the entire area could be settled. The valleys are all narrow, hemmed in by high volcanic mountains like gigantic walls.

"The withdrawal of this tract, therefore, from sale or settlement takes nothing from the value of the public domain, and is no pecuniary loss to the Government, but will be regarded by the entire civilised world as a step of progress and an honour to Congress and the nation.

Department of the Interior,
Washington, D. C., January 29, 1872

Sir,—I have the honour to acknowledge the receipt of your communication of the 27th instant relative to the Bill now pending in the House of Representatives dedicating that tract of country known as the Yellowstone Valley as a national park.

I hand you herewith the report of Dr. F. V. Hayden, United States geologist, relative to said proposed reservation, and have only to add that I fully concur in his recommendations, and trust that the Bill referred to may speedily become a law.

Very respectfully, your obedient servant,
C. DELANO, Secretary.

Hon. M. H. Dunnell, House of Representatives.

"The committee therefore recommend the passage of the bill without amendment."

DR. LIEBREICH ON TURNER AND MULREADY

DR. R. LIEBREICH, in a lecture delivered at the Royal Institution on Friday, the 8th inst., "On the effects of certain faults of vision on painting, with special reference to Turner and Mulready," successfully vindicated the title of physical science to extend its researches into the domain of art criticism by applying optical laws to painting. The lecture may be said to consist of three parts, the first of which demonstrates, by the example of Turner, that there are certain conditions of the eye which alter the appearance of nature, whilst they leave the impression a picture produces upon the eye unchanged. The second part of the lecture proves, by the example of a French artist yet living, whose name, therefore, was withheld, that there is another defect of the eye, which produces an incorrect impression of the picture as well as of nature, the error, however, being dissimilar, and affecting the picture and nature in opposite ways. The third part of the lecture shows, by the example of Mulready, that there is yet another disease of the eye affecting colours in such a manner that pigments used in painting are influenced by the disease, whilst natural colours continue unaltered.

I.—Turner

Surprised at the great difference between Turner's earlier and later works, said the lecturer, he examined one of the great artist's later pictures from a purely scientific point of view, and analysed it with regard to colour, drawing, and distribution of light and shade.

It was particularly important to ascertain if the anomaly of the whole picture could be deduced from a regularly recurring fault in its details. This fault is a vertical streakiness, which is caused by every illuminated point having been changed into a vertical line. The elongation is, generally speaking, in exact proportion to the brightness of the light; that is to say, the more intense the light which diffuses itself from the illuminated point in nature, the longer becomes the line which represents it on the picture. Thus, for instance, there proceeds from the sun in the centre of a picture a vertical yellow streak, dividing it into two entirely distinct halves, which are not connected by any horizontal line. In Turner's earlier pictures the disc of the sun is clearly defined, the light equally radiating to all parts; and even where, through the reflection of water, a vertical streak is produced, there appears, distinctly marked through the vertical streak of light, the line of the horizon, the demarcation of the land in the foreground, and the outline of the waves in a horizontal direction. In the pictures, however, of which I am now speaking (the lecturer proceeded to say), the tracing of any detail is perfectly effaced when it falls in the vertical streak of light. Even less illuminated objects, like houses or figures, form considerably elongated streaks of light. In this manner, therefore, houses that stand near the water, or people in a boat, blend so entirely with the reflection in the water, that the horizontal line of demarcation between house and water or boat and water entirely disappears, and all becomes a conglomeration of vertical lines. Everything that is abnormal in the shape of objects, in the drawing, and even in the colouring of the pictures of this period, can be explained by this vertical diffusion of light.

How and at what time did this anomaly develop itself?

Till the year 1830 all is normal. In 1831 a change in the colouring becomes for the first time perceptible, which gives to the works of Turner a peculiar character not found in any other master. Optically this is caused by an increased intensity of the diffused light proceeding from the most illuminated parts of the landscape. This light forms a haze of a bluish colour which contrasts too much with the surrounding portion in shadow. From the year 1833 this diffusion of light becomes more and more vertical. It gradually increases during the following years. At first it can only be perceived by a careful examination of the picture; but from the year 1839 the regular vertical streaks become apparent to every one. This increases subsequently to such a degree, that when the pictures are closely examined they appear as if they had been wilfully destroyed by vertical strokes of the brush before they were dry, and it is only from a considerable distance that the object and meaning of the picture can be comprehended. During the last years of Turner's life this peculiarity became so extreme that his pictures can hardly be understood at all.

It is a generally received opinion that Turner adopted a peculiar manner, that he exaggerated it more and more, and that his last works are the result of a deranged intellect. I am convinced of the incorrectness, I might almost say of the injustice, of this opinion. According to my idea, Turner's manner is exclusively the result of a change in his *eyes*, which developed itself during the last twenty years of his life. In consequence of it the aspect of nature gradually changed for him, while he continued in an unconscious, I might almost say in a naïve manner, to reproduce what he saw. And he reproduced it so faithfully and accurately, that he enables us distinctly to recognise the nature of the disease of his eyes, to follow its development step by step, and to prove by an optical contrivance the correctness of our diagnosis. By the aid of this contrivance we can see nature under the same aspect as he saw and represented it. With the same we can also, as I shall prove to you by an experiment, give to Turner's early pictures the appearance of those of the later period.

After he had reached the age of fifty-five, the crystalline lenses of Turner's eyes became rather dim, and dispersed the light more strongly, and in consequence threw a bluish mist over illuminated objects. In the years that followed, as often happens in such cases, a clearly defined opacity was formed in the slight and diffuse dimness of the crystalline lens. In consequence of this the light was no longer evenly diffused in all directions, but principally dispersed in a vertical direction. At this period the alteration offers, in the case of a painter, the peculiarity that it only affects the appearance of natural objects, where the light is strong enough to produce this disturbing effect, whilst the light of his painting is too feeble to do so: therefore, the aspect of nature is altered, that of his picture correct.

The lecturer proceeded to demonstrate the truth of his remarks by a series of experiments, which showed, for instance, a natural tree, and then, by means of lenses prepared for the purpose, changed it into a "Turner-tree;" likewise the artist's early picture of "Venice" was shown, and, by means of lenses, changed into the "Venice" of Turner's later period.

II.—Astigmatism

The optical state of the eye during its adaptation for the farthest point, when every effort of accommodation is completely suspended, is called its refraction.

There are three different kinds of refraction: firstly, that of the normal eye; secondly, of the short-sighted eye; thirdly, of the over-sighted eye.

1. The normal eye, when the activity of its accommodation is perfectly suspended, is adjusted for the infinite distance; that is to say, it unites upon the retina parallel rays of light. (Fig. 1.)

2. The short-sighted eye has in consequence of an extension of its axis a stronger refraction, and unites, therefore, in front of the retina the rays of light which proceed from infinite distance. In order to be united upon the retina itself the rays of light must be divergent, that is to say, they must come from a nearer point. The more short-sighted the eye is, the stronger must be the divergency; such an eye, in order to see distinctly distant objects, must make the rays from a distant object more divergent, by aid of a concave glass. (Fig. 2.)

3. The over-sighted (hypermetropic) eye, on the contrary, has too weak a refraction ; it unites convergent rays of light upon the retina ; parallel or divergent rays of light it unites behind the retina, unless an effort of accommodation is made. (Fig. 3.)

Hypermetropy, the lecturer explained, does not essentially influence painting, and is easily corrected by convex glasses. Short-sightedness, on the contrary, generally influences the choice of subject as well as its manner of execution.

Sometimes the shape of the eye diverges from its normal spherical form, and this is called astigmatism. This has only been closely investigated since Airy discovered it in his own eye. Figure to yourself meridians drawn on the eye as on a globe, so that one pole is placed in front ; then you can define astigmatism as a difference in the curvature of two meridians, which may, for instance, stand perpendicularly upon each other ; the consequence of which is a difference in the power of refraction of the eye in the direction of the two meridians. An eye may, for instance, have a normal refraction in its horizontal meridian, and be short-sighted in its vertical meridian. Small differences of this kind are found in almost every eye, but are not perceived. Higher degrees of astigmatism, which decidedly disturb vision, are, however, not uncommon, and are therefore also found among painters.

I observed a very curious influence of astigmatism upon the works of a portrait painter. He was held in high esteem in Paris, on account of his excellent grasp of character and intellectual individuality. His admirers considered even the material resemblance of his portraits as perfect ; most people, however, thought he had intentionally neglected the material likeness by rendering in an indistinct and vague manner the details of the features and the forms. A careful analysis of the picture shows that this indistinctness was not at all intentional, but simply the consequence of astigmatism. Within the last few years the portraits of this painter have become considerably worse, because the former indistinctness has grown into positively false proportions. The neck and oval of the face appear in all his portraits considerably elongated, and all details are in the same manner distorted. What is the cause of this? Has the degree of his astigmatism increased ? No ; this does not often happen ; but the effect of astigmatism has doubled, and this has happened in the following manner :—An eye which is normal as regards the vision of vertical lines, but short-sighted for horizontal lines, sees the objects elongated in a vertical direction. When the time of life arrives that the normal eye becomes far-sighted, but not yet the short-sighted eye, this astigmatic eye will at short distance see the vertical lines indistinctly, but horizontal lines still distinctly, and therefore near objects elongated in a horizontal direction. The portrait painter, in whom a slight degree of astigmatism manifested itself at first only by the indistinctness of the horizontal lines, has now become far-sighted for vertical lines, therefore he sees a distant person elongated in a vertical direction ; the portrait he paints, on the contrary, being at a short distance, is seen enlarged in a horizontal direction, and thus painted still more elongated than the subject is seen ; so the fault is doubled.

The lecturer proved these remarks by showing a picture which he made to appear in its natural shape or distorted by elongation, in either a vertical or a horizontal direction, by means of a lens which he held at various distances from the optical apparatus.

III.—Mulready

The lens, continued the lecturer, always gets rather yellow at an advanced age, and with many people the intensity of the discoloration is considerable. This, however does not essentially diminish the power of vision. In order to get a distinct idea of the effect of this discoloration, it is best to make experiments with yellow glasses of the corresponding shade. Only the experiment must be continued for some time, because at first everything looks yellow to us. But the eye soon gets accustomed to the colour, or rather it becomes dulled with regard to it, and then things appear again in their true light and colour. This is at least the case with all objects of a somewhat bright and deep colour. A more careful examination, however, shows that a pale blue, or rather a certain small quantity of blue, cannot be perceived even after a very prolonged experiment, and after the eye has long got accustomed to the yellow colour, because the yellow glass really excludes it. This must, of course, exercise a considerable influence when looking at pictures, on account of the great difference which necessarily exists between real objects and their representation in pictures.

These differences are many and great, as has been so thoroughly explained by Helmholtz. Let us for a moment waive the consideration of the difference produced by transmitting an object seen as a body upon a simple flat surface, and let us only consider the intensity of light and colour. The intensity of light proceeding from the sun and reflected by objects is so infinitely greater than the strongest light reflected from a picture, that the proportion expressed in numbers is far beyond our comprehension. There is also a great difference between the colour of light or of an illuminated object, and the pigments employed in painting, and it must appear wonderful that the art of painting can produce by the use of them such perfect optical delusions. It can, of course, only produce optical delusions, never a real optical identity ; that is to say, the image which is traced in our eye by real objects is not identical with the image produced in our eye by the picture.

Returning to our experiment with the yellow glass, we shall find that it affects our eye very much in the same way as a yellow tint of light. The small quantity of blue light which is excluded by the yellow glass produces no sensible difference, as the difference is equalised by a diminution of sensibility with regard to yellow. In the picture, on the contrary, there is found in many places only as much blue as is perfectly absorbed by the yellow glass, and this therefore can never be perceived, however long we continue the experiment. Even for those parts of the picture which have been painted with the most intense blue the painter could produce, the quantity of blue excluded by the yellow glass will make itself felt, because its power is not so small with regard to pigments as with regard to the blue in nature.

With aged people we often find the crystalline lens to be of a yellowish tint. In pictures painted after the artists were over sixty, therefore, the effect of the yellow lens can often be studied. As a striking example, the lecturer mentioned Mulready. It is generally stated that in his advanced age he painted too purple. A more careful examination shows, however, that the peculiarity of the colours of his later pictures is produced by an addition of blue. Thus, for instance, the shadows on the flesh are painted in pure ultramarine. Blue drapery he painted most unnaturally blue. Red of course became purple. If we look at these pictures through a yellow glass all these faults disappear ;—what formerly appeared unnatural and displeasing is at once corrected ; the violet colour of the face shows a natural red ; the blue shades become grey ; the unnatural glaring blue of the drapery is softened. It happens that Mulready has painted the same subject twice, first in the year 1836, when he was fifty years of age and his lens was in a normal state, and again in 1857, when he was seventy-one and the yellow discolouring had already considerably advanced. The first picture was called when exhibited "Brother and Sister ; or, Pinching the Ear ;" the second was called "The Young Brother." If we look at the second picture through a yellow glass, the difference between the two almost entirely disappears, as the glass corrects the faults of the picture. The smock of the boy no more appears of that intense blue which we may see in a lady's silk dress, but never in the linen smock of a peasant. It changes into the natural tint we find in the first picture. The purple face of the boy

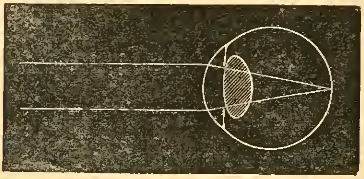


FIG. 1

also becomes of a natural colour. The shades on the neck of the girl and the arms of the child, which are painted in a pure blue, look now grey, and so do the blue shadows in the clouds. The grey trunk of the tree becomes brown. Surprising is the effect upon the yellowish green foliage, which, instead of appearing still

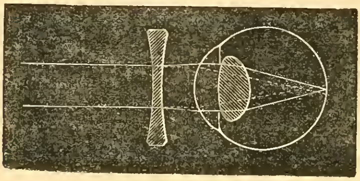


FIG. 2

more yellow, is restored to its natural colour, and it shows now the same tone of colour as the foliage in the earlier picture. This last fact is most important to prove the correctness of my supposition. The endeavour to explain this fact became for me the starting-point of a series of investigations to ascertain the optical qualities of the pig-

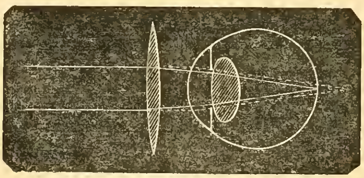


FIG. 3

ments used in painting, and thus to enable us to recognise them by optical contrivances when the vision of the naked eye does not suffice to analyse the colours of a picture.

If it is the dispersion of light which, as in Turner's case, alters the perception of nature, it can be partly rectified by a kind of diaphragm with a small opening (Donder's sthenopeical spectacles).

In cases of astigmatism, the use of cylindrical glasses will completely correct the aspect of nature, as well as of the picture. Certain anomalies in the sensation of colour may also be counteracted to some extent by the use of coloured glasses; for instance, by a blue glass, when the lens has become yellow, as was the case with Mulready.

If science aims at proving that certain works of art offend against physiological laws, artists and art critics ought not to think that, by being subjected to the material analysis of physiological investigation, that which is noble, beautiful, and purely intellectual would be dragged into the dust. They ought, on the contrary, to make the results of these investigations their own. In this way art critics will often obtain an explanation of the development of the artist, and artists will avoid the inward struggles and disappointments which often arise through the difference between their own perceptions and that of the majority of the public. Never will science be an impediment to creations of genius.

Dr. Liebreich's lecture will appear *in extenso* in the April number of *Macmillan's Magazine*.

THE NATURAL HISTORY OF EASTERN THIBET

DR. CAMPBELL, Superintendent of Darjeeling, has recently published a series of valuable papers on Eastern Thibet in *The Phœnix*, a monthly magazine for China, Japan, and Eastern Asia, ably edited by the Rev. James Summers, Professor of the Chinese Language in King's College. As a journal of this kind must naturally have only a limited circulation, and is not likely to be in the hands of many of our readers, we have no hesitation in abstracting from Dr. Campbell's contributions the following notes on the Zoology and Mineralogy of a country that at the present time is of special interest, both in a geographical and a commercial point of view. The following is a list of the animals of Eastern Thibet, the native name being attached to each :—*Goa*, an antelope; *Gnow*, the *Ovis ammon; Rigong*, the hare; *Kiang*, the wild ass; *Lawa*, the musk-deer; *Shaoo*, a large deer, *Cervus affinis; Cheu*, *Antelope Hodgsoni; Dong*, the wild yak of Thibet; *Pegoo*, the yak; A small cow, whose native name is not given; *Sauh*, cross between cow and yak; *Ba Sauh*, produce of female yak by bull; *Look*, sheep; *Peu Ra*, Thibet goat; *Phák*, the pig; *Cha*, the common fowl; *Damjhar*, the duck; *Damjhar Cheemoo*, the goose (besides the duck and goose there are numerous wild fowls, swimmers and waders, which migrate from India in March, and return in October); *Chungoo*, a reddish wild dog; *Koong*, a mottled civet; *Sik*, the leopard; *Tagh*, the tiger; *Somb*, the bear (a red and a black species); *Nehornehu*, a large sheep, goat, or antelope of various colours, four feet high, with enormous horns four feet long, sloping backwards, and a tail fifteen inches in length.

This completes Dr. Campbell's list of the indigenous mammals and birds. With regard to the *Dong* or wild yak of Thibet, he observes that it is the fiercest of all known ruminants, and will rarely allow a man to escape alive if it can come up with him. It is generally hunted on horseback, the great aim being to detach one from the herd. The horns of the full-grown buck are said to be three feet long, and the circumference must be enormous. They are used by the Grandees at marriage and other feasts as gigantic drinking cups, and handed round to the company. The horns so used are finely polished, and mounted in silver or gold and precious stones. A stuffed "Dong" is common in Thibetan Lamaserais, standing in front of the image of Mahákkáli, at whose shrine the animal is thus figuratively sacrificed.

Of *Look* or sheep there are four principal varieties—1st, Chang Look or northern sheep, very large, with fine wool; flocks of from 400 to 1,000 tended by one man. 2nd. Sok Look, rare, but greatly praised; it is a heavy-tailed sheep, coming from the province of Sok, east of Lassa; wool not very fine. 3rd. Lho Look, a very small sheep indeed, generally white but sometimes black, bred about Lassa; wool very fine and like the shawl wool. 4th. Changumpo Look, abundant about Geroo and in Dingcham, generally very large; the white wool very fine and soft. The flesh of all these sheep is fine-grained and good.

Of the *Phák* or pig there are two varieties, the southern pig, which is similar to the Indian village pig, and the small Chinese pig. There are no wild hogs in Thibet. The Chinese butchers at Lassa blow their pork so as to give it a deceptively fine appearance.

Ducks and geese are not eaten by the Thibetans, but are greatly used by the Chinese, for whom they are specially bred in Lassa.

The lakes of Thibet are full of fish, of which only one kind, named *Choolap*, is described; it grows to the weight of 8lb., and is a coarse food. It is, however, caught and preserved largely; the fish being gutted, split up, the tail put in the mouth, and dried, without salt, in the open air. Thus prepared they will keep for a year. The mode of catching them is singular; when the lakes are frozen over, a hole is made in the ice, to which they rush in such abundance that they are pulled out by the hand.

There are no leeches or mosquitoes in Thibet, nor are maggots or fleas ever seen there; and in Dingcham or Thibet Proper there are no bees or wasps.

Dr. Campbell gives us some very interesting information regarding the food of the Thibetans. During the summer months they use very little fresh meat. They do not like it boiled, and are not partial to it raw, unless it has been dried. In November there is a great slaughter, and a wealthy man, who has perhaps 7,000 sheep, will kill 200 at this time for his year's consumption. The animal after being killed is skinned and gutted and then placed on its feet in a free current of air. In a couple of days it becomes quite hard and is then ready for eating. It is kept in this way for more than a year without spoiling, even during the rainy periods. When long exposed to the wind of Thibet it becomes so dry that it may be rolled into powder between the hands. In this state it is mixed with water and drunk, and used in various other ways. The Thibetans eat animal food in endless forms, and a large portion of the people live on nothing else. The livers of sheep and other animals are similarly dried or frozen, and are much prized, but to strangers they are very distasteful for their bitterness and hardness. The fat is dried, packed in the stomachs, and then sent to market or kept for home use.

With regard to edible vegetables, it is stated that wheat, barley, and buckwheat sown in April or May and irrigated, are reaped in September, barley in Thibet taking the place of potatoes in Ireland, four-fifths of the population living on it. Besides these, the other crops are composed of peas, turnips, and a little mustard. The grain is ground in water mills. The bread is all unleavened, and cooked on heated stoves or gridirons. The sweet pure farinaceous taste of the fine flour equals the best American produce. The staple food of the country is *champa*, called *suttoo* in India; it is finely-ground flour of toasted barley. It is much eaten without further cooking; mixed up with hot tea it is called *paak*, and when prepared with tepid water it is known as *seu*. If any of our readers wish to enter upon "pastures new" in the breakfast department, they may try *Tookpa*, which, to be properly appreciated, should be taken at daybreak before any matutinal ablutions. It is a sort of broth made with mutton, champa, dry curds, butter, salt, and turnips.

Goats are also reared in considerable flocks, but for their milk rather than their flesh. The milk of yaks, cows, sheep, and goats is used alike for making dried curds and the various preparations of milk used by these people. Mares' milk is not used in Eastern Thibet.

We now proceed to notice the mineral wealth of this remarkable country.

Pen, a carbonate of soda, is abundant south of the Yaroo; it appears in a whitish powder on the soil, never in masses underground. It is not used for soap-making or otherwise in the arts, but is always put into the water when tea is made, and is much employed medicinally.

Chulla, borax, is only obtained north of the Yaroo, whence it is imported to other parts of Thibet, to India, *viâ* Nepaul, Sikkim, and Bootana, and thence to Calcutta and Europe.

Sicha, saltpetre, is abundantly manufactured in the Cara Thibetan sheep-folds, where composts of sheep's dung and earth are found to produce it.

Lencha, common salt, occurs in commerce in three forms, viz.: *Sercha*, white and best; *Cháma*, reddish and good; and *Pencha*, yellowish and bad, containing soda or magnesia and earthy matter. All the salt used in Eastern Thibet is the produce of the lakes and mines north of the Garoo, or comes from Lache, a district between Digarchi and Ladak. According to the best information, all the salt is the produce of lakes, while some assert that it is dug out of the earth. It is certain that the salt-producing districts are all but inaccessible, and can only be traversed by men and sheep; and that their elevation prevents the working from being carried on except in the warmer part of the year, from April to November. Thousands of sheep are employed in carrying the salt to places accessible to yaks, the former animals carrying a load of 20lb. to 24lb. on open places, or of 8lb. to 10lb. in the rugged vicinity of the deposits, whose elevation is not less than 22,000 feet, while the latter are capable of bearing a load of 160lb.

Ser, gold, is found in the sands of a feeder of the Garoo, on its northern side, but the name of the river could not be ascertained by Dr. Campbell. The Garoo itself does not yield any gold washings. Most of the gold of Thibet is the produce of mines or diggings.*

Pabea, the yellow arsenic of commerce, is found west of Lassa, near the borders of China.

There are no mines of iron, silver, copper, quicksilver, lead, or coal in Thibet; the latter substance is, however, imported from China.

The turquoise, real or artificial, is universally worn in rings, necklaces, &c., and large, amber-like beads are a favourite ornament; but it is uncertain whether they are natural products of Thibet. The latter are apparently composed of turpentine mixed with some hardening material. Numerous imitations of turquoise are imported from China; and real but not valuable stones are sent, *viâ* Cashmere (but from what locality is not stated). The only test of a real stone that is resorted to by the Thibetans is to make a fowl swallow it; if real it will pass through unchanged.

In conclusion, we may add that Dr. Campbell's articles in *The Phœnix* contain much valuable matter on the geography, the government, and army of Thibet, the personal habits, customs, and ceremonies of the Thibetans, their religious festivals, the seasons, soil, and agriculture of the country, the wages of labour, and the most prevalent diseases. Amongst "Things not generally known," we may mention *Goomtook*, or *The laughing disease*, which consists of violent fits of laughter with excruciating pain in the throat. It equally attacks men and women, and often proves fatal in a few days.

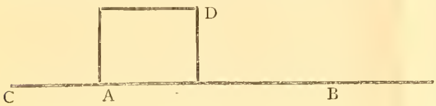
ON THE CAUSE OF FIXED BAROMETRIC VARIATIONS

THE chief difficulty in the way of explaining the annual and diurnal variations of the barometer by the heating and cooling of the air, appears to be the existence of a double maximum and minimum. To show how such a double maximum and minimum might result from the

* Notices of the Thibetan Gold Mines may be found in several recent numbers of the "Proceedings of the Royal Geographical Society."

changes in the temperature of the dry air alone, is the object of the present paper. I commence with the diurnal variation.

Let us suppose an atmosphere of dry air hardly absorbing any heat from the solar rays, and therefore chiefly heated and cooled by contact with the earth. Let us take the moment when the earth first begins to be heated by the sun's rays. (This will probably take place a little before sunrise, in consequence of the large amount of reflected or diffused heat which accompanies the morning twilight.) The earth then becomes heated at A, while at B, a little more to the west, no heat is yet felt. The earth



at A communicates its heat to the air in contact with it, and the latter expands and becomes lighter than the air in contact with the earth at B. (At C of course the earth is more highly heated than at A, and therefore the air in contact with the earth at C is still lighter.) The immediate consequence is that the heavier air at B rushes into the heated space A D (see fig.), driving out the lighter air which occupies it; and A D becoming filled with heavier air than before, the barometer at A rises. The heating goes on however at A, which remains at a higher temperature than B, until the epoch of greatest heat arrives; and consequently during all this time there is a flow of air from B towards A next the earth, with a flow in the contrary direction at a greater elevation. It might at first sight appear that the barometer at A would go on rising all this time. But a moment's reflection will show us that though it does so at first, it could not continue to do so all through. For as at the epoch of greatest cold (with which we commenced) C, A, and B were sensibly at the same temperature, so they will arrive at sensibly the same temperature at the epoch of greatest heat; and immediately afterwards the direction of the under-current will be reversed, C having become colder than A, while B is hotter. It is therefore evident that during the whole time which has elapsed between the epochs of greatest cold and greatest heat, the two currents will have counter-balanced each other, the under-current having carried exactly as much air from B to A as the upper-current has carried from A to B. Making a somewhat rough approximation, we may assume that during the first half of this period the under-current has been in excess, and the barometer at A has risen, while in the latter half the upper-current has been in excess, and the barometer at A has been falling. Immediately after the epoch of greatest heat, the cooler and heavier air at C will displace the air in the space A D, causing the barometer at A to rise. The moment of greatest heat will, therefore, correspond to a minimum reading of the barometer, not a maximum; and after it the barometer will go on rising until half way between it and the moment of greatest cold, when it will again fall until the latter moment. The barometer will, therefore, attain its minimum height at the hours of greatest heat and greatest cold, while the maximum heights will occur at about halfway between these epochs. Now this result appears to conform exactly to observation. It must be recollected that the minimum of temperature occurs not more than half an hour before sunrise, while the maximum is generally not reached for two or three hours after noon. This will explain why the morning barometric maximum seems to be nearly an hour earlier than the evening one. Indeed observation corresponds so exactly with the results arrived at, that I think it will appear that they cannot be seriously modified by the presence of aqueous vapour.

The mean of barometric pressures at different latitudes confirms these results. If the trade-winds extended to the poles—which they probably would do were it not that the parallels of latitude become so narrow before reaching them—on the same principles we might expect a minimum of pressure at the equator and the poles with a maximum at a latitude of about 45°. For the second of these minima we must evidently substitute the limit of the trades, or rather perhaps of the anti-trades, since the latter seem ultimately to become the under-currents; and our maximum will be situated about halfway between this limit and the equator. This agrees with observation. The phenomena of the tides too are analogous. There is low water where the moon's attraction is strongest and where it is feeblest, while high water corresponds to the mean attraction. Putting heat for attraction and the sun for the moon, the diurnal variations of the barometer follow the same law.

This law, however, does not appear to hold so well for the annual barometric changes. We can hardly trace in this case a double maximum in May and November, with minima in January and July. I think, however, that this result may be in part at least explained by the northern and southern shifting of the system of trades and anti-trades. For example, if a place in the northern hemisphere be near this limit (which corresponds to a minimum), the southern movement of the system in winter may cause the barometer to rise instead of falling as we approach the coldest day (supposing of course that it lies to the north of it). On the other hand, at a locality a little to the south of the limit, the northern movement of the system in summer may cause the barometer to rise at the time of greatest heat. I should perhaps notice, however, that the results here arrived at suppose the three points A, B, C to be situated on a horizontal plane, and the specific heat and conductibility of the earth at each of these points to be nearly identical. Hence they cannot be expected to hold for very elevated positions, or for places situated on the sea coast, or the shores of a large lake. They will be found most accurate in the interior of continents, where the land is level, and where the amount of aqueous vapour in the air is comparatively small. This anticipation is also verified by observation, so far as my knowledge reaches.

W. H. S. MONCK

*REMARKS ON THE ADAPTIVE COLOURATION OF MOLLUSCA**

NATURALISTS have long recognised the curious cases oftentimes occurring, of the resemblance between the colour of an animal and its immediate surroundings. It had been supposed that climatic influences, or peculiarities of food, or greater or less access to light, had something to do with these coincidences. Mr. Alfred R. Wallace has shown that the varied phases of these phenomena could not be explained by such agents, and in a paper "On Mimicry and other protective resemblances among Animals," published in the *Westminster Review*, July 1867, and since made widely public in his work on "Natural Selection," he shows that the singular resemblances between the colour of animals and their surroundings are mainly brought about by the protection afforded them through greater concealment. Many very interesting examples are then cited from the Vertebrates and Articulates in support of these views. Briefly may be mentioned, as examples, the almost universal sand colour of those animals inhabiting desert tracts; the white colour of those animals living amid perpetual snows; the resemblance seen again and again between the colour of many insects and the places they frequent. Among the hosts of examples cited by Mr. Wallace as illustrating plainly the views he advances, may be mentioned the

* From the Proceedings of the Boston Society of Natural History, vol. xiv., April 5, 1871.

many species of Cicindela, or tiger beetle. The common English species, "*C. campestris*, frequents grassy banks, and is of a beautiful green colour, while *C. maritima*, which is found only on sandy sea shores, is of a pale bronzy yellow, so as to be almost invisible." He then states that a great number of species found by himself in the Malay Archipelago were similarly protected. "The beautiful *Cicindela gloriosa*, of a deep velvety green colour, was only taken upon wet mossy stones in the bed of a mountain stream, where it was with difficulty detected. A large brown species (*C. heros*) was found chiefly on dead leaves in forest paths; and one which was never seen except on the wet mud of salt marshes, was of a glossy olive so exactly the colour of the mud as only to be distinguished when the sun shone, by its shadow. Where the sand beach was coralline and nearly white, I found a very pale *Cicindela*; wherever it was volcanic and black, a dark species of the same genus was sure to be met witb."

But little attention has been given to adaptive colouring among the lower invertebrate animals. Darwin, in his last work on the "Descent of Man," calls attention to the statements of Haeckel that the transparency of the Medusæ and other floating animals is protective, since their glass-like appearance renders them invisible to their enemies, though Wallace also alludes to this same feature (p. 258). Mr. Edward Burgess informs me of a species of Acaleph, *Polyclonia frondosa*, on the coast of Florida which lives in the mud, and is brown in colour. Darwin, while admitting that the transparency of these animals unquestionably aids them to escape the notice of their enemies, yet doubts whether the colour of mollusks affords similar protection. He says, "The colours do not appear in most cases to be of any use as a protection; they are probably the direct result, as in the lower classes, of the nature of the tissues, the patterns and sculpture of the shell depending on its manner of growth" (vol. i. p. 316).

In glancing over our New England Mollusca, however, it seems that we do have very clear evidences of protective adaptations among them, not only in their form, but more particularly in their colour. It would seem strange indeed if this were not so, since so many species of Mollusca form an important portion of the food of many fishes,* and also of certain species of birds.

In a general way, we recall the sombre colours of the shells of most species, varying through different shades of yellow, brown, and green, in this respect resembling the sand, mud, and rocks, or seaweed, in or upon which they live, and we then recall by groups the land snails of our woods, with their almost uniform brown tints, like the dead leaves or rotten wood in which they live.

The freshwater snails have similar shades to match their peculiar habitats.

The freshwater mussels, coloured likewise brown, greenish, or black, accord with their places of refuge.

Among the marine forms we notice the adaptive colouration of certain species very well marked The common *Littorina* of the coast swarms on the bladder weed, the bulbous portions of which are olive brown in colour, or yellowish, according to age. The shells of the *Littorina* found upon it, present in their varieties these two colours, and are limited to these colours, though now and then delicately banded specimens are seen.

Purpura lapillus, which generally hides beneath overhanging ledges, or is concealed under flat rocks, has generally a dirty white shell, with, now and then, a specimen bright yellow, or banded with brown. We are not aware of any fish that feeds upon this species, though in the almost universal white colour of the species an adaptive colour may be secured in resembling the white barnacles which oftentimes whiten the rocks by their numbers.

In pools left at low tide where the rocks are often clothed with the red calcareous algæ we find the little red Chiton. Certain *Mytili* are green. The young of the large *M. modiolus* has a rough coat of epidermal filaments, looking like the aborescent growth of some Alga or Hydroid.

The few species common to the mud flats exposed by the retreating tide are coloured black or dark olive. *Ilyanassa obsoleta* has the shell black, while the soft parts are quite dark. A related form, *Nassa trivittata*, lives in more sandy places, and has a similarly coloured shell. *Rissoa minuta*, inhabiting mud flats, has a shell dark olive, or nearly black, while other species of *Rissoa* are much lighter in colour. The fronds of the large Laminarian are frequented by *Lacuna vincta* and its variety *fusca*. The first is greenish or purplish horn colour, with darker bands, while the variety *fusca* is uniformly dark brown or chestnut; the colours in both cases quite match the Laminarian upon which they are found. Another species of the same genus, *Lacuna neritoidea*, Mr. Fuller has observed spawning on bladder-weed, and its yellowish tinge accords well with its surroundings. *Margarita helicini* I have found in numbers on the large Laminarian, and on seaweed at low-water mark, and its colour is decidedly protective; while other species of *Margarita*, dredged in deep water on shelly ground, are whitish, pearly, or red.

The protective colouring of certain species is well seen upon stones dredged in deep water, the various mollusks adhering to them closely resembling the calcareous algæ and the stones themselves.

Species similar to sand beaches are of various sand-coloured shades, as for example *Machæra*, *Mactra*, *Cochlodesma*, *Cyprina*, the little *Solenomya*, and *Solen*. On muddy ground we notice certain *Tellinas* and other species with white shells. It has been supposed that those species hidden from the light were generally white, and this would seem to be the case when we recall *Mya*, certain species of *Teredo*, *Tellina*, *Pholas*, and other species. Yet we do have cases where the shell is oftentimes conspicuously banded or marked. It might appear that in those species living buried in the mud or sand, the shell was protected by a very thin epidermal layer, and that this layer was eroded, thus exposing the white shell; there are certain species, however, living buried in the mud or sand, which have an epidermal coat, very thick, and dark brown or black: such examples are seen in *Solenomya borealis* and *Glycymeris siliqua*.

It has been noticed that the same species occupying different stations are differently coloured. Dr. A. A. Gould noticed this in regard to *Astarte castanea*; those thrown up from deeper water are darker coloured than those found in quiet sandy places. In his "Report on the Invertebrate Animals of Massachusetts," first edition, p. 78, speaking of the shells found in the sandy harbour of Provincetown, he says: "The colour of all the shells in that harbour is remarkably light."

A very evident case of protective colouring is seen in the three species of *Crepidula* found on our coast. *Crepidula fornicata* is drab, variously rayed and mottled with brown, and it lives attached to stones near the roots of the large Laminarian, or upon stones clothed with algæ of similar colours, or attached to the large *Mytilus*. *Crepidula convexa*, a much smaller species, lives on the roots of seaweed. Prof. Perkins records its occurrence on the black shell of *Ilyanassa obsoleta*. This *Crepidula* has a very dark brown shell, according well with the dark colour of its various places of lodgment. *Crepidula plana*

* In an inlet near Salem the writer observed a school of minnows swimming along the bottom, and as they approached a certain point jumped right and left in great alarm. For some time the disturbing cause could not be found. On closer examination, however, a Cottus was seen to open his large mouth and take in several of the little fishes. The Cottus was so perfectly protected by its colours that it was only recognised when the capacious mouth opened, and only then were the minnows alarmed. Just beyond in their track was a rusty tin fruit can, the little tin remaining on it reflecting the rays of the sun, and from this harmless object they all turned affrightedly away. In this connection it would be interesting to inquire into the food of fishes in respect to their colours. Those fishes feeding upon Mollusca would certainly not require that protection for concealment as those living upon more active prey.

or *unguiformis* lives within the apertures of larger species of Gasteropods, as *Buccinum*, *Natica*, *Busycon*, and others. The shell of this *Crepidula* is absolutely white.

There are many species that undoubtedly receive protection in allowing foreign substances to grow upon their shells, and these species, oftentimes covered by a dense growth of calcareous or other algæ, are difficult of detection by the experienced collector.

There are also certain species that habitually accumulate foreign substances upon their shells. The little *Pisidium ferrugineum* possibly finds greater immunity from danger in its habit of accumulating a ferrugineous deposit on that portion of the shell most conspicuous. *Nucula delphinodonta* has likewise a similar habit. The delicate *Lyonsia arenosa*, with its habit of entangling particles of sand in its epidermal filaments, undoubtedly finds protection in this peculiarity.

It was not the intention to go outside of New England species in citing these examples, but in this connection I cannot forbear mentioning the tropical genus *Phorus*. The species are said to frequent rough bottoms, and to scramble over the ground, like the Strombs, and not to glide evenly. This peculiar manner of moving would render them very conspicuous, and it is curious to observe that most of the species attach foreign substances to the margins of their shells as they grow, so that when a shell has attained its growth, it is almost completely concealed by fragments of shells large and small, spines of Echini, bits of coral, and stones.

These few observations are offered (and they might be multiplied) with the belief that if there is any truth in the theory of protective colouring, as advanced by Wallace, the various colours of Mollusca in many cases can be explained, and the occurrence of varieties in colour are also accounted for by the same theory.

EDWARD S. MORSE

SCIENCE AT THE LONDON SCHOOL BOARD

PROF. J. J. SYLVESTER has issued his address as candidate for election to the London School Board for Marylebone in the room of Prof. Huxley. The importance of having at least one representative of Science on the Board induces us to print his Address in full. It must be obvious that many subjects will come before the Board wherein the opinion of a man of Prof. Sylvester's scientific training will be of the highest value; and we heartily wish the Board may be fortunate enough to obtain the additional strength which will be secured by his election.

"LADIES AND GENTLEMEN,—An influential body of ratepayers have appealed to me as a man of science, to offer my services on the London School Board.

"It has been represented to me, as the wish of your great constituency, that Prof. Huxley should be replaced by one who, like himself, has made the scientific part of education the chief business of his life. On this ground I have ventured to place myself in your hands.

"My University career at Cambridge, added to my experience both as Professor of Natural Philosophy at University College, London, and subsequently as Government Professor of Mathematics during a period of fifteen years at the Royal Military Academy at Woolwich (from which I have recently retired), have given me considerable knowledge of educational matters in England. My position as Corresponding Member of the Institute of France, as Corresponding Member of the Royal Academy of Science of Berlin, as Foreign Member of the Royal Academy of Science of Naples, and other learned corporations, gives me an early and accurate knowledge of what is passing in the chief intellectual centres of the Continent. I have ample leisure for the work that is to be done, not only in attending the ordinary meetings of the Board, but also the various sub-committees on which the general working of the Act devolves, as well as the divisional and district committees, on the efficiency of which the local benefit of that Act depends.

"If you send me to the London School Board, I shall be prepared, while looking forward to the gradual adoption of a National system of Education, to adhere to that wise and moderate compromise by which, without violation of principle, you may obtain the use of existing school machinery.

"I have the honour to be, Ladies and Gentlemen, your obedient servant,

"J. J. SYLVESTER, LL.D., F.R.S.

"Central Committee Room,
25, Great Quebec Street, Marylebone Road, W."

Dr. Sylvester has already received the promise of the support of the following scientific men:—Sir Chas. Wheatstone, D.C.L.; Prof. Sharpey (Sec. Royal Society); Prof. Busk, Pres. Royal Col. Surgeons; Phillip H. Calderon, R.A.; William Heywood, C.E.; E. H. Lawrence, F.S.A.; J. Norman Lockyer, F.R.S.; J. Gerstenberg, F.R.G.S.; J. Gwyn Jeffreys, F.R.S.; Nicholas Trübner, M.R.A.S.; Prof. T. Hewitt Key, F.R.S.; Dr. Wilson; David Forbes, F.R.S.; H. W. Bates, Sec. Royal Geog. Society; Henry Holiday; Henry Watts, F.R.S.; Dr. Pick; Thomas Woolner, A.R.A.; Professor Williamson, F.R.S.; Charles Brooke, F.R.S.; Sir Henry Thompson; Colonel Stuart Wortley; Dr. Forbes Winslow, F.R.S.; Joseph Durham, A.R.A.; C. Murchison, M.D., F.R.S.; Prof. Henry Charlton Bastian, F.R.S.; William Perkins; Noel Humphreys, F.S.A.; T. Spencer Cobbold, M.D., F.R.S.; A. W. Bennett, F.L.S.; Sir Julius Benedict; Prof. W. Warington Smyth, F.R.S.; George Cruickshank; Prof. J. Percy, F.R.S.; George Harley, M.D., F.R.S.; Nevil S. Maskelyne, F.R.S.; W. S. Dallas, Sec. Geol. Soc.; Prof. G. C. Foster, F.R.S.; William Chaffers, F.S.A.; J. J. Stevenson, F.R.G.S.; and J. H. Pepper.

NOTES

WE congratulate the Science and Art Department on a resolution at which they have just arrived, in consequence of applications from science schools, to form collections of such specimens, models, diagrams, &c., as are best adapted for teaching the various branches of science which the Department aids by grants. It is proposed that collections shall be sent on loan for short periods to the local schools, to assist them in furnishing themselves with the necessary apparatus. The specimens and apparatus already in the Educational Department of the South Kensington Museum have been arranged for examination under the different subjects of instruction, and a letter has been forwarded to all the Examiners of the Department, requesting them to inspect the collections, with the view of advising what portion of them they consider may with advantage form part of the proposed travelling collections; what additions should be made, so as to give the science schools an idea of what they would require for a complete outfit; and what are the best and cheapest forms of apparatus, &c., for them to provide themselves with.

A FEW months ago we noticed the expedition to Moab which, by the aid of the British Association, was organised by Dr. Ginsburg and Dr. Tristram. We have now to announce the safe return of Dr. Ginsburg, and hope soon to be able to state some of the results of the expedition, which we have reason to believe are both numerous and interesting.

THE Society for the Encouragement of Arts, Manufactures, and Commerce is about to organise examinations in the science and technology of the various arts and manufactures of this country, which shall be conducted by a Board of Examiners, capable of testing the practical knowledge and skill required in the application of the scientific principles involved in each art or manufacture. We heartily commend this movement on the part of the Society of Arts, and may probably recur to the subject at some future time.

THE Geologists' Association has made the following excursion arrangements for March and April:—Thursday, March 21, a visit to the Museum of Practical Geology, under the guidance of Prof. Morris. Tuesday, April 2, an excursion to Maidstone, under the direction of Mr. W. H. Bensted and Prof. Tennant. Upon arriving at Maidstone the party will visit the Charles Museum, and afterwards the fine sections of the Lower Greensand, exposed in the "Iguanodon Quarries." The Kentish Rag is here well seen *in situ*. Subsequently the party will proceed to Aylesford, crossing the Medway at Allington Lock, and the Gault, Lower Greensand, and Valley Deposits yielding Mammalian Remains, there exposed, will be inspected. Saturday, April 13, an excursion to Watford and Bushey, under the leadership of Mr. John Hopkinson. The special object of interest will be the sections of the Chalk, the Woolwich and Reading Series, and of the London Clay (Basement Bed). Saturday, April 27, excursion to Hampstead, directed by Mr. Caleb Evans and Mr. S. R. Pattison. The party will visit the shaft of the Midland Railway Tunnel, and afterwards proceed to Hampstead Heath to observe the sections of the Bagshot Sands here exposed, as well as the Physiography of the District. The Annual Report of the Association for 1871 furnishes satisfactory evidence of the prosperity and progress of this useful institution. We have from time to time given so full a report of its proceedings that we need not do more than congratulate the Society on its success.

THE Board of Directors of the Edinburgh School of Art have appointed Dr. Robert Brown to the newly-created Lectureship on Geology and Palæontology, viewed more especially in the relation of the science to landscape painting, sculpture, architecture, and other fine arts and industries.

A LECTURE will be delivered for the Society of Telegraph Engineers at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Wednesday, March 27, at 7.30 P.M., by Captain P. H. Colomb, R.N., on "Telegraphing at Sea."

A LECTURE will be delivered at the London Institution, Finsbury Circus, this evening (March 21) at 7.30 P.M., on "How Plants are Fertilised," by Mr. A. W. Bennett.

MESSRS. SAMPSON LOW AND Co. have in the press Captain Butler's account of his connection with the Red River Expedition in 1869-70, and of his subsequent travels and adventures in the Manitoba country and across the Saskatchewan Valley as civil agent for the Government.

ONE of the best papers on local geology which we have recently come across was read by Mr. Thos. Beesley at the Annual Meeting of the Warwickshire Naturalists' and Archæologists' Field Club on March 5, "On the Geology of the neighbourhood of Banbury." Mr. Beesley gave a detailed account of the various strata represented in the neighbourhood, and the fossils found in them, and he ably sustained the view, in opposition to that held by Prof. Phillips, that the Inferior Oolite extends far into Oxfordshire.

THE *Traveller*, which has now been in existence nearly a year, continues to contain excellent articles on travel and geographical research, of special interest to English and Americans.

WE have received the seventh Annual Report of the Massachusetts Institute of Technology. It was established on the principle that all the studies and exercises of the first and second years should be pursued by the whole school. At the beginning of the third year, each student selects one of the following special courses of study:—1. A course in Mechanical Engineering; 2. Civil and Topographical Engineering; 3. Geology and Mining Engineering; 4. Building and Architecture; 5. Chemistry; 6. Science and Literature; 7. Natural History. These courses differ widely, but certain general studies are common to them all. It is intended to secure to every student, whatever his special course of study, a liberal mental development and general culture, as well as the more strictly technical education which may be his chief object. The course in Science and Literature, and the course in Natural History, differ from the others in having a less distinctly professional character. The former offers a sound education, based on the sciences and modern literature, and furnishes, with its wide range of elective studies, a suitable preparation for any of the departments of active life, or for teaching science. The course in Natural History affords an appropriate general training for those whose ulterior object is the special pursuit of Geology, Mineralogy, Botany, Zoology, or of Medicine, Pharmacy, or Rural Economy.

SINCE the days of its foundation, the Federal School at Zurich has, according to the *Mining Magazine and Review*, not only fulfilled its object, but has even surpassed the most well-founded hopes. In fact, each year the number of students has increased; the most distinguished professors have been happy to accept the offer of a chair in a college so flourishing; and it has already produced a number of distinguished pupils, whose reputation has placed it among the first establishments of the kind in Europe. The Swiss pupils are surpassed in number by students drawn from all the other nations of Europe, but chiefly from Russia, Poland, and Hungary, while there is a fair proportion both of Americans and Asiatics. All the cantons, however, are well represented, and the French and Italian cantons, in spite of the difference of tongue, send a very good contingent of their children. So many candidates presented themselves for admission in 1871, that it was not possible to accommodate them all; and this has again brought to the surface the idea of a Federal University, which will no doubt be speedily realised.

THE *British Medical Journal* says that the people of Rome are very much interested just now in the fate of a poor fellow, Cipriani, who has swallowed a fork in public, prongs downwards, and who is now suffering, in consequence, agonies which are the subject of daily bulletin. Some comfort may be derived by his friends from the record lately published of Mr. Lund's patient at Manchester, who survived swallowing a dessert knife six inches long; and from the perusal of a recent article in the *Journal de Médecine et de Chirurgie*, in which instances are cited where the alimentary canal has safely supported the most unexpected foreign bodies—among others, lizards, a file, a tea-spoon, a hat; and, finally, from the whimsical but melancholy instance of a man who, to amuse himself, swallowed successfully and safely a five-franc piece, a closed pocket-knife, and a coffee-spoon, but killed himself at last in the vain effort to digest a pipe.

THE *Medical Times and Gazette* of March 16 contains some interesting remarks on Prof. Laycock's Lecture on Ears delivered in Paris in 1862, a subject of special interest in connection with the recent Tichborne trial. The woodcuts with which the article is illustrated show the remarkable similarity between the square lobeless ear, met with in cases of dementia, and the ear of the chimpanzee.

DURING the last few days of December 1871, Adelaide, in South Australia, was visited, according to the *Gardener's Chronicle*, by dense clouds of locusts. Dr. Schomburgk describes the visitation as a very remarkable one. He says the air was quite darkened with them. They came from the north, and devoured everything looking green. Nothing remained of the fine lawns in the Botanic Garden but the bare brown earth.

A RICH instance of the mode in which the phenomena of nature present themselves to certain minds is furnished by the following extract from the *Prophetic News* for March 1872, published by G. J. Stevenson, 54, Paternoster Row :—" St. John in the Apocalypse has described his vision of the descent of 'the city of the New Jerusalem' into the air. . . . The Royal city may at first appear as a comet, which astronomers may be unable to understand, for its luminosity and stationary position in the eastern hemisphere may at first be but just discoverable. The news may then flash all over the globe by means of the telegraph. The unusual brilliancy of the aurora borealis seems a fitting harbinger, together with the spots *which appear* in the sun, of the approaching climax (Luke xxi. 25, 26), for through the prophetical telescope alone can we realise the intention of these wonderful phenomena. I shall be glad if some of your correspondents who may have given their thought to these points would avail themselves of the *Prophetic News* to help others to a better understanding of so important a subject."

IN the last year there was exported from Nicaragua 100 dols. worth of the waters of Nejapa, reported to have the virtue of curing drunkenness. This may be recommended to the Liquor League as better than a Maine Liquor Law. In the neighbouring State of Columbia, it is asserted by natives and Europeans, that there is an Indian cure for drunkenness.

ON the 16th of January two slight shocks of earthquake were felt at Valparaiso at 10·20 P.M. The weather was intensely cold.

ON the night of the 10th of January several shocks of earthquake were felt in Arequipa, in Peru, but no damage was done. It was observed they occurred a few hours after the new moon, and coincided with one of the highest tides of the year.

ON the 31st of January a severe shock of earthquake was felt at Patna, in Bengal.

IN the month of January there were frequent shocks of earthquake at Broossa, in Asia Minor.

ON Jan. 14 and 15 three shocks of earthquake were felt in the English hill-town of Darjeeling, in the Himalayas.

A SLIGHT shock of earthquake was felt in the middle of October at Memeodsbad, in the Ahmedabad Collectorate, Bombay Presidency.

ON the 23rd Jan. there was an earthquake at Guayaquil, in Ecuador.

IN January the heaviest fall of snow known for years took place in the hills of the Deyrsh Dhoon.

LARGE deposits of coal have been discovered at Cobquecura, in the province of Itata, Chile.

FURTHER important mineral discoveries are officially reported from Bolivia, which are expected to produce great results. In the Chaco on the road from La Paz to Fungas silver ore has been found yielding 12,000 ounces per ton, or half silver. A hundred claims were at once taken up. On the Llisa and Condormanana hills, near San Andres de Mochaca, veins of gold have been found, as well as in Vilaquil, eighteen miles from La Paz, where ancient winnowing grounds have been recognised.

ACCORDING to a report made by the Rev. Father Wolf to the Government of Ecuador, there are extensive fossil remains of the Tertiary and Quaternary epoch on the coast of Manabi and near Punin. Besides the mastodon the fossil horse is found, showing that in pre-historic times such animals were found there, though they became extinct, and the present race was introduced by the Spaniards.

IN Bolivia has been discovered an ancient mine, known as the Narango, twelve miles S. of Antofogasta, in the Mejillones district, near the Pacific. The vein is reported as composed of ochre-coloured ore, backed by a stratum, 2½ in. thick, of copper studded with gold, and containing about 20 per cent. of this precious metal.

A CORRESPONDENT of the *Ceylon Times* draws attention to the circumstance that that island is, as he believes, on the eve of an important change of climate, depending on the great cycle of thirty or thirty-three years. The past thirty years have, he maintains, shown a complete contrast to the previous thirty years, with manifestly different effects on animal and vegetable life, from the much smaller amount of rain. The next cycle of thirty years will be, he thinks, above the average, wet.

A SUIT has lately taken place in the High Court of Madras respecting a two-mouthed cow, the value of which is estimated at 1,000*l*., as large sums were made by exhibiting it. She had been seized by the sheriff, as is alleged, on wrongful distraint.

THE Ipecacuanha plants in the Neilgherries are flourishing. Two have blossomed, but have yielded no seed. Twelve plants in good condition were received at the Calcutta Botanic Gardens from England in August.

THE English Vice-Consul at Ciudad Bolivar, on the Orinoco River, Venezuela, reports that an old woman had applied an efficacious remedy for yellow fever and black vomit. It is the juice of the leaves of the vervain plant, which is obtained by bruising, and is taken in small doses three times a day. Injections of the same juice are also administered every two hours until the bowels are completely relieved of their contents. The medical men have adopted the remedy, and the number of fatal cases have been much reduced. The leaves of the female plant alone are used.

THE wild elephant which has lately destroyed fifty-six lives in the Central Provinces of India and committed such ravages, was shot on November 15 by two officers of the Government. The night before his death he killed ten persons.

A GOOD deal of attention has been excited among Egyptologists by the comparatively recent discovery in excavations made at Tanis, on the eastern or Pelusiac branch of the Nile, of a trilingual stone, somewhat of the character of the celebrated Rosetta stone, but much more perfect, and believed to be of about two hundred and fifty years' greater antiquity. This, which is now deposited in the Museum of Egyptian Antiquities at Cairo, is a perfect stela, about six feet high, two and a half feet broad, and one foot thick, the summit being arched.

PUCHMURREE or Pachmari, in the central provinces of India, is now to be marked on our maps as a town; this hill site having been successfully established as a sanitarium for English soldiers in 1870.

A SHOWER of stones is reported from Rosario, in December. A great tempest was felt, ending in a shower of stones from N.W. to S.W., and doing much damage. The shower lasted ten minutes, and the stones were abundant and large, weighing from a nut in size to a pigeon's egg. The corn fields have severely suffered. It is remarked the like occurrence had not been seen for many years, so it is to be inferred such a phenomenon is not unknown. As the Bernstadt colony was affected some European observations may be received.

TWO new discoveries of gold are announced, the one in the Transvaal district near Natal, where the gold is stated to exist in large quantities, and the other in Manitoba, in Canada.

THE STUDY OF NATURAL HISTORY

A LECTURE under this title delivered at the Royal Artillery Institution, Woolwich, by the Rev. Canon Kingsley, has just been published, containing some admirable remarks on the relation between the soldier and the naturalist, from which we cannot forbear making the following extracts.

After some introductory matter, he proceeded:—

"It seemed to me, therefore, that I might, without impertinence, ask you to consider a branch of knowledge which is becoming yearly more and more important in the eyes of well-educated civilians—of which, therefore, the soldier ought at least to know something, in order to put him on a par with the general intelligence of the nation. . . . .

"Let me, however, reassure those who may suppose, from the title of my lecture, that I am only going to recommend them to collect weeds and butterflies, 'rats and mice, and such small deer.' Far from it. The honourable title of Natural History has, and unwisely, been restricted too much of late years to the mere study of plants and animals; but I desire to restore the words to their original and proper meaning—the History of Nature; that is, of all that is born, and grows—in short, of all natural objects.

"If any one shall say, by that definition you make not only geology and chemistry branches of natural history, but meteorology and astronomy likewise—I cannot deny it; they deal, each of them, with realms of Nature. Geology is, literally, the natural history of soils and lands; chemistry the natural history of compounds, organic and inorganic; meteorology the natural history of climates; astronomy the natural history of planetary and solar bodies. And more, you cannot now study deeply any branch of what is popularly called Natural History—that is, plants and animals—without finding it necessary to learn something, and more and more as you go deeper, of those very sciences. As the marvellous interdependence of all natural objects and forces unfolds itself more and more, so the once separate sciences, which treated of different classes of natural objects, are forced to interpenetrate, as it were, and supplement themselves by knowledge borrowed from each other. Thus—to give a single instance—no man can now be a first-rate botanist unless he be also no mean meteorologist, no mean geologist, and—as Mr. Darwin has shown in his extraordinary discoveries about the fertilisation of plants by insects—no mean entomologist likewise.

"It is difficult, therefore, and indeed somewhat unwise and unfair, to put any limit to the term Natural History, save that it shall deal only with nature and with matter, and shall not pretend—as some would have it do just now—to go out of its own sphere to meddle with moral and spiritual matters. But, for practical purposes, we may define the natural history of any given spot as the history of the causes which have made it what it is, and filled it with the natural objects which it holds. And if any one would know how to study the natural history of a place, and how to write it, let him read—and if he has read its delightful pages in youth, read once again—that hitherto unrivalled little monograph, White's 'History of Selborne;' and let him then try, by the light of improved science, to do for any district where he may be stationed what White did for Selborne nearly 100 years ago. Let him study its plants, its animals, its soils and rocks, and last, but not least, its scenery, as the total outcome of what the soils, and plants, and animals have made it. I say, have made it. How far the nature of the soils and the rocks will affect the scenery of a district may be well learnt from a very clever and interesting little book of Prof. Geikie's on 'The Scenery of Scotland, as affected by its Geological Structure.' How far the plants and trees affect not merely the general beauty, the richness or barrenness of a country, but also its very shape; the rate at which the hills are destroyed and washed into the lowland; the rate at which the seaboard is being removed by the action of waves—all these are branches of study which is becoming more and more important.

"And even in the study of animals and their effects on the vegetation, questions of really deep interest will arise. You will find that certain plants and trees cannot thrive in a district, while others can, because the former are browsed down by cattle, or their seeds eaten by birds, and the latter are not; that certain seeds are carried in the coats of animals, or wafted abroad by winds—others are not; certain trees destroyed wholesale by insects, while others are not; that in a hundred ways the animal and vegetable life of a district act and react upon each other, and that the climate, the average temperature, the maximum and minimum temperatures, the rainfall, act on them, and in the case of the vegetation, are reacted on again by them. The diminution of rainfall by the destruction of forests, its increase by re-planting them, and the effect of both on the healthiness or unhealthiness of a place—as in the case of the Mauritius, where a once healthy island has become pestilential, seemingly from the clearing away of the vegetation on the banks of streams—all this, though to study it deeply requires a fair knowledge of meteorology, and even a science or two more, is surely well worth the attention of any educated man who is put in charge of the health and lives of human beings.

"You will surely agree with me that the habit of mind required for such a study as this, is the very same as is required for successful military study. In fact, I should say that the same intellect which would develop into a great military man, would develop also into a great naturalist. I say, intellect. The military man would require—what the naturalist would not—over and above his intellect, a special force of will, in order to translate his theories into fact, and make his campaigns in the field and not merely on paper. But I am speaking only of the habit of mind required for study; of that inductive habit of mind which works, steadily and by rule, from the known to the unknown—that habit of mind of which it has been said:—'The habit of seeing; the habit of knowing what we see; the habit of discerning differences and likenesses; the habit of classifying accordingly; the habit of searching for hypotheses which shall connect and explain those classified facts; the habit of verifying these hypotheses by applying them to fresh facts; the habit of throwing them away bravely if they will not fit; the habit of general patience, diligence, accuracy, reverence for facts for their own sake, and love of truth for its own sake; in one word, the habit of reverent and implicit obedience to the laws of Nature, whatever they may be—these are not merely intellectual, but also moral habits, which will stand men in practical good stead in every affair of life, and in every question, even the most awful, which may come before us as rational and social beings.' And specially valuable are they, surely, to the military man, the very essence of whose study, to be successful, lies first in continuous and accurate observation, and then in calm and judicious arrangement.

"Therefore it is that I hold, and hold strongly, that the study of physical science, far from interfering with an officer's studies, much less unfitting for them, must assist him in them, by keeping his mind always in the very attitude and the very temper which they require. . . . .

"I should like to see the study of physical science an integral part of the curriculum of every military school. I would train the mind of the lad who was to become hereafter an officer in the army—and in the navy likewise—by accustoming him to careful observation of, and sound thought about, the face of nature—of the commonest objects under his feet, just as much as of the stars above his head; provided always that he learnt, not at second-hand from books, but where alone he can really learn either war or nature—in the field, by actual observation, actual experiment. A laboratory for chemical experiment is a good thing, it is true, as far as it goes; but I should prefer to the laboratory a naturalists' field club, such as are prospering now at several of the best public schools, certain that the boys would get more of sound inductive habits of mind, as well as more health, manliness, and cheerfulness, amid scenes to remember which will be a joy for ever, than they ever can by bending over retorts and crucibles, amid smells even to remember which is a pain for ever.

"But I would, whether a field club existed or not, require of every young man entering the army or navy—indeed, of every young man entering any liberal profession whatsoever—a fair knowledge, such as would enable him to pass an examination, in what the Germans call *Erd-kunde* (earth-lore)—in that knowledge of the face of the earth and of its products for which we English have as yet cared so little that we have actually no English name for it, save the clumsy and questionable one of physical geography, and, I am sorry to say, hardly any readable school books about it, save Keith Johnston's 'Physical Atlas'—an acquaintance with which last I should certainly require of young men.

"It does seem most strange—or rather will seem most strange 100 years hence—that we, the nation of colonies, the nation of sailors, the nation of foreign commerce, the nation of foreign military stations, the nation of travellers for travelling's sake, the nation of which one man here and another there (as Schleiden sets forth in his book, 'The Plant,' in a charming ideal conversation at the Travellers' Club) has seen and enjoyed more of the wonders and beauties of this planet than the men of any nation, not even excepting the Germans—that this nation, I say, should as yet have done nothing, or all but nothing, to teach in her schools a knowledge of that planet, of which she needs to know more, and can if she will know more, than any other nation upon it. . . .

"Thus much I can say just now—and there is much more to be said—on the practical uses of natural history. But let me remind you, on the other side, if natural history will help you, you in return can help her; and would, I doubt not, help her, and help scientific men at home, if once you look fairly and steadily at the Immense importance of natural history—of the knowledge of the 'face of the earth.' I believe that all will one day feel, more or less, that to know the earth *on* which we live, and the laws of it *by* which we live, is a sacred duty to ourselves, to our children after us, and to all whom we may have to command and to influence; ay, and a duty to God likewise. For is it not an act of common reverence and faith towards Him, if He has put us into a beautiful and wonderful place, and given us faculties by which we can see, and enjoy, and use that place—is it not a duty of reverence and faith towards Him to use those faculties, and to learn the lessons which He has laid open for us? If you feel that, as I say you all will some day feel, you will surely feel likewise that it will be a good deed—I do not say a necessary duty, but still a good deed and praiseworthy—to help physical science forward, and add your contributions, however small, to our general knowledge of the earth. And how much may be done for science by British officers, especially on foreign stations, I need not point out. I know that much has been done, chivalrously and well, by officers, and that men of science own them, and give them hearty thanks for their labours; but I should like, I confess, to see more done still. I should like to see every foreign station, what one or two highly-educated officers might easily make it—an advanced post of physical science, in regular communication with our scientific societies at home, sending to them accurate and methodic details of the natural history of each district—details $\frac{99}{100}$ of which might seem worthless in the eyes of the public, but which would all be precious in the eyes of scientific men, who know that no fact is really unimportant, and more, that while plodding patiently through seemingly unimportant facts, you may stumble on one of infinite importance, both scientific and practical.

"There are those, lastly, who have neither time nor taste for the technicalties, the nice distinctions, of formal natural history; who enjoy Nature, but as artists or as sportsmen, and not as men of science. Let them follow their bent freely: but let them not suppose that in following it they can do nothing towards enlarging our knowledge of Nature, especially when on foreign stations. So far from it, drawings ought always to be valuable, whether of plants, animals, or scenery, provided only they are accurate; and the more spirited and full of genius they are, the more accurate they are certain to be; for Nature being alive, a lifeless copy of her is necessarily an untrue copy. Most thankful to any officer for a mere sight of sketches will be the closet botanist, who, to his own sorrow, knows three-fourths of his plants only from dried specimens; or the closet zoologist, who knows his animals from skins and bones. And if any one answers, 'But I cannot draw,' I rejoin, you can at least photograph. If a young officer, going out to foreign parts, and knowing nothing at all about physical science, did me the honour to ask me what he could do for science, I should tell him, learn to photograph; take photographs of every strange bit of rock formation which strikes your fancy, and of every widely-extended view which may give a notion of the general lie of the country. Append, if you can, a note or two, saying whether a plain is rich or barren; whether the rock is sandstone, limestone, granitic, metamorphic, or volcanic lava; and if there be more rocks than one, which of them lies on the other; and send them to be exhibited at a meeting of the Geological Society. I doubt not that the learned gentlemen there will find in your photographs a valuable hint or two, for which they will be much obliged. I learnt, for instance, what seemed to me most valuable geological lessons, from mere glances at drawings—I believe from photographs—of the Abyssinian ranges about Magdala.

"Or again, let a man, if he knows nothing of botany, not trouble himself with collecting and drying specimens; let him simply photograph every strange tree or new plant he sees, to give a general notion of its species, its look; let him append, where he can, a photograph of its leafage, flower, fruit, and send them to Dr. Hooker, or any distinguished botanist, and he will find that, though he may know nothing of botany, he will have pretty certainly increased the knowledge of those who do know.

"The sportsman, again—I mean the sportsman of that type which seems peculiar to these islands, who loves toil and danger for their own sakes; he surely is a naturalist, *ipso facto*, though he knows it not. He has those very habits of keen observation on which all sound knowledge of nature is based; and he, if he will—as he may do without interfering with his sport—can study the habits of the animals among whom he spends wholesome and exciting days. . . .

"The two classes which will have an increasing, it may be a preponderating, influence on the fate of the human race for some time, will be the pupils of Aristotle and those of Alexander—the men of science and the soldiers. They, and they alone, will be left to rule; because they alone, each in his own sphere, have learnt to obey. It is therefore most needful for the welfare of society that they should pull with, and not against, each other; that they should understand each other, respect each other, take counsel with each other, supplement each other's defects, bring out each other's higher tendencies, counteract each other's lower ones. The scientific man has something to learn of you, gentlemen, which I doubt not that he will learn in good time. You, again, have (as I have been hinting to you to-night) something to learn of him, which you, I doubt not, will learn in good time likewise. Repeat, each of you according to his powers, the old friendship between Aristotle and Alexander; and so, from the sympathy and co-operation of you two, a class of thinkers and actors may yet arise which can save this nation, and the other civilised nations of the world, from that of which I had rather not speak, and wish that I did not think, too often and too earnestly.

"I may be a dreamer; and I may consider in my turn, as wilder dreamers than myself, certain persons who fancy that their only business in life is to make money, the scientific man's only business to show them how to make money, and the soldier's only business to guard their money for them. Be that as it may, the finest type of civilised man which we are likely to see for some generations to come, will be produced by a combination of the truly military with the truly scientific man. I say, I may be a dreamer: but you at least, as well as my scientific friends, will bear with me; for my dream is to your honour."

SCIENTIFIC INTELLIGENCE FROM AMERICA *

A LATE number of the *College Courant*, of New Haven, contains a detailed account of the exploring expedition under Prof. Marsh, which occupied the greater part of the warm season of 1871, and of which we have already furnished occasional notices to our readers. The general plan, as already stated, embraced excursions from several points, exploring as many different fields, with special reference to the examination of regions comparatively little known. The first starting-point of operations was Fort Wallace, and from this post the cretaceous deposits of South-Western Kansas and the region of the Smoky River were investigated. The second proceeded from Fort Bridger in Western Wyoming, to examine the ancient tertiary lake basin previously discovered by Prof. Marsh. Salt Lake City was the initial point of the third exploration, and the party proceeded thence to the Shoshone Falls, on Snake River, and from there to Boisé City, in Idaho; thence they passed over the Blue Mountains to the head waters of the John Day River, and followed down to Cañon City. On the route they made extensive collections of fossil fishes. They also explored two basins, one of the pliocene and the other of the miocene age, and in these remains of extinct animals were found in large numbers; the upper bed containing the bones of the elephant, rhinoceros, lion, &c., with several species of the fossil horse; the lower and older basin was found to contain species of the rhinoceros, oreodon, turtles, &c. From this point the party proceeded to the Columbia, and thence to Portland, Oregon, where they took a steamer to San Francisco. Here the expedition divided, a portion going to the Yosemite and elsewhere, while several, with Prof. Marsh, sailed, *viâ* Panama, for New York, reaching that

* Communicated by the Scientific Editor of *Harper's Weekly*.

city on the 14th of January. We understand that the expedition was thoroughly successful in every respect, securing the collection of large numbers of fossils, as also numerous skeletons of recent animals, together with valuable antiquities, &c. The expense of the exploration amounted to nearly 15,000 dols., exclusive of the value of the services rendered by the Government. This was defrayed entirely by the gentlemen composing the party; and it is understood that the material results are to be placed in the Museum of Yale College, which will thereby be rendered the richest in America in this department of natural history.—According to Dr. Petermann, the peak of Itatiaiossu, the highest mountain in Brazil, was ascended during the past summer and its altitude determined by Mr. Glaziou, the Director of the Imperial Parks in Rio de Janeiro. It proved to have an elevation of 8,899 English feet, being somewhat less than had been previously estimated. Many species of plants were found on the mountain, and what is of great interest, a large number of Alpine species, especially of *Compositæ*, were collected at from three to seven hundred metres below the summit.—The report of progress for 1870 of the Geological Survey of Ohio, under the direction of Prof. J. S. Newberry, has just been published at Columbus, forming a volume of nearly 600 pages, with a number of accompanying maps and sections. The volume contains, besides a report of progress of 1870, a sketch of the structure of the lower coal measures in North-Western Ohio, by Prof. Newberry; the report of labours in the second geological district, by Prof. E. B. Andrews, and on the geology of Highland County, by Prof. Orton; the report of the Agricultural Survey of the State, by Mr. J. H. Klippart; a report of the chemical department, by Prof. Wormley; sketches of the geology of several counties, by Messrs. M. C. Read and E. Gilbert; a sketch of the present state of the iron manufacture in Great Britain, by W. W. Porter; and a sketch of the present state of the steel industry, by Henry Newton. All these subjects are treated with great care, and the whole volume bears ample testimony to the ability of the chief geologist and the industry of his assistants. This volume is intended as simply preliminary to the final report, which Prof. Newberry hopes to have embodied in four volumes—two of them devoted to geology and palæontology, one to economical geology, and one to agriculture, botany, and zoology. The materials for these volumes are in advanced stage of forwardness, and will embrace monographic treatises on the several subjects, which will be of the utmost benefit in ascertaining and developing the resources of the State.—A society was organised in New York some time since under the name of the "Palestine Exploration Society," with the Rev. Dr. J. P. Thompson, chairman, Dr. Howard Crosby, secretary, and James Stokes, jun., treasurer, with a list of members including the principal archæologists of the Eastern States. Its first report was published some time ago, embracing an account of the American explorers in Palestine, and the proceedings of the English Palestine Exploration Society, notices of the late explorations in Jerusalem, the Moabitic stone, &c., and concluding with an appeal to all persons interested for contributions of funds to aid in carrying out the proposed researches of the society. The field of investigation proposed includes the territory east of the Dead Sea and the Jordan Valley, as also Hermon, Lebanon, and the valleys and plains of Northern Syria. A simultaneous prosecution of researches in this field by two such bodies as the American and English societies will probably be productive of very important results, especially if supported with proper official documents from the Turkish Governments. As so much of what is now on record in regard to the geography and condition of Palestine is due to Americans, it is much to be hoped that the work may be continued by them toward a successful completion.

SCIENTIFIC SERIALS

Annalen der Chemie und Pharmacie viii. Supplement band, 3 Heft. Hesse has contributed a lengthy paper on the alkaloids of opium. It is the most exhaustive essay on the rarer alkaloids that has yet been published. He has examined minutely the following:—Pseudomorphin, laudamine, codamine, narcotine, papaverine, nitropapaverine, cryptopine, nitrocryptopine, protopine, laudanosine, and hydrocatarine, and numerous salts of each of the above. The author groups the alkaloids into four classes, the morphine, thebaine, papaverine, and narcotine groups, and gives the distinctive characters with which the members of these groups dissolve in pure concentrated sulphuric acid. Marignac follows with a long communication "On the specific heat, density, and expansion of certain solutions." Bousingault has made some experiments on the freezing of water. He took an exceedingly strong steel cylinder, placed in it a small steel bullet, and filled it entirely with water at 4° C, the cylinder was then closed by means of a cap, so that it was absolutely tight; the cylinder was exposed to a temperature of −24° for some time, but the water inside was not frozen, as was proved by the mobility of the bullet in the interior. Immediately on opening the cylinder and relieving the pressure, the water became a mass of ice.

THE *Geological Magazine* for February (No. 92) opens with some excellent notes on fossil plants by Mr. Carruthers, illustrated with a plate and several woodcuts. The subjects here referred to are the *Palæopteris hibernica*, the presence of sporangia belonging to the *Hymenophylleæ* in coal, *Osmundites Dowkeri*, the genus *Antholites*, a revision of the British forms belonging to which is given, the coniferous wood of Craigleith quarry and *Pothocites grantoni*.—Mr. S. R. Pattison communicates a note on the pyrites deposits in the province of Huelva, in Spain, and Mr. James Geikie the conclusion of his memoir on changes of climate during the glacial epoch. The latter contains a comparison of the glacial deposits of Scotland, Switzerland, Scandinavia, and North America. The other articles in the number are an abstract of the contents of Heer's "Flora Fossilis Arctica," by Mr. R. H. Scott, and an early notice (50 years old) of the occurrence and use of meteoric iron in Greenland.

SOCIETIES AND ACADEMIES

LONDON

Anthropological Institute, March 18.—Dr. Charnock, vice-president, in the chair. M. Letourneur and Dr. Haast were elected corresponding members. Mr. Geo. Harris read a paper on "The comparative Longevity of Animals of different species, and of Man; and the probable causes which mainly conduce to produce that difference." He cited several remarkable instances of longevity both in animals and man, and alluded to the opinions on the subject, both of ancient and modern writers. The influence of climate, air, and food were discussed, and also of domestication and civilisation. The theory of disease in connection more especially with concurrent decay and renovation was inquired into, and some speculations were made as to the effect future scientific discovery, as regards the medical properties both of plants and animals, might have on the question at issue.—Sir Duncan Gibb, Bart., M.D., read a paper on "The Physical Condition of Centenarians." His remarks were founded upon an examination of six genuine examples, in whom he found the organs of circulation and respiration in a condition more approaching to the prime of life than old age. There was an absence of all those changes usually observed in persons reaching 70, and in nearly all the special senses were unimpaired, the intelligence perfect; thus showing, at any rate, the complete integrity of the nervous system. The author's views were opposed to those held regarding the extreme longevity of centenarians.—Dr. Leith Adams exhibited and described a series of stone implements from the island of Herm; and Col. Fox contributed a note on some stone implements and pottery from St. Brienne, Normandy.

Entomological Society, March 4.—Prof. J. O. Westwood, president, in the chair.—Prof. Westwood exhibited living specimens of the *Acarus* described by him at the last meeting as *Argas reflexus*, from Canterbury Cathedral, and also another species of the genus found by Dr. Livingstone in Central Africa, which enters the feet of the natives between the toes, causing pain and inflammation.—Mr. S. Stevens exhibited an apparently new species of *Phycita* from near Gravesend, remarkable for its pearly colour and *Crambus*-like form.—Mr. F. Smith read an extract from a further communication from Mr. J. T. Moggridge respecting the storing of grain by ants at Mentone. Mr. Moggridge had confined a colony of the ants in a glass vessel so as to observe their habits, and he was now able to state positively that they fed upon the grain. A detailed account of the observations will be furnished by Mr. Moggridge upon his return to England.—Mr. Müller exhibited galls formed by *Acari*, of the genus *Phytoptus*, upon the leaves of *Cinnamomum nitidum*, from Bombay, being the first observation of the occurrence of those creatures in India.—Mr. H. W. Bates exhibited a series of species of *Cara-*

bus from Britain and Eastern Siberia, and remarked upon their affinities. The exhibition represented five British species and five corresponding Siberian forms, which differed totally specifically, though they might be considered representative species. One species only, *C. granulatus*, was common to the two extremities of the vast district comprising Dr. Sclater's Palæarctic Region, though there are at least fifty known European forms, and fifty others from Siberia. One other species was common to Siberia and Western North America. Mr. Bates was inclined to doubt the advisability of separating the Palæarctic and Nearctic Regions, and further he considered the partition of the globe, from a zoological point of view, into great divisions, was, to a considerable extent, based upon arbitrary evidence. He looked rather to the later geological changes, and the present configuration of land and sea, for dates upon which to ground theories of geographical distribution.—Mr. Baly communicated a paper "On new species of exotic *Cassididæ*."—Mr. Kirby communicated notes upon the butterflies described by Jablonsky and Herbst in their "Natursystem aller bekannten Insekten."—Mr. Dunning read an exhaustive memoir on the genus *Acentropus*, and after a review of the writings of the various authors who had treated upon this singular genus, he arrived at the conclusion, now almost universally maintained, that the genus is truly Lepidopterous, and further, that the evidence adduced failed to convince him of the existence of more than one species, for which he retained the name *Acentropus niveus*.

Photographic Society, March 12.—Mr. John Spiller, vice-president, in the chair. Mr. Valentine Blanchard read a paper on "Retouching: its use and abuse." While utterly condemning the frequent and elaborate retouching of negatives, such as one sees every day, Mr. Blanchard pointed out that there were occasionally some instances—for example, the correcting of false lights—where retouching was not only allowable, but really desirable, in order to render the picture more true to nature. The camera was at times at fault in reproducing objects in their true character; and under these circumstances the retouching brush or pencil might be fairly used.

CAMBRIDGE

Philosophical Society, February 12.—"Further Observations on the state of an Eye affected with a peculiar malformation," by the Astronomer Royal. In this paper the author showed by the discussion of numerical results obtained during a period of several years that the astigmatism had changed.—"The Comparison of Measures *à traits* with Measures *à bouts*," by Professor Miller. A method of comparing these measures without sinking cavities in the bars, was described, and the various processes that had been used were commented upon.

February 26.—"On Teichopsia, a form of transient half-blindness; its relation to nervous or sick headache, with an explanation of the phenomena," by Dr. Latham. The author considered the cause of the affection to be contraction of the vessels of the brain (probably the middle cerebral artery), and so a diminished supply of blood, produced by excited action of the sympathetic; and that the subsequent exhaustion of the sympathetic caused dilation of the vessels and consequent headache.—"A Machine for Tracing and otherwise exhibiting curves in connection with the theory of Vibration of Strings," by Mr. S. C. W. Ellis.

PARIS

Academy of Sciences, March 4.—M. de Saint-Venant read a continuation of his memoir on the hydrodynamics of streams.—M. Guibal presented a memoir on a ventilator applied to the aeration af mines.—M. H. Sainte-Claire Deville presented a note by M. D. Gernez on the absorption-spectra of chlorine and chloride of iodine.—M. W. de Fonvielle communicated an explanation of three cases of fulguration in which the lightning-conductors proved to be insufficient.—M. Sainte-Clare Deville presented a note by M. E. H. von Baumhauer on the origin of auroras, in which the author called attention to an explanation of these phenomena given by him in a work "De ortu lapidum meteoricorum," published at Utrecht in 1844. The author ascribes the production of auroras to the penetration into our atmosphere of clouds of uncondensed cosmical matter, the presence of iron and nickel in which, he seems to think, may account for their being attracted towards the magnetic poles of the earth. —A note by M. H. Caron on crystallised or "burnt" iron was read, in which the author treated of the brittle condition produced in a bar of iron when heated to whiteness and allowed to cool in the air. He finds that this effect is not due to an absorption of oxygen as has been supposed. He also states that good iron is not rendered crystalline by exposure to intense cold.—M. Wurtz presented a note by M. G. Bouchardat upon the acetic æthers of dulcite, in which the author describes the following compounds:—diacetic dulcite, diacetic dulcitane, hexacetic dulcite, tetracetic dulcitane, pentacetomonochlorhydric dulcite, and pentacetic dulcite.—M. Wurtz also presented a note by M. Reboul on the hydrobromates and hydrochlorates of allylene, and a note on pyruvine, by M. Schlagdenhauffen. The latter is a glyceride of pyruvic acid obtained by heating glycerine with tartaric acid.—M. Fremy communicated a note by M. E. Landrin, on the reciprocal action of acids and alkaline bases when separated by a porous partition.—M. L. Kessler forwarded a note on a modification of the processes for the determination of nitrogen in a free state in the analysis of organic substances.—M. Decaisne presented a note by M. J. E. Planchon, on *Cratægus aronia* (Spach) and its relations with *C. oxyacantha* and *C. azarolus* of Linné. The author regards *C. aronia* as a cross of the other two forms, which are probably distinct races of the same species.—M. E. Robert accounts for the renewed fermentation of wines at the period of the flowering of the vine, by the abundance of germs of *Mycoderma vini* in the atmosphere at that period.

BOOKS RECEIVED

ENGLISH.—The Year Book of Facts, 1872: J. Timbs (Lockwood and Co.). —An Elementary Treatise on Curve Tracing: P. Frost (Macmillan and Co.). —Monograph of the British Graptolitidæ: H. A. Nicholson (Edinburgh, Blackwood and Sons).

DIARY

THURSDAY, MARCH 21.

ROYAL SOCIETY, at 8.30.—New Researches on the Phosphorus Bases: Dr. Hofmann, F.R.S.—On some Heterogenic Modes of Origin of Flagellated Monads, Fungus-Germs, and Ciliated Infusoria: Dr. Bastian, F.R.S.

SOCIETY OF ANTIQUARIES, at 8.30.—Ballot for the Election of Fellows.

LONDON INSTITUTION, at 7.—How Plants are Fertilised: A. W. Bennett.

ROYAL INSTITUTION, at 3.—On the Chemistry of Alkalies and Alkali Manufacture: Prof. Odling, F.R.S.

LINNEAN SOCIETY, at 8.—On the Geographical Distribution of Compositæ: G. Bentham.

CHEMICAL SOCIETY, at 8.

FRIDAY, MARCH 22.

ROYAL COLLEGE OF SURGEONS, at 4.—On the Digestive Organs of the Vertebrata: Prof. Flower, F.R.S.

ROYAL INSTITUTION, at 9.—On the Results of the last Eclipse Expedition; J. Norman Lockyer, F.R.S.

QUEKETT MICROSCOPICAL CLUB, at 8.

SATURDAY, MARCH 23.

ROYAL INSTITUTION, at 3.—Demonology: M. D. Conway.

MONDAY, MARCH 25

ROYAL COLLEGE OF SURGEONS, at 4.—On the Digestive Organs of the Vertebrata: Prof. Flower, F.R.S.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.

WEDNESDAY, MARCH 27.

ROYAL COLLEGE OF SURGEONS, at 4.—On the Digestive Organs of the Vertebrata: Prof. Flower, F.R.S.

ROYAL SOCIETY OF LITERATURE, at 8.30.—On some Greek and other inscriptions recently procured in the Haurân: W. S. W. Vaux.

CONTENTS

ERRATA.—Page 379, 2nd col., lines 16, 17, should read thus:—

Present River . {low water. 75
{flood water . . . . 8·81 2,400

Line 13 from bottom, for "4·6 per cent." read 4·6 degrees.

THURSDAY, MARCH 28, 1872

THE IRON AND STEEL INSTITUTE

THE Third Annual Meeting of the Iron and Steel Institute was last week held in London, under the presidency of Mr. Henry Bessemer, and has been numerously attended by representatives, not only of the principal iron and steel works in the United Kingdom, but also by those of many of the most important metallurgical establishments on the Continent, which in several instances have sent special delegates to this meeting.

It will perhaps be remembered that the Iron and Steel Institute was founded barely three years ago, and that upon the occasion of the Inaugural Address, delivered by the first president (the Duke of Devonshire), it had then only received the adherence of some two hundred gentlemen connected with the trade; whereas, on this occasion, notwithstanding that the rules of the society only allow the admission of those either practically engaged in the manufacture or application of iron and steel, or connected therewith by their scientific attainments, it has increased so rapidly in this short interval as to number at present about five hundred members, including in this list nearly all the influence and talent associated with the iron and steel industries of Great Britain. It is self-evident, therefore, that its establishment must be regarded as a complete success, such as could not have been expected had it not supplied a tacitly acknowledged previously existing want. That this conclusion is one accepted not only here at home, but also in every part of the world where the manufacture of these metals is carried on, may be considered as demonstrated on the occasion of this last meeting of the Institute, by the attendance of gentlemen connected with the iron and steel trades of France, Belgium, Germany, Sweden, Russia, Spain, and the United States, several of whom, although foreigners, have, we understand, been so impressed with the good service which the Institute is doing to these metallic industries, as to have enrolled themselves on its list of members.

This unexampled success is no doubt in great part due to the fact that the Council of the Institute have conscientiously adhered to the original programme, in not allowing any of what may be termed trade or purely mercantile considerations to interfere with the true objects for which the Institute was from the first established; these objects being, the scientific and practical inquiry into and the open discussion of all subjects bearing directly or indirectly upon the production and working of iron and steel, to provide the members with a means of intercommunication of their ideas and practical experiences, and to supply them with as accurate information as possible as to what is being done in the same direction in foreign countries as well as at home. How far these aims have been attained in practice, may best be judged of by the rapid increase in members, and by referring to the volumes of the Journal already published by the Institute, which, both abroad as well as at home, have been universally admitted to sustain the high standard aspired to from the first by this young but vigorous institution, and to stand alone in their line, whether regarded from a purely scientific or a practical point of view.

The two annual (London and country) meetings of the Institute may be likened to those of a permanent technical tribunal, before which everything new in connection with iron and steel has to be brought forward, and judged upon as to its merits, after having first passed through the ordeal of cross-examination by the scientific and practical members of the Institute, with the object, as the president tersely expressed it, of sifting out the grain from the chaff; and short as the existence of the Institute has as yet been it has still been long enough to prove how much the iron trade in general, and inventors in particular, may gain by the constitution of such a tribunal.

The most interesting and important feature of the present meeting has been the reports of the committee on machine puddling. The operation of puddling in the conversion of cast into wrought iron is one of so arduous and trying a nature to the workmen that it is daily becoming, in great part owing to the spread of education and the growing desire of men to better their position in society, more difficult to find hands willing to engage in such heavy work; and as it requires long training to make a good puddler, it has now become altogether impossible to obtain a supply of such workmen sufficient to keep pace with the increasing demand for the product; for which reason we find the manufacturer of wrought-iron completely at the mercy of these men, who, besides not ranking very high in the scale of humanity, keep the ironmasters in a perpetual state of terror by their frequent strikes, which, as a rule, do not benefit either party, yet always result in damaging the general iron trade of the kingdom, by driving it abroad and otherwise. This state of things has, as might naturally be expected, given rise to numerous attempts to supersede manual labour in puddling, by machinery, although it may be said, as yet, unsuccessfully; since, notwithstanding that attempts have been made in all directions, and on the most opposite systems, no one of them, when carefully examined into by the Puddling Committee of the Institute, has been considered to fulfil all the conditions requisite to insure its general adoption. When, therefore, at the meeting of the Institute last autumn, in Dudley, Mr. Danks (an American, although born in Staffordshire) declared that he had successfully solved this problem, his announcement was received with considerable incredulity, and he was requested to explain his system before the Institute. To the surprise, yet it may also be added gratification, of all, his explanations, after having been submitted to a severe cross-examination, were considered so far feasible that the members of the Institute unanimously decided upon taking up the matter, and at once sending out a commission (at an expense of some two thousand pounds) to test the system there, with the furnaces and machinery already erected by Mr. Danks, at the Cincinnati Ironworks, but taking with them sufficient pig-iron and other materials from England and Wales to enable them to thoroughly test the system on the large scale, and thereby insure that the process is adaptable to the products we have to treat in this country. After a most patient and painstaking investigation, the three gentlemen who composed this committee — Messrs.

Snelus, Jones, and Lester—reported the system as a complete success, and well suited for the treatment of the iron of this country, an announcement which was received with the greatest interest; and steps were immediately taken to erect similar appliances in England, so that already in the month of February, one of Mr. Danks's furnaces was at work with results which fully corroborated the report of the commissioners, and left no doubt but that the invention must entirely revolutionise this branch of the iron manufacture, doing away with the severe, and it might almost be called degrading, labour of manual puddling altogether, and in other respects producing wrought-iron of a more certain and superior quality to the product obtained from the same pig-iron by the old system.

It is almost impossible to over-estimate the direct and indirect benefits which must accrue to that greatest of all metallic industries, the iron manufacture; and as it might have been years before this invention had asserted itself had it not been taken up so energetically by the Iron and Steel Institute, this may be mentioned as a striking instance of the important results which may be expected from the labours of such a society.

NICHOLSON ON THE GRAPTOLITES

Monograph of the British Graptolitidæ. By H. A. Nicholson, M.D., &c. (Edinburgh: Blackwood and Sons.)

IT is with no small degree of satisfaction that we welcome the appearance of the first part of Dr. H. A. Nicholson's Monograph of the British Graptolites, the first English essay attempting a clear digest or history of this very difficult and perplexing group of fossils. Dr. Nicholson has, however, for years lived in those regions whose rock masses, life contents, and structure were long since elucidated and rendered classical and famous by the researches of Sedgwick in 1848; and where these organisms are most abundantly distributed. Patient investigation of the great stores of entombed materials at his command, combined with requisite knowledge of zoology, has favoured the author in the preparation of this valuable contribution to our hitherto limited knowledge of these extinct forms of life.

Much has been written upon the Graptolitidæ, but in a disjointed manner, by numerous writers since 1727; but Linnæus, in his "Skanska Resa" in 1768, first applied the name "graptolithus" to some or certain allied forms occurring in the Scandinavian rocks. Much controversy has been carried on about this original scalariform type of graptolite; some writers believing it to have been a monoprionidian, others a diprionidian genus. It signifies little now save as matter of history. Since then eighteen genera and ninety species have been established and recognised in Britain alone, and these have been mostly obtained from rocks of Lower Silurian age. Seven species out of the ninety are only known in the Upper Silurian rocks, and four of these are peculiar to that horizon, or do not range lower. The authenticity then of the character of the one and disputed Linnæan form, will do little more after all than add to the literature of the group. This original figure is sufficient to show us that it was a graptolite in our acceptation of the genus, and doubtless the form looked upon and drawn by the illustrious Swede was *one of millions* contained in the black and slaty rocks over which he travelled; a form, with many others since discovered, and now known to all students of those Silurian rocks which belt the earth from Canada to Britain, Scandinavia, Saxony, and Bohemia, and on to Australia. The historical notice of the Graptolitidæ occupies seventeen pages, and forms a compilation of the bibliography of the group, for which all students will gladly thank the author, from 1821-2, when Wahlenberg and Schlotheim advocated their alliance to the Cephalopoda, to Hopkinson's last paper in 1871 (*describing the reproductive capsules*). We have, in fact, a well-digested chronological history, enumerating about eighty notices, and embracing the labour of thirty-five authors.

To study and examine the graptolites *in situ*, or as they occur in the black paper like flaggy shales of the Arenig, Llandeilo, and Caradoc beds, to which they are chiefly confined in Wales, Westmoreland, Scotland, and Ireland, is no small pleasure; but after their stratigraphical position or succession in time is definitely settled in any area to the satisfaction of the physical geologist or stratigraphist, the question of their zoological affinities, or the position they hold in the animal kingdom with relation to modern and existing types becomes one of high importance and value, yet one even now not satisfactorily determined or established. Were they free swimming or floating bodies, in the old Silurian seas, or were they attached like the hydroid Sertularidæ of modern shores and time? These questions are dealt with by the author under two heads: first, *their mode of existence*, and secondly, their *systematic position and affinities*. To our mind the modes of existence of the Graptolitidæ have little weight in classification; a knowledge of their intimate structure alone must be the basis of their zoological position in the animal kingdom.

It was natural that the older writers should have referred this extinct group to many divisions which themselves were not then really understood; and they have been placed in no less than six divisions of the animal kingdom.

Modern systematists, however, have referred them to three groups—the Hydrozoa, Polyzoa, and Actinozoa. In 1839 Sir R. Murchison, in his Silurian System, placed them with the Actinozoa, assigning their position to the Pennatulidæ, and related to the Virgularia of the northern seas. No real analogy however exists between the tubular chitonous fibre of the graptolites, and the calcareous or sclerobasic rod of Virgularia, whose cœnosarc secretes no external envelope, and where the polypes are not contained in, or protected by, special chitonous thecæ. All research also tends to show that the graptolites were free bodies and perhaps oceanic; the structure and condition of the radicle or initial point is conclusive on this point. With respect to their development we as yet know little; but the fact that, as in other Hydrozoa, the reproductive organs were outwardly developed processes of the body wall, strongly allies them to the Hydrozoa. Hopkinson has of late added much to our knowledge of the external reproductive sacs or gonothecæ of Diplograpsus.

To Colonel Pollock is undoubtedly due the suggestion of their sertularian affinities through Sertularia and Plumularia, but they certainly are not their fossil representatives.

The author wisely "regards them as a special group of Hydrozoa" unrepresented by any living forms, and forms them into a distinct sub-class.

Chapter II. is devoted to the form and mode of reproduction. This, we think, would have been better placed after the chapter on their special morphology, or prior to Chapter VIII., which is devoted to their geological distribution. We are prepared to admit, however, that much error has arisen from our want of clearly understanding their true history and the mode of their preservation in rocks of such varied physical texture and chemical condition.

Chapters III. and IV. are devoted to the general and special morphology of the graptolites; typical forms being selected in Chapter III., in which the main anatomical features and aspects are recognisable. For this purpose the author has selected the well-known forms of *G. sagittarius*, *G. colonus*, and *Climacograpsus teretiusculus*, and devotes fifteen figures to the elucidation of the monoprionidian and diprionidian type of structure.

Chapter IV. embraces thirteen pages and thirty-five figures devoted to the special morphology of the graptolites. We regard this chapter as a condensed history or digest of the labours of European, American, and British graptolithologists. The views and labours of Hall in Canada, Geinitz, Nilsson, and Barrande in Europe, Salter, Carruthers, M'Coy, Hopkinson, Harkness, &c., and the author in Britain, are embodied under the *nature of the solid axis*, *common canal*, *cœnosarc*, *cellules*, *and ornamentation of the polypary*.

Space forbids us to do more than notice that in Chapter V. twelve pages and twenty-two figures are occupied by the consideration of chief and special portions of the graptolites, viz. the "radicle or initial point of Hall," and the basal process, the funicle, or non-celluliferous connecting process, largely developed in the Dichograpsi, and the central disc of the Tetragrapsi. Whether these corneous bodies find their analogue in the float of certain oceanic Hydrozoa has yet to be determined.

The chapter upon reproduction and development contains much important matter. The evidence of reproductive organs, however, amongst a group so obscurely preserved as the graptolites must be studied with much care, and deductions received with much caution, but since Hall, in 1858, first drew attention to what he believed were ovarian capsules, Mr. Hopkinson in 1871 confirmed the discovery and description of pyriform gonothecæ or ovarian capsules in *Diplograpsus pristis*.

Nicholson had, in 1866, noticed bodies which he believed to be, and referred to, reproductive bodies, and named them *grapto-gonophores*. He, however, had doubts as to their analogy. Mr. Carruthers differed from the deductions of Nicholson, maintaining that these bodies were accidental, or did not belong to the graptolites, although associated or in juxtaposition with them.

Mr. Carruthers first drew attention to and noticed the existence of young forms of graptolites; but Prof. Hall appears to have been the first to make accurate observations upon their development (Grap. of Quebec group, Pl. B, p. 12—19). We, however, as yet know little about this obscure question or point in their history.

The chapter upon the systematic or zoological position of the graptolites is a valuable one, the author taking and adopting what we believe to be the right view, *placing them in the Hydrozoa*. This is the first and invariable question of the systematist; the naturalist shirks the question and waits.

It is quite impossible within the limits at our command to discuss the interesting problem of the geological distribution of the graptolites. Although strictly Silurian as regards age, and only occurring in rocks of that period, yet their assignment to the special area which gave birth to them, and from whence they became distributed in space, is a problem yet to be worked out. We believe this has been elsewhere attempted by the author. That the Quebec genera and many species agree in the main with the so-called Arenig or Skiddaw slate forms in Britain is certain, and this is a fact of much interest as a question of distribution. At present we know of no species in the Tremadoc beds, omitting Dictyonema of doubtful affinity; and the statement that the lower Llandeilo flags of Wales are the precise equivalents of the Skiddaw slate of Westmoreland needs confirmation; neither should we hastily accept the generalisation that the Potsdam group in America is upon the horizon of the Skiddaw series, but rather perhaps refer the Quebec and Chazy series to the Arenig or Skiddaw beds of the lake country, where, or in the Llandeilo area in Wales, the graptolites perhaps came first into existence, unless to Canada we refer their birth-place. Homotaxically, however, we require more data. Nine out of fifteen genera are common to Britain and Canada; and this though the Skiddaw slates of Westmoreland, indeed the Skiddaw and Llandeilo rocks and their equivalents, are the graptolitic beds throughout Europe if not the world. The old generalisation as to the diprionidian species occurring in the Upper Silurian is confirmed and borne out by the researches of Nicholson: the unsatisfactory genus Retiolipes alone being found. The sea which deposited the Caradoc rocks saw the last of the compound species, and the physical nonconformity was also a zoological one, especially in hydrozöoid life. Indeed only 140 species of all groups of 1,450 known Silurian species, or 10 per cent., are common to rocks of Lower Silurian and Upper Silurian time.

Chapter IX. deals with the generic characters of the radiculate group, omitting those of doubtful affinity; the author follows the sectional grouping of Barrande, adopting monoprionidian and diprionidian, &c., as modified by Hopkinson.

We look forward with much interest to the part containing full and detailed descriptions of the species. The splendid volume by Prof. Hall and Sir William Logan upon the Canadian species (Report of Progress of the Geological Survey of Canada, 1857, and figured descriptions of Canadian organic remains Decade 2, Grap. of Quebec group) we hope to see equalled if not surpassed by the author of the present valuable memoir. R. E.

OUR BOOK SHELF

Observations upon the Climate of Uckfield. A Meteorological Record for the district from 1843 to 1870, &c. By C. Leeson Prince, M.R.C.S., F.R.A.S. (London: Churchill, 1871.)

WE opened this work expecting to find in it a mere record of the barometric and thermometric observations taken by an assiduous observer for twenty-seven years. It is this, however, and much more; and Mr. Prince must be congratulated upon having written a very interesting and readable book upon what we fear would, in the hands of most men, be a very dry subject. The observations he has collected show what valuable information might be stored up by many country surgeons, clergymen, and farmers, at little cost of time or money, by adopting a regular system. The parish of Uckfield, Mr. Prince tells us, lies upon an undulatory tract of country situated about midway between the South Downs and the highest point of Ashdown Forest. The upper portion of the town is 200 feet, and the lower 66 feet, above the level of the sea. It is situated on the Horsted beds of the Hastings Sands. The instruments were read every morning at nine o'clock. The annual mean height of the barometer at Uckfield, as deduced from observations extending over seventeen years, was 29·982 in. Mr. Prince gives the mean temperature of winter at Uckfield from all his observations at 38°·96 Fahr.; of spring at 47°·66; of summer at 61°·34, and of autumn at 50°·45. The coldest winter was that of 1845; the warmest that of 1869; the difference being 10°·99. The coldest spring was that of 1845; the warmest that of 1848; the difference, 5°·84. The coldest summer was that of 1860; the warmest that of 1859; the difference being 6°·74. The coldest autumn was that of 1867; the warmest that of 1857; the difference being 6°·22.

Mr. Prince points out that "the mean annual temperature varies 5°·3, viz.: from 51°·93 in 1857 to 46°·62 in 1845, and although at first sight this difference may not appear considerable, yet it is sufficient to exert an enormous influence upon the general character of the seasons, the produce of the soil, and the health of the population. The Registrar-General's interesting returns have fully established the important fact that there is a very intimate connection between temperature and mortality. Whenever the mean temperature falls to 45°, or thereabouts, the number of deaths from diseases of the respiratory organs increases, and should it fall below 40°, death-rate from such diseases is still higher. When a period of intense cold prevails, so that the temperature scarcely rises above the freezing point for two or three weeks, the number of deaths will be found to exceed what takes place during an epidemic of cholera or scarlet fever. But when the mean temperature rises to 55°, there will be an increase in the number of deaths from diseases of the abdominal viscera, and this number will fluctuate as the temperature fluctuates between 55° and 65°. Hence we are informed that the mortality from all causes is least when the temperature is about 50°, which is very little above our mean annual temperature." In this way Mr. Prince deduces important conclusions from statistics, and renders his book much lighter reading than might have been anticipated. He devotes a chapter to the general characters of the months, and then inserts a series of monthly remarks respecting atmospheric phenomena from the year 1843 to 1870, both inclusive. His fifth chapter treats of prognostics of atmospheric changes, and includes a translation of the poet Aratus' "Diosemeia." He remarks very sensibly that with reference to prognostics of seasons, there are very few upon which any reliance can be placed. But the following, of which we can only quote a few, need not, he thinks, be altogether discarded.

From whatever quarter the wind blows at the quarter days, there is a probability of its being the prevalent wind during the ensuing quarter. Whenever the latter part of February and beginning of March are dry, there will be a deficiency of rain up to Midsummer-day. When the foliage of the ash appears before that of the oak, we shall probably have much rain the first half of the summer; but there will be a good harvest-time. When during the spring more swifts than swallows arrive, expect a hot and dry summer. Many other prognostics of change of weather are given, drawn from the habits of mammals, birds, insects, and plants, some of which are very curious.

The last chapter gives some vital statistics in regard to the population of the country; from which it appears that Sussex is one of the most salubrious counties in England, its death-rate being 1·82 per cent., in which it is surpassed only by the extra-Metropolitan portion of Surrey, the mortality of which is only 1·78; whilst that of Lancashire is 2·78 per cent. Upon the whole we warmly recommend Mr. Prince's book to our readers, and trust that some of them may be induced to commence a similar series of observations. A flora of the district, with the times of flowering of the plants, would, we think, be an interesting addition to Mr. Prince's work. 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.*]

Circumpolar Land

In a previous letter* I have endeavoured to show that the land surrounding the North Pole is rising in a continuous and definite area. I find that what I there said about the land north of America is very scanty and unsatisfactory, and before proceeding to the next part of my subject, I wish to strengthen it somewhat. Speaking of the eastern part of Melville Island, Captain Parry says one of the *Hecla's* men brought to the boat a narwhal horn, which he found on a hill more than a mile from the sea. Sergeant Martin and Captain Sabine's servant brought down to the beach several pieces of fir tree, which they found nearly buried in the sand, at the distance of 300 or 400 yards from the present high-water mark, and not less than thirty feet above the sea level (Parry's Voyage, 1819, 1820, p. 68). Again, "in the north of Melville Island, two pieces of drift wood were found, ten or twenty feet above the present sea level, and both partly buried in the sand" (p. 193). Again, speaking of west of the same island, "The land gains upon the sea, as it is called, in process of time, as it has certainly done here, from the situation in which we found the drift wood and the skeletons of whales" (p. 235).

In Franklin's voyage in 1819, 20, and 21, he mentions having found much drift wood in the estuary of the Copper Mine River. He also picked up "some decayed wood far out of reach of the water" (see his narrative, p. 357). In his second voyage along the Arctic Sea, he describes the coast from the Mackenzie River to the Rocky Mountains as very shallow, and full of shoals and reefs. Inside some of the latter was brackish water, as was also the water in pools at some distance inland; piles of wood were also thrown up far from the coast (see p. 134). While Franklin surveyed the coast westward, Dr. Richardson did the same to the east. He says, "On the coast from Cape Lyon to Point Keats, there is a line of large drift timber, evidently thrown up by the waves, about twelve feet in perpendicular height above the ordinary spring tides." He shortly afterwards mentions that in the Polar Sea, when cumbered with ice, such waves are impossible, and as his journey was in the hottest season, and the sea was then crowded with hummocks, the inference that the drift wood was thrown up by the waves is inadmissible; and the line of drift wood twelve feet above the sea level is only a parallel to the numerous cases we have mentioned. The vast sheet of shallow and brackish water, 140 miles long and 150 broad, which is separated from the Polar Sea by low banks and spits of sand, and is called by Dr. Richardson Esquimaux Lake, formed, there can be little doubt, very recently, as that traveller suggested, a bay of the Polar Sea, and is an example of the formation of huge brackish lakes by a sea which is constantly contracting, such as are so familiar in the eastern borders of the Caspian.

It would be impossible, in the short space at my command, to collect the many instances of the same kind that are found in the later Arctic voyages; but I would especially commend the pages of Captain Maclure's and of Sir Edward Belcher's narratives, as containing very striking ones.

The orthodox school of physical geographers generally speak

* See NATURE, vol. v., p. 163.

of Behring's Straits, and the shallow sea about the islands, as an area of depression, but without any authority, so far as I know.

Those barren and desolate islands, so well described by the Russians, bear all the traces of having recently been under water, and the American Birkbeck has proved, beyond much doubt, that the eastern coasts of Asia, including China and Japan, are being upheaved. I find I was forestalled by Pennant in the conjecture of the very recent junction of the White Sea and the Baltic, and I am very glad to quote him as an authority. He says the lakes Sig, Ondar, and Wigo, form successive links from the Lake Onega to the White Sea. The Lake Saima almost cuts Finland through from north to south; its northern end is not remote from Lake Onda, and the southern extends very near to the Gulf of Finland, a space of nearly 40 Swedish or 260 English miles. These, probably, were part of the bed of the ancient Streights (*sic*) which joined the White and Baltic Seas (Appendix to Arctic Zoology, 23).

In regard to the rise of Spitzbergen, it is curious to find the following passage so early as 1646:—"These mountains (twenty-two mountains of Spitzbergen) increase in bulk every year, so as to be plainly discovered by those that pass that way. Leonin was not a little surprised to discover upon one of these hills about a league from the sea-side, a small mast or a ship, with one of its pullies still fastened to it; this made him ask the seamen how that mast came there, who told him they were not able to tell, but were sure they had seen it as long as they had used that coast. Perhaps, formerly, the sea might either cover or come near their mountain, where some ship or other being stranded, this mast is some remnant of that wreck." (Account of Greenland by M. La Peyrere in Churchill's Voyages, vol. ii.) Parry, in his account of his journey towards the Pole, 126, also refers to the vast quantities of drift wood stranded on the Spitzbergen coasts above high-water mark.

Having strengthened my former paper by instances of upheaval in other points, and I hope satisfied your readers of the justice of the generalisation about the rise of circumpolar land, it is natural to ask if this remarkable fact is paralleled in any way at the southern pole,—whether we can show that both in the Arctic and Antarctic seas there is a bulging out of the land, and a displacement of the sea at present in progress. Our knowledge of the lands immediately about the southern pole is very scanty; but fortunately we have unmistakeable evidence at the various points of those better known austral lands which approach the antarctic seas, from which we may be justified in drawing a sound conclusion, South America, New Zealand, Australia, Tasmania, and Southern Africa.

To begin with South America, I cannot quote a better authority than Mr. Darwin:—

"Everything in this southern continent has been effected on a grand scale: the land from the Rio Plata to Terra del Fuego, a distance of 1,200 miles, has been raised in mass (and in Patagonia to a height of between 300 and 400 feet) within the period of the now-existing sea shells. The old and weathered shells left on the surface of the upraised plain still partially retain their colours. . . . . I have said that within the period of existing sea shells, Patagonia has been raised 300 to 400 feet; I may add that within the period when icebergs transported boulders over the upper plain of Santa Cruz the elevation has been at least 1,500 feet" (Naturalists' Voyage p. 171). Again, "M. d'Orbigny found on the banks of the Parana, at the height of 100 feet, great beds of an estuary shell now living 100 miles lower down nearer the sea, and I found similar shells at a less height on the banks of the Uruguay; this shows that just before the Pampas was slowly elevated into dry land the water covering it was brackish. Below Buenos Ayres there are upraised beds of sea-shells of existing species, which also proves that the period of elevation of the Pampas was within the recent period" (p. 130). So much for the East Coast. Now for the West. Speaking of the Hacienda of Quintero, in Central Chili, he says:—"The proofs of the elevation of this whole line of coast are unequivocal. At the height of a few hundred feet old-looking shells are very numerous." Again, speaking of Northern Chili, he says:—"I have convincing proofs that this part of the continent of South America has been elevated near the coast at least from 400 to 500 feet, and in some parts from 1,000 to 1,300 feet, since the epoch of existing shells, and further inland the rise may have been greater." In Peru, about Callao, he also found evidences of rising land; but here we come to one of the horizons where rising and sinking land meet. If it be necessary to supplement the account of Mr. Darwin, I have the authority of Mr. Baxendall for stating that he found numerous skeletons of whales and seals stranded above high-water mark on the coast near Africa, where a tide (as is well known to be the case in all the Eastern Pacific) is almost unknown.

Having satisfied ourselves of the rise of the southern portion of South America, we must now shortly state the reasons for making it very recent. Speaking of the earthquake of 1822, which caused a general upheaval of the land, Mr. Darwin says, "The most remarkable effect of this earthquake was the permanent elevation of the land; the land round the Bay of Conception was upraised two or three feet, at the island of Santa Maria (about thirty miles distant) the elevation was greater. On one part Captain FitzRoy found beds of putrid mussel-shells still adhering to the rocks 10 feet above high water-mark; the inhabitants had formerly dived at low-water spring tides for these shells" (p. 310). Again, two years and three-quarters afterwards Valdivia and Chiloe were again shaken, and an island in the Chonos Archipelago was permanently elevated more than 8 feet. At Valparaiso within the last 220 years the rise has been somewhat less than 19 feet, while at Lima a sea beach has certainly been upheaved from 80 to 90 feet within the Indo-human period (*id. passim*). Eighty-five feet above the sea level in an island in the Bay of Callao he found on a sea beach some Indian corn and pieces of Indian thread, similar to those found in Peruvian tombs, a parallel find to that made by Sir Charles Lyell in Scandinavia, which I previously referred to.

Having examined the evidence for South America, we will now turn to the other great southern continent, Africa. I will quote a few passages. "There cannot be the slightest doubt that the upheaval of the country is still going on; for along the whole coast of South Africa from the Cape to Durham Bluff, and still farther north, even as far as Zanzibar, modern raised beaches, coral reefs, and oyster banks may everywhere be seen. At the Izinhluzabalungu Caves is such a point, where the rising of the coast is plainly visible, recent oyster-shells are now 12 feet and more above high-water mark. The same can be observed on the whole line of the Natal Coast. Van der Decken has observed the same thing at Zanzibar, and is of the same opinion as myself, viz., that the Eastern Coast is rising early in the present year (*i.e.*, 1870). I had the opportunity of observing at the Bazanito Islands about ninety miles north of Inhambane, on the east coast of Africa, a series of raised coral reefs round the island of Marsha containing many living shells and quite recent oyster-banks." (Griesbach, Geology of Natal, Quart. Journ. Geol. Soc. xxvii. part ii. p. 69.) Mr. Griesbach also mentions that he saw implements of early man, which were obtained by Richard Thornton and others in old raised beaches of Natal, near Inanda, and at the mouth of the Zambesi River.

Mr. Griesbach is confirmed by Mr. Stow in his papers on the Geology of South Africa in the same Journal (see vol. xxvii. p. 526 *et seq.*), where bones and teeth are found mixed with shells, quite in a recent state, about Port Elizabeth, &c.

In regard to Tasmania, I quote the following from Mr. Wintle's paper on the Geology of Hobart Town (*Mine Journal*, vol. xxvii. p. 469):—"Until a very recent period in the geological annals of this island, a great portion of what now constitutes the site of this city was under water. This is proved by the extensive deposits of comminuted shells, all of recent species, which are met with for miles along the banks of the Derwent. Some of these deposits are at an elevation of upwards of 100 feet above high-water mark, and from 50 to 100 yards from the water's edge, plainly showing thereby that a very recent elevation of the land has taken place."

In New Zealand the evidence is the same. M. Reclus says the port of Lyttelton has risen 3 feet since it was occupied by the settlers. Mr. Forbes says that proofs of upheaving of the land are even now obvious to any intelligent traveller. Some of these changes have been witnessed by the present generation. Again, in the Middle Island upheaval of the land is observable in a marked manner through the entire length of the western coast from Cape Farewell to Dusky Bay. Some of the most extraordinary changes in these regions have taken place within the last few years.

This has been confirmed by Dr. Haast, who, however, found some signs of depression at the north-western extremity of the lands. In Australia our evidence is ample:—The north-east, if not the whole of the east coast of Australia, is slowly rising, as proved by the gradual shoaling of the Channel between Hinchinbrook Island and the mainland, due to all appearance neither to silting up nor growth of coral water-worn caves, now well above high-water mark in the sandstone cliffs of Albany Island, and those of the mainland opposite, and in the existence along many parts of the coast, especially towards the north of the peninsula, of extensive tracts of level country now covered with sand dunes, bearing a scanty vegetation, stretching inland 10, 15, and 20 miles off, but which once bordered the sea" (Rattray, Geology of Cape York Peninsula, Australia, *Mine Journal*, vol. xxv. p. 297).

"An immense portion of the continent of Australia is known to be uprising. . . . The whole coast round to a distance of several miles inland is covered with recent shells; the drainage of the country is apparently altering. Lakes known to have been formerly filled with salt water are now filling up with fresh or becoming dry. The lagoons near the coast are filled with salt and brackish water, and their banks are filled with marine shells with their colours in many cases preserved. Reefs of rocks are constantly appearing in places where there were none formerly. At Rivoli Bay the soundings have altered so much as to make a new survey requisite. A reef has lately almost closed this harbour. Other reefs have appeared at Cape Jaffa, &c. It would appear that a vast movement is taking place in the whole of the south of Australia. In Melbourne the observations of surveyors and engineers have all tended to confirm this remarkable fact. In Western Australia the same thing is observed at King George's Sound, the same," &c., &c., and so on, for many pages. (See Wood's Geological Observations in South Australia, 135-207, and *passim.*)

The facts I have enumerated, which might be almost indefinitely multiplied, are sufficient to prove the position that every large mass of land near the South Pole which we can examine shows signs of upheaval, and justifies the conclusion that the circumpolar land is rising at both poles, and that there is a general thrusting out of the earth's periphery in the direction of its shorter axis.

I must modify the opinion expressed in a previous paper that the 57th parallel is the southern limit of upheaval in the northern hemisphere. The limit of upheaval is an irregular line. I believe that the district intervening between the two projecting poles, with its focus along the equator, is an area of subsidence. This conclusion I believe to be of crucial importance in solving both geological and meteorological problems.

H. H. HOWORTH

New Zealand Trees

I HAVE been greatly astonished by the perusal of a paragraph on New Zealand timber trees, which appears on p. 14 of the current volume of NATURE (No. 105, Nov. 2, 1871). Almost all that is said, either directly or inferentially in that paragraph is so grossly inaccurate that I cannot understand how such statements found their way into a periodical like yours. In the first place, the Rimu (*Dacrydium cupressinum*), the Matai (*Podocarpus spicata*), and the Totara (*P. totara*), are spoken of as if peculiar to the North Island, whilst the truth is that they are common to all parts of New Zealand. These trees are never "cut down wholesale" for firewood, except perhaps now and then when bush land is being cleared so far from other settlements that transport of the timber to any market is a physical impossibility. The woods enumerated are, Kauri (*Dammaris australis*), and the white pine (*Podocarpus dacrydioides*), the principal building timbers of the colony. The Rimu is not "valuable for furniture and all ornamental work," although some choice sections of it look well when carefully polished. Totara and Kauri look better when polished, but their brittleness spoils their usefulness for ordinary furniture work. When I deny that these timbers are "valuable" for cabinet work, I mean that they have not, and never will have, the value which attaches to mahogany, rosewood, walnut, and similar woods. That the Rimu, Matai, and Totara "are none of them Coniferæ," is news to botanists on this side the world. All these trees are to be found in horticultural collections in England and Scotland, and it is to be regretted that the writer of this paragraph did not acquaint himself with them before he undertook to instruct others as to their botanical characteristics. But the most amazing of all the statements in this paragraph is that about the Rata (*Metrosideros lucida*). This appears to have been quoted from somewhere. I should very much like to know who is responsible for such a monstrous fiction. I can only conceive that its author has confused the Akakura (*Metrosideros scandens*) with the Rata in his memory—he could never have confused the objects themselves when before his eyes. The whole story of the manner of growth of the Rata is utterly without foundation.

I may take this opportunity of mentioning that the description of *M. lucida* in Hooker's "Handbook of the New Zealand Flora" is inaccurate. The tree is there described as a small one, whereas it grows in the South Island to the dimensions of a large forest tree. Probably Dr. Hooker had to depend on information derived from North Island sources only. W.

Dunedin, N. Z., January 13

Earthquakes in the Philippine Islands

IN the middle of December, 1871, the volcano Albay in the S.E. of Luzon began to play, and threw out smoke, stones, and lava for several weeks.

The following phenomena have also to be recorded :—

1871.—October 8 and 9, at Pollok on Mindanao, sulphurous springs arose in the neighbourhood.

December 8 to 14, at Kottabato on Mindanao, very heavy earthquakes, which destroyed all the houses.

1872.—January 29, at 7 P.M., at Manila, three slight shocks from E. to W., which I witnesed.

Manila, Feb. 5 A. B. MEYER

Height of Auroras

ALLOW me to suggest the following rules, to be attended to by those who incline to make observations on the heights of auroras :—

1. Observations to be made at the exact hours and half hours, Greenwich mean time.

2. If there is an arch, the position of the apex of its central line should be noted with reference to the stars ; or else its altitude should be ascertained carefully, and its azimuth approximately. If the lower or the upper edge of the arch is well defined, give similar particulars respecting it. State the width of the arch ; state whether it is regular or not. If it is somewhat irregular, instead of its actual position, give that of an imaginary arch having its average position.

3. If there is any other very conspicuous feature, its position among the stars may be observed ; care being taken to describe it sufficiently for it to be recognised in any account from another place. But the position of the corona, or point to which the rays converge, is of no value for determining the height of the aurora, for it is merely an apparent phenomenon.

Observers must not consider themselves tied down to observe on every occasion ; *any* observations, if made in accordance with these rules, may be useful. If they are sent to me, I will endeavour to calculate the aurora's height from them, unless some one else volunteers to take them in hand.

T. W. BACKHOUSE

West Hendon House, Sunderland, March 20

Eccentricity of the Earth's Orbit

I SHALL feel obliged if some of your correspondents would inform me if, with the exception of Grant's Physical Astronomy, there is any treatise or encyclopædic article on Astronomy, published in this country before 1864, where the superior limit of the eccentricity of the earth's orbit, as determined by Lagrange or by Leverrier, is given ; or even any reference made to the researches of these geometricians on the subject.

Edinburgh, March 11 JAMES ELLIS

Barometric Depression

IN Mr. Monck's article on barometric variations in NATURE of 21st inst. there is a serious mistake about the theory of trade-winds. He says the trade-winds would probably extend to the poles were it not that the parallels of latitude become so narrow before reaching them. The trade-winds are east winds ; and if, as is certainly the case, the only motive power acting on the earth's atmosphere is the sun's heat, it follows from the law of the conservation of rotation that the total force of the east and west winds must exactly balance each other. This must be the case even were the earth of some other form than a sphere.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, March 25

*FURTHER INVESTIGATIONS ON PLANETARY INFLUENCE UPON SOLAR ACTIVITY**

1. IN a previous communication by us to this Society, an abstract of which was published in the Proceedings, vol. xiv. p. 59,† we showed some grounds for believing that the behaviour of sun-spots with regard to increase and diminution, as they pass across the sun's visible disc, is not altogether of an arbitrary nature. From the information which we then had, we were led to think that during a period of several months sun-spots will, on the whole, attain their minimum of size at the centre of the disc. They will then alter their behaviour so as, on the whole, to diminish during the whole time of their passage across the disc; thirdly, their behaviour will be such that they reach a maximum at the centre; and, lastly, they will be found to increase in size during their whole passage across the disc. These various types of behaviour appear to us always to follow one another in the above order; and in a paper printed for private circulation in 1866, we discussed the matter at considerable length, after having carefully measured the area of each of the groups observed by Carrington, in order to increase the accuracy of our results. In this paper we obtained nineteen or twenty months as the approximate value of the period of recurrence of the same behaviour.

2. A recurrence of this kind is rather a deduction from observations more or less probable than an hypothesis; nevertheless, it appeared to us to connect itself at once with an hypothesis regarding sun-spot activity. "The average size of a spot" (we remarked) "would appear to attain its maximum on that side of the sun which is turned away from Venus, and to have its minimum in the neighbourhood of this planet." In venturing a remark of this nature, we were aware it might be said, "How can a comparatively small body like one of the planets so far away from the sun cause such enormous disturbances on the sun's surface as we know sun-spots to be?" It ought, however, we think, to be borne in mind that in sun-spots we have, *as a matter of fact*, a set of phenomena curiously restricted to certain solar latitudes, within which, however, they vary according to some complicated periodical law, and presenting also periodical variations in their frequency of a strangely complicated nature. Now these phenomena must either be caused by something within the sun's surface, or by something without it. But if we cannot easily imagine bodies so distant as the planets to produce such large effects, we have equal difficulty in imagining anything beneath the sun's surface that could give rise to phenomena of such a complicated periodicity. Nevertheless, as we have remarked, sun-spots do exist, and obey complicated laws, whether they be caused by something within or something without the sun. Under these circumstances, it does not appear to us unphilosophical to see whether as a matter of fact the behaviour of sun-spots has any reference to planetary positions. There likewise appears to be this advantage in establishing a connection of any kind between the behaviour of sun-spots and the positions of some one prominent planet, that we at once expect a similar result in the case of another planet of nearly equal prominence, and are thus led to use our idea as a working hypothesis.

3. We have now a larger number of observations at our disposal than we had in 1866. We had then only the groups observed by Carrington, the positions and areas of all of which we had accurately measured. We have now in addition five years of the Kew observations, for each group of which the positions and areas have been recorded by us in our previous communications to this society. We have thus altogether observations extending from the beginning of 1854 to the end of 1860, forming the series of Carrington; and observations extending from the beginning of 1866, forming the Kew series, as far as this is yet reduced. We have, in fact, altogether a nearly continuous series, beginning a year or two before one minimum, and extending to the next, and thus embracing rather more than a whole period.

We propose in the following pages to discuss the behaviour with regard to size of the various groups of these two series, as each group passes from left to right across the sun's visible disc. Unfortunately for this purpose, a large number of groups has to be rejected; for, on account of bad weather, we have frequent blank days, during which the sun cannot be seen, and on this account we cannot tell with sufficient accuracy the behaviour of many groups as they pass across the disc. In our catalogue of sun-spot behaviour, we have only retained those groups for which, making the times abscissæ, and the areas ordinates, we had sufficiently frequent observations to enable us to construct a reasonably accurate curve exhibiting the area of the group for each point of its passage across the disc. From these curves a table was then formed denoting the probable area of each non-rejected group at the following heliographic longitudes (that of the visible centre of the disc being reckoned as zero):—

$$-63^\circ - 49^\circ - 35^\circ - 21^\circ - 7^\circ + 7^\circ + 21^\circ + 35^\circ + 49^\circ + 63^\circ;$$

in fact, giving the area of the group for the ten central days of its progress, and rejecting those observations that were too near the sun's border on either side, on account of the uncertainty of measurement of such observations. We have succeeded in tabulating in this manner 421 groups of Carrington's series, and 373 groups of the Kew series up to the end of 1866, in all 794 groups. In this catalogue the area is that of the whole spot, including umbra and penumbra; and in measuring these areas a correction for foreshortening has always been made, as described in a paper which we presented to this society, and which constitutes the first series of our researches. These areas are expressed in millionths of the sun's visible hemisphere.

4. When we began this present investigation into the behaviour of spots, we soon found reason to conclude that in the case of sun-spots the usual formula for foreshortening is not strictly correct. Perhaps if a sun-spot were strictly a surface-phenomenon, the usual formula might be correct, though even that is doubtful; for the earth as a planet may not impossibly affect the behaviour of all spots as they cross the disc, so as to render the formula somewhat inexact. However this may be, a spot is probably always surrounded more or less by faculous matter, forming in many cases a sort of cylindrical wall round the spot. Now the effect of such a wall would be to allow the whole spot to be seen when at or near the centre of the disc, but to hide part of the spot as it approached the border on either side. A spot thus affected would therefore appear to be more diminished by foreshortening than the usual formula would indicate; and we should therefore expect, if this were the case, that, on the whole, after making the usual allowance for foreshortening, spots would nevertheless be found deficient in area near the borders as compared with their area at the centre of the disc. As a matter of fact we have something of this kind, as will be seen from the following table, in which we have used the whole body of spots forming the catalogue to which we have made allusion.

In this table the first column denotes the heliocentric longitude from the centre of the disc reckoned as zero; the second denotes the united areas at the various longitudes of all those groups from both series, the behaviour

* By Warren De La Rue, D.C.L., F.R.S., Balfour Stewart, LL.D., F.R.S., and Benjamin Loewy, F.R.A.S. Read before the Royal Society, March 14, 1872.

† See NATURE, vol. v., p. 192.

of which we have been able to obtain with accuracy; while the third column exhibits the residual factor for foreshortening, which will bring the areas of the second column into equality with each other.

TABLE I.

Longitude observed.	United areas of all groups at longitude of column 1.	Residual factor for foreshortening necessary to equalise the areas of column 2.
−63°	147,508	1·229
−49	156,758	1·156
−35	168,697	1·075
−21	176,417	1·028
− 7	178,990	1·013
+ 7	181,336	1·000
+21	178,638	1·015
+35	175,747	1·032
+49	171,140	1·059
+63	162,541	1·115

5. From the above table it appears that the average behaviour of spots, as far as can be judged from the information at present attainable, is not quite symmetrical as regards the centre of the disc. Without attempting at present to enter into an explanation of this remarkable phenomenon, we may point to it as a confirmation of our view previously stated, that most spots are accompanied by a wall-shaped surrounding of facula. Observations show that on the whole the life-history of the facula begins and ends earlier than that of the spot which it surrounds, and that throughout a gradual subsidence of this elevated mural appendage seems to be taking place. But such a diminution of the wall discloses more of the spot itself, and hence the spot-areas, measured on the eastern half of the hemisphere, might be expected, *cæteris paribus*, to be smaller than those observed in the western half, a fact strikingly demonstrated by the above table.

Our present object, however, is not to account for the average behaviour of spots, but rather to investigate the causes or concomitants of a departure from this average behaviour. We have, therefore, in all cases made use of the factors given in the above table as those which, judging by the average behaviour, tend to equalise the areas that pass the various longitudes. We have called this *earth-correction*, and have limited our discussion to any well-marked behaviour that remains after the earth-correction has been applied.

Let us now divide the whole mass of observations into four portions, depending upon the position of the planet Venus with reference to the earth or point of view. First, let us take each occasion on which the planet is in the same heliographic longitude as the earth, that is to say, when the earth and Venus are nearly in a line on the same side of the sun.

Let us use five months' observations for each such occasion, extending equally on both sides of it; thus, for instance, if the planet Venus and the earth had the same heliocentric longitude on September 30, 1855, we should make use of sun-spots from the middle of July to the middle of December of that year as likely to represent any behaviour that might be due to this particular position of Venus. Let us do the same for all similar occasions, and finally add all the spots thus selected together. We have thus obtained a mass of observations which may be supposed to represent any behaviour due to this position of the planet Venus with reference to the earth or point of view.

Secondly, let us now take each occasion on which Venus is at the same longitude as the extreme right of the visible disc, that is to say, 90° before the earth, and do the same as we did in the previous instance, using five months' observations for each occasion We shall thus, as before, obtain a mass of observations which may be supposed to represent the behaviour due to a position of Venus 90° before the earth. Thirdly, let us obtain in a similar manner a mass of observations representing the behaviour of sun-spots for a position of Venus 180° before the earth, Venus and the earth being now at exactly opposite sides of the sun; and fourthly, let us finally obtain, in a similar manner, those observations representing the behaviour of sun-spots when Venus is 270° before the earth, being now of the same heliocentric longitude as the extreme left of the visible disc.

These four series of five months each will in fact split up the whole body of observations into four equal parts, the synodical revolution of Venus being nearly twenty months. The following table exhibits these series after the earth-correction has been applied to each. It also represents each series reduced so as to exhibit its characteristic behaviour for an average size of spot = 1000.

TABLE II.

Longitude.	Sum of areas corrected for earth-effect.							
	(A) Venus=Earth+0°.		(B) Venus=Earth+90°.		(C) Venus=Earth+180°.		(D) Venus=Earth+270°.	
		1000		1000		1000		1000
−63°	48905	+54	60573	+56	44031	−16	27776	−152
−49	48385	+42	59869	+43	44075	−15	28881	−118
−35	47508	+23	60210	+49	43606	−25	30023	− 84
−21	46203	− 4	59847	+43	43974	−17	31331	− 44
− 7	45026	−30	58493	+20	45084	+ 7	32711	− 1
+ 7	43603	−61	56496	−15	47446	+61	33791	+ 31
+21	44134	−49	54867	−44	47768	+68	34547	+ 55
+35	45306	−25	54184	−55	46821	+47	35068	+ 71
+49	46476	+ 1	54782	−46	43693	−23	36285	+107
+63	48742	+49	54473	−51	40875	−87	37143	+135
	464288	10000	573794	10000	447373	10000	327556	10000

7. We may do the same for the planet Mercury as we have done for Venus, that is to say, we may split up the whole body of observations into four parts, representing the behaviour of sun-spots when Mercury is in the same four positions with respect to the earth as those which are given for Venus in the above table. Only in this case we must bear in mind that, owing to the eccentricity of Mercury's orbit, this planet will sometimes take a longer,

and sometimes a shorter time to go from one configuration to another. Thus, for instance, we have

Mercury = earth + 0° on March 24, 1854;
Mercury = earth + 90° on May 6, 1854;
and Mercury = earth + 180° on May 29, 1854.

We should therefore take the observations between April 15, 1854, and May 18, 1854, as representing the behaviour of sun-spots due to a position of Mercury 90° before the earth, and so on for other cases. The following table has been constructed on this principle, and it may be regarded as exhibiting for Mercury precisely what the second table exhibited for Venus.

TABLE III.

Longitude.	Sum of areas corrected for earth-effect.							
	(A) Mercury=Earth+0°		(B) Mercury=Earth+90°.		(C) Mercury=Earth+180°.		(D) Mercury=Earth+270°.	
°		1000		1000		1000		1000
−63	45298	+22	45555	+85	39034	−84	50409	+ 0
−49	45492	+26	44183	+52	40288	−54	49868	−10
−35	45978	+36	41723	− 7	42303	− 8	48996	−28
−21	43870	−11	41398	−14	44554	+46	48453	−39
− 7	42568	−40	41386	−15	45266	+62	48817	−31
+ 7	42384	−44	41096	−21	45502	+68	49844	−11
+21	42885	−33	41460	−13	44817	+52	51341	+18
+35	44270	− 2	40649	−31	42740	+ 3	53000	+51
+49	45780	+32	40337	−39	41478	−27	51772	+27
+63	44922	+14	42157	+ 3	40122	−58	51562	+23
	443447	10000	419944	10000	426104	10000	504062	10000

8. The following is a table constructed on a precisely similar principle with reference to the planet Jupiter:—

TABLE IV.

Longitude.	Sum of areas corrected for earth-effect.							
	(A) Jupiter=Earth+0°.		(B) Jupiter=Earth+90°.		(C) Jupiter=Earth+180°.		(D) Jupiter=Earth+270°.	
°		1000		1000		1000		1000
−63	29348	− 34	35369	−20	48871	−25	42794	+39
−49	28665	− 57	35256	−24	50118	− 1	43163	+48
−35	28836	− 51	35176	−25	51432	+26	40747	−11
−21	28623	− 57	34962	−32	51029	+18	41318	+ 3
− 7	28779	− 53	35739	− 9	51116	+20	40500	−17
+ 7	30321	− 1	36494	+11	50360	+ 9	40599	−15
+21	31309	+ 31	37264	+32	50266	+ 3	40979	− 5
+35	31488	+ 36	36935	+24	50489	+ 7	41579	+ 9
+49	32400	+ 67	36584	+13	49558	−11	40876	− 7
+63	34017	+119	37147	+30	47792	−46	39373	−44
	303786	10000	360926	10000	501231	10000	411928	10000

9. If we now examine the two tables for the planets Venus and Mercury, we shall find in them indications of a behaviour of sun-spots appearing to have reference to the positions of these planets, and which seems to be of the same nature for both. This behaviour may be characterised as follows:—the average size of a spot would appear to attain its maximum on that side of the sun which is turned away from Venus or from Mercury, and to have its minimum in the neighbourhood of Venus or of Mercury.

10. The apparent behaviour is so decided with regard to Venus, that the whole body of observations will bear to be split up into two parts, namely, Carrington's series and the Kew series, in each of which it is distinctly manifest. The following treatment will serve to render this effect more visible to the eye.

In Table II., column (A) (Venus = earth + 0°), we have ten final numbers denoting the behaviour of a spot of average area = 1,000 at ten central longitudes as follows:

+ 54 + 42 + 23 − 4 − 30 − 61 − 49 − 25 + 1 + 49.

Let us take the mean of the first and second of these, the mean of the second and third, and so on, and we get the following nine numbers:—

+ 48 + 32 + 10 − 17 − 45 − 55 − 37 − 12 + 25.

Performing the same operation once more, we obtain the following eight numbers, corresponding to the eight central longitudes:—

+ 40 + 21 − 3 − 31 − 50 − 46 − 25 + 7.

In Table V. we have exhibited the results obtained by this process.

11. If we now refer to the table of Jupiter, we find that we cannot detect the same kind of behaviour that we did in the case of Venus and Mercury. We cannot say that such a behaviour does not exist with reference to this planet; but, if it does, it is to such an extent that the observations at our disposal have not enabled us to detect it.

12. The following evidence from a different point of view goes to confirm the results we have now obtained.

We might expect, if there really is a behaviour of sun-spots depending upon the position of Venus, and of the nature herein stated, that the average area of a spot as it passes the central longitude of the disc ought to be greatest when Venus is 180° from the earth, and least when Venus and the earth are together, and the same ought to hold for Mercury and for Jupiter, if these planets have any influence. Taking the mean of the four central

TABLE V.

Longitude.	Venus (whole series).				Venus (Carrington's series).				Venus (Kew series).				Mercury (whole series).			
	(A)	(B)	(C)	(D)	(A)	(B)	(C)	(D)	(A)	(B)	(C)	(D)	(A)	(B)	(C)	(D)
−49°	+40	+48	−18	118	+8	+30	−10	−160	+117	+58	−27	−46	+28	+45	−50	−12
−35	+21	+46	−20	−82	+9	+24	−5	−95	+47	+58	−39	−59	+21	+6	−6	−26
−21	−3	+39	−13	−43	+1	+24	+10	−37	−16	+45	−38	−52	−6	−12	+36	−34
−7	−31	+17	+15	−3	−12	+16	+36	+16	−74	+13	−9	−36	−33	−16	+60	−28
+7	−50	−14	+49	+29	−23	+2	+53	+58	−113	−29	+45	−20	−40	−18	+63	−9
+21	−46	−40	+60	+53	−15	−20	+46	+82	−119	−57	+77	+4	−28	−20	+43	+19
+35	−25	−50	+34	+76	+4	−45	+13	+100	−91	−56	+59	+36	−1	−28	+7	+36
+49	+7	−50	−22	+105	+14	−50	−40	+118	−9	−44	+1	+82	+19	−27	−27	+32

areas as giving the best value of the araea of a spot its passes the centre, we have for Venus the following results :—

Mean of 4 central areas,

(A)	(B)	(C)	(D)
44741	57426	46068	33095

and the number of groups for these are as follows :—

229	265	150	181

hence the mean area of one group will be,—

195	217	307	183

from which we get (A) = 195 ; mean of (B) and (D) = 200 ; (C) = 307 ; that is to say, A is least, and C is greatest.

Doing the same in the case of Mercury, we get

(A) = 204 ; mean of (B) and (D) = 217 ; (C) = 246 ;

and finally, doing the same in the case of Jupiter, we get

(A) = 185 ; mean of (B) and (D) = 207 ; (C) = 282 ;

it thus appears that in all these cases the same order is preserved.

13. We leave it to others to remark upon the nature and strength of the evidence now deduced as to a connection of some sort between the behaviour of sun-spots and the positions of the planets Venus and Mercury. We think, however, it must be allowed, that the investigation is one of interest and importance, and we trust that arrangements may be made for the systematic continuance of solar observations in such localities as will ensure to us a daily picture of the sun's disc.

The influence of blank days in diminishing the value of a series of sun-observations is very manifest. We have been able to record the behaviour across the sun's disc of 421 groups of Carrington's series for a total number of 885 groups, and we have been able to record the same behaviour for 373 out of 544 groups observed at Kew. Thus, out of a total of 1,429 groups, we have only been able to record the behaviour of 794. Nor are the records which we have obtained so perfect as we could wish, on account of blank days, which make interpolations necessary. It is therefore of much importance for the future of such researches as the present that there should be several observing-stations so placed that we may reckon on having at least a daily picture of the sun's disc.

It will be easily seen that such observations are very different from experiments which may be multiplied *ad libitum* ; for in this case Nature gives us in a year or in ten years a certain amount of information, and no more ; while it depends upon ourselves to make a good use of the information which she affords.

It is already universally acknowledged that we ought to make the best possible use of the few precious moments of a total eclipse ; but such observations must necessarily be incomplete unless they are followed up by the equally important if more laborious task of recording the sun's surface from day to day.

RHINOCEROSES

THE few species of Rhinoceros which now exist on the world's surface are divisible into two distinct groups, one of which inhabits Africa, the other certain portions of Asia. The Asiatic rhinoceroses are readily distinguishable from their Æthiopian brethren by the presence of incisor teeth throughout life, and by the remarkable folds of the skin. In the African rhinoceroses the incisor teeth are absent, or rather never cut the gums, and the skin is smooth, or, at all events presents scarcely any appearance of the peculiar folds which distinguish the Asiatic species.

Commencing with the Asiatic group, the great Indian rhinoceros (*Rhinoceros unicornis*) is the largest, oldest, and best known species. Of this animal the Zoological Society's Collection contains two adult specimens—a female, purchased in 1850, and a male, presented by Mr. Grote in 1864. But long before the arrival of these animals the large Indian rhinoceros was represented in the Society's Collection by a specimen which died in 1849, and which formed one of the subjects of Prof. Owen's elaborate memoir upon the anatomy of this animal, published in the Society's "Transactions," vol. iv., p. 31.

The habitat of the large Indian rhinoceros is the wooded district called the Terai, which lies along the foot of the Himalayas from Nepaul to Bhotan, and thence extends into Assam.

The Sondaic rhinoceros (*Rhinoceros sondaicus*) appears to be very like its larger brother in general conformation, having but one horn on its nose, and the same complicated folds of the skin. It is, however, much smaller in size, and, according to the best authorities, presents certain well-marked cranial characters, which render it easily distinguishable. This rhinoceros was, until recently, supposed to be confined to Java, Sumatra, and Borneo, in which latter island, however, its existence in the present epoch is somewhat problematical.*

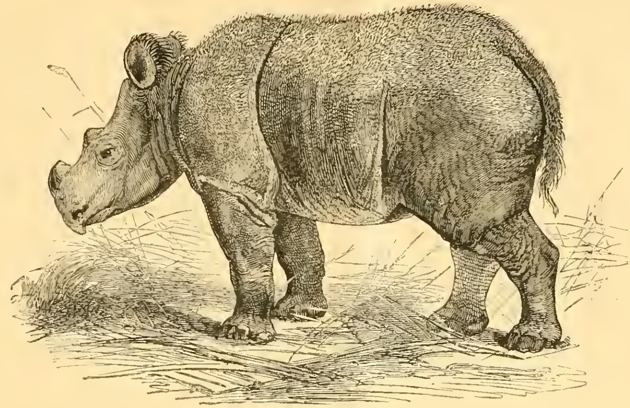
Mr. Blyth, however, has recently shown that the one-horned rhinoceros of the Malay peninsula is in all probability referable to this species, and that the rhinoceros which occurs in the Sunderbunds of Bengal is most likely the same animal.

Of the Sondaic rhinoceros, the Zoological Society has

* See Busk in Proc. Zool. Soc. 1869, p. 409, and Fraser, *ibid.*, p. 29.

not yet succeeded in obtaining a specimen, and I am not aware that the animal has ever been brought alive to Europe. It would be of great interest to place a living example of this species by the side of its larger ally in the Regent's Park.

The third Asiatic species of rhinoceros is a very different looking animal from the two previously mentioned, having two horns on its forehead, the smaller of which is situated just above the eye, and the other still farther forward. Its body is, moreover, covered with bristly hairs, and there is only one strong, well-marked cutaneous fold of skin on the back, which renders it very unlike its mailed brethren. This animal was until lately supposed to be only found in the Island of Sumatra. Cuvier called it *Rhinoceros sumatrensis* from this circumstance; and our countryman, Sir Stamford Raffles, who obtained it in that island about the same period, likewise proposed to name it after the country to which he believed it to be confined. It has, however, been recently discovered that the Sumatran rhinoceros extends northwards along the whole range of the Malay peninsula, at least as far as Chittagong. The fine female specimen of this rhinoceros now in the Gardens of the Zoological Society of London was captured a little way south of Chittagong about four years ago. At the time of its capture, it is said to have been quite young, perhaps two years old. Now, however, it is about four and a half feet high, and has probably nearly attained its adult stature; this species being the



SUMATRAN RHINOCEROS

AFRICAN BLACK RHINOCEROS

smallest of existing rhinoceroses. Singularly enough, at the time this animal was on its way to England, a second specimen of the same species was received by the Zoological Society of Hamburg, and is now living in their gardens in that city. The Hamburg animal is likewise a female, and is said by those who have examined both individuals, to agree in nearly every particular with that belonging to the Zoological Society of London, but to be about one-third smaller.

It must be observed, that although the Sumatran rhinoceros has two horns, it is by no means nearly related to the African two-horned rhinoceros, but has the incisor teeth and other cranial characters of the Indian division of the group.

Of the African rhinoceroses, which constitute the second division of the genus as explained above, many nominal species have been made by naturalists who delight in conferring names upon fragments of horns, and imperfect skulls; but we have not as yet certain evidence of the existence of more than two species, commonly known as the Black rhinoceros and the White rhinoceros.

The Black rhinoceros (*Rhinoceros bicornis* of Linnæus) has its upper lip long and prehensile. This organ, in fact, forms almost a short proboscis, well fitted for grasping the small branches of trees, upon which it principally subsists. The two horns are not very different in size and length, although the front one is usually longest. The Black rhinoceros is found in Eastern Africa, as well as in the interior of the Cape Colony. In his well-known work on the Nile tributaries of Abyssinia, Sir Samuel Baker describes it as being not unfrequently met with in Upper Nubia. The young male example of this animal obtained by the Zoological Society in September 1868, was captured in this district by the Hamram Arabs, of whose prowess Sir Samuel Baker tells us such wonderful stories. A living example of the African Black rhinoceros has been since added to the collection of the Zoological Society of Berlin; but these two specimens are, we believe, the only individuals of this species that have been brought to Europe, since the days when rhinoceroses were exhibited and slain in the Roman amphitheatres.

The White African rhinoceros is immediately distinguishable from its black brother, apart from the difference in the colour of its skin, by its short upper lip, whence Dr. Burchell, the first scientific traveller who met with it, proposed to call it *Rhinoceros simus*. It is a grazing animal, feeding chiefly upon grass, and inhabits more open districts than *R. bicornis*. But the most noticeable distinction of the White rhinoceros is the enormous length of the front horn, which in old individuals reaches to three and a half, or even four feet in length, and, after sloping forwards, curves gently backwards towards the summit. The hinder horn, on the contrary, always remains small, and slightly developed. The range of the White rhinoceros in Africa is not very perfectly known. From the inner parts of the Cape Colony it extends probably on to the Zambesi and its affluents. How much farther northwards it may go is uncertain; but, according to Sir Samuel Baker, it is not known in Upper Nubia, where the Black rhinoceros is the only species met with.

No specimen of the African White rhinoceros has yet been brought to Europe, and few additions could be made to the collection of the Zoological Society of London, which would be more acceptable than a young male of this rare and curious animal. P. L. S.

SCIENCE IN THE NAVY

It is with great satisfaction that we learn, from a speech made by Mr. Goschen in the House of Commons last week, that the Government proposes a vote of 2,000*l*. to Mr. Archibald Smith, Q.C., for great services rendered by him to the Admiralty, not in his professional capacity, but as a man of science whose researches into matters connected with magnetism had been of great service to the Navy and the country. This grant was not proposed as a compensation for Mr. Smith's very laborious services, but as a small mark of the high appreciation the Government had of his eminent scientific services. There was another increase proposed also in aid of the expedition about to be organised under the auspices of the Royal Society to make researches into the depth, temperature, composition, circulation, and distribution of animal life in the Atlantic, Indian, and Pacific Oceans. The total cost to the country, supposing the inquiry to extend over two and a half years, would be about 25,000*l*., a sum which would not be grudgingly paid in order to secure a vast amount of important scientific knowledge.

The following announcement, with respect to the education of naval officers, will be welcomed with great satisfaction by the scientific public generally:—

"It was proposed that cadets should first go for two years to a Naval College, to master some of the rudiments of their profession, cruisers being attached, so that they might begin to go to sea. At the expiration of or within twelve months they would go out in a seagoing man-of-war, with naval instructors, when they would have for three years a much better education than they now obtain, the same amount of sailoring experience being retained. It would then be desirable that they should have six months' teaching preliminary to their examination, when many young officers would ascertain which way their bent lay, and whether they should apply themselves to higher courses of study, for which arrangements could be made, but which would not be entered upon till they had passed the lieutenant's examination. . . The question that the Government had before them in reference to this subject was how to unite in one establishment all the various branches of naval study which were at present taught in the Royal Naval College at Portsmouth, and in the Naval School of Architecture at South Kensington. At present the Royal Naval College conducted their examinations themselves—that is to say, they first taught and then examined, which was not at all a desirable state of things. It was now proposed to combine the scheme which he had described as regarded the education of the young officers with one for the education of the commissioned officer, and also to make better arrangements for the education of the Engineer and Marine officers. In order to carry out these objects it was proposed to found a Royal Naval College at Greenwich, where all branches of a general naval education would be taught, and to do so upon a scale which would be calculated greatly to raise the tone of our naval officers. In the first place there would be received in the College sub-lieutenants, who would be kept there for six months before their passing their general examination, and also naval officers. It was proposed that after the sub-lieutenants had passed their examinations and had been a short time at sea, those who chose to avail themselves of it should have an opportunity accorded to them of pursuing a higher course of study, of which half-pay officers might also avail themselves, and the establishment being so near London they should be able to offer a better course of study, under more able professors, than would be possible to give at Portsmouth. But, in addition to thus offering an education of this description to the young and to the commissioned officers who now went to Portsmouth, they trusted to be able to make arrangements with regard to the education of Engineer officers. At present these latter officers were all brought up in our own yards, which they entered at about fifteen or sixteen years of age, and in which they remained for four or six years as Engineering apprentices, and at the end of the fourth year three were selected to go to the School of Naval

Architecture at South Kensington. In the same way, from a certain number of shipwrights' apprentices three or four were also selected every year to go up and study at the latter school. As regarded the Engineers, it was proposed that not merely three or four out of the thirty should be sent every year to South Kensington, but that all of them, after having been four years in the yards, should have the advantage of going through a course of one year's education at Greenwich, which should include all the higher branches of engineering education, such as metallurgy and chemistry. It was further proposed to take a similar course with reference to the shipwright's apprentices, but only as regarded a limited number, who would have an opportunity of studying naval architecture at Greenwich. The South Kensington School would be removed to Greenwich. . . With regard to the cadets, it was not proposed that they should go to Greenwich. No definite arrangements had as yet been proposed with reference to them, but that there was no great hurry in the matter, because in future they would not be taken under fifteen years of age, and it would be as well to wait until those who had entered at thirteen had attained the latter age before new arrangements were entered into with regard to them."

We heartily congratulate the Government on this commencement of a higher scientific instruction of officers of the Navy, and trust that the course thus commenced will be persisted in.

NOTES

THE Royal Commission on Scientific Instruction and the Advancement of Science have, we are informed, concluded their inquiry into the scientific instruction afforded in training colleges and elementary schools, and in the science classes of the Science and Art Department.

THERE will be an election to a Natural Science Fellowship in Exeter College, Oxford, on Wednesday, June 19. The examination will be in Biology. The Fellow elected will be required to reside and take part in the instruction of the College. The election will take place under the conditions of the special ordinance of the College with regard to residence. The Fellow elected under the ordinance will be subject in all other respects to the Statutes of the College. The examination will probably begin on Tuesday, June 11, and no person can be admitted as a candidate who has not passed all the examinations necessary for the degree of Bachelor of Arts in the University of Oxford, or been incorporated as a graduate in the University. Candidates are requested to make application by letter to the rector on or before June 1.

THE examinations for Scholarships in Natural Science, which have recently been held at Clare and at Emmanuel College, Cambridge, have both terminated without an election being made. The reason of this is that at neither of the colleges did candidates present themselves, whose attainments, in the opinion of the examiners, entitled them to receive the distinction. The number of competitors was but small in each case, in one three only.

THE Vice-Chancellor of the University of Cambridge has promulgated the text of a memorial addressed to the University upon the subject of higher education, and adopted at a public meeting at Birmingham. It is similar to the memorials addressed upon the same subject from Rochdale, Leeds, Crewe, and the North of England Council for the Education of Women, and the prayer of the memorial is that a Syndicate be appointed to investigate the subject, and to inaugurate such means as would produce—firstly, a standard of excellence in the departments of literature, science, and art, fixed by some universally recognised authority, and attainable by students of this class, which would secure for their studies the definiteness and thoroughness that are so much needed; secondly, an opportunity, offered to all who might be inclined to take advantage of it, of bringing their acquirements to the test of an examination; thirdly, the command of teaching power of a high order for the benefit of those who might wish to place themselves under instruction.

PROF. HUXLEY is, we learn from the *Times*, the favourite candidate for the rectorship of St. Andrew's University.

THE following are the probable arrangements for the Friday evening meetings at the Royal Institution after Easter:—April 12, Mr. John Morley, "On Rousseau's Influence on European Thought;" April 19, Mr. Vernon Harcourt, F.R.S., "On the Sulphurous Impurity in Coal Gas and the means of removing it;" April 26, Prof. Blackie, "On the Genius and Character of the Modern Greek Language." May 3, Wm. Spottiswoode, Treas. R.S.; May 10, N. Story-Maskelyne, F.R.S., "On Meteoric Stones;" May 17, Prof. Abel, F.R.S.; May 24, Prof. Clifford, "On Babbage's Calculating Machines;" May 31, Mr. E. J. Poynter, A.R.A. June 7, Prof. Odling, F.R.S. And the following lecture arrangements are announced:—Dr. Wm. A. Guy, F.R.S., three lectures, "On Statistics, Social Science, and Political Economy," on Tuesdays, April 9, 16, and 23; Mr. Edward B. Tylor, F.R.S., six lectures, "On the Development of Belief and Custom amongst the Lower Races of Mankind," on Tuesdays, April 30 to June 4;" Prof. Tyndall, F.R.S., nine lectures, "On Heat and Light," on Thursdays, April 11 to June 6; Mr. R. A. Proctor, five lectures, "On the Star Depths," on Saturdays, April 13 to May 11; Prof. Roscoe, F.R.S., four lectures, "On the Chemical Action of Light," on Saturdays, May 18 to June 8.

PROF. THISELTON DYER is about to deliver a course of lectures on flowers and fruits to the Royal Horticultural Society, with the following titles:—Thursday, April 11, "Flowers: their common plan of construction." April 25, Flowers: the variety in their forms, and how brought about." May 9, "Flowers: their colours and odours." May 23, "Fruits: their structure." June 6, "How seeds are sown in Nature." June 20, "Flowers and Fruits under cultivation." The lectures will commence at 3 P.M.

M. SCHIMPER, the celebrated botanist and palæontologist, is the only one of the old professors in the French University of Strasburg who has consented to continue to hold his post under the German rule. M. Schimper is a Frenchman by birth and descent, and had been offered a superior position elsewhere by the French Government.

M. PRILLIEUX, the French botanist, having declined to continue an honorary Associate of the Leipsic Leopold Academy of Natural Science, some German professors call upon their countrymen to return the "brevets" they have received from French scientific bodies. But it is satisfactory to see Dr. Virchow coming forward to warn his colleagues against imitating such a bad example.

AN ingenious patent is now being worked, by which leather for the sides of boots and shoes is rendered impervious to wet and damp by exhausting the air from the pores of the leather, and filling them up with a substance which unites with and adheres to the fibre, thereby strengthening without impairing the elasticity of the material. It is stated that the patent, known as "Fanshawe's Waterproof Leather," is not only likely to be largely employed for the purpose to which we have referred, but that when asphalte pavement becomes more general, it will be possible to shoe horses with a material as hard as the asphalte itself, and which will prevent them slipping.

A NOVEL and most interesting experiment in the field of elementary instruction has just been resolved upon in Saxony. Hitherto, as everywhere else, so in that small but highly-developed kingdom, the youth of the lower orders, upon being apprenticed to a trade, have been left at liberty to forget the little they learnt at school. Attendance at Sunday schools and evening instruction provided by the State and charitable societies were perfectly optional. By a law just passed this liberty is abridged, and compulsory attendance at evening schools exacted for a period of three years. This is, we believe, the first time in the annals of the world that an attempt has been made by a State to extend the education of the humbler classes beyond the merest rudiments, and after they have entered upon the business of life. Saxony, already the best taught portion of Germany, will by the new law be more than ever in advance of her sister States.

IT has been necessary to remove the Parliamentary copies of the Imperial standards, in consequence of the wall of the Palace at Westminster, in which they were immured, having been pulled down in order to form an entrance to the refreshment rooms. On the 7th of March, 1872, in the presence of the President of the Board of Trade and five other public functionaries, the standards were deposited in their new resting place in the wall on the right-hand side of the second landing of the public staircase, leading from the lower waiting-hall up to the Commons' Committee Room. One alteration has been made. When the standards were originally immured, a brass plate was fixed upon the wall bearing the following inscription in old English letters:—"Within this wall are deposited standards of the British yard and the British pound weight, 1853." The word "measure" has now been inserted after "yard."

IN another column will be found an article on the recent meeting of the Iron and Steel Institute, referring, among other matters of interest, to the new puddling machine. We learn that an agreement has been entered into between Mr. Danks, the inventor, and a combination of iron manufacturers representing the different districts, whereby the latter undertake to have 200 furnaces on his plan put up within six months, and in consideration of his permission to do so, to pay him 50,000*l*. at that time, whether the furnaces are in operation or not. This step, which adds something like 450 furnaces to our producing power, will effect such a revolution as has never before occurred in the history of this branch of industry, and it is the more to be wondered at when it is remembered that, till July last, it was thought that hand-puddling must for ever continue, every machine to do away with it having, before that, entirely failed.

THE *Swiss Times* says that the late Professor Pictet-de-la-Rive has left the whole of his remarkable palæontological collection to the Museum of Natural History, and the greater part of his valuable scientific library to be divided between the Academic Museum and the Library of the city of Geneva.

WE learn with regret of the death, at his plantation, not far from Vera Cruz, of Dr. Charles Sartorius, a gentleman well known in the United States and Europe as a naturalist and meteorologist. Few students of the zoology and botany of Mexico have failed to become acquainted with the labours of the doctor, as shown by the numerous specimens sent to the museum of the Smithsonian Institution and to the American Entomological Society, &c.

THE French Minister of Agriculture and Commerce has ordered the institution at the Central School of Arts and Manufactures at Paris of a new course of lectures to be devoted to the higher teaching of agriculture.

THE Annual Meeting of French *savants*, held at Paris under the auspices of the Ministry of Public Instruction, will commence on Monday, April 1, at the Sorbonne, and continue on the three following days.

WE regret to learn that the proposed Dredging Expedition of the *Norna* is postponed, one of the party being seriously ill, and her owner somewhat unexpectedly having to join his regiment in May instead of autumn. He is anxious to employ a vessel large enough to carry a good stock of fuel for a donkey engine, to save time and labour, and the *Norna* being small for this, and for carrying a steam launch, as is also to be desired, Mr. Marshall-Hall will, in all probability, part with her. If he is successful in organising the more extensive undertaking now proposed, he fully expects to contribute very interesting observations to marine science, and to investigate several chemical questions, besides the zoological work.

PROF. PEPPER, who has done good service in working some of the more popular and easily-illustrated departments of science at the Polytechnic, is about to leave that Institution, and to start an exhibition on his own account at the Egyptian Hall, Piccadilly, in conjunction with Mr. T. W. Tobin.

PROF. LUTHER has discovered a new planet (No. 118) of the 11th magnitude. The discoverer suggests the name "Peitho."

AT the meeting of the Royal Geographical Society, held on Monday evening last, Mr. W. Giffard Palgrave read a paper, detailing a journey made by him during his late residence as Consul in Asia Minor. He began by giving a rough account of the general divisions of that region, confining himself more particularly, however, to that tract of country consisting of tableland, formerly known as Armenian, and where, moreover, the Tigris, Euphrates, and other important rivers take their rise. Many observations made of phenomena in the neighbourhood of the range of mountains known as the Kolat Tagh all tended to show beyond doubt that at some period or other glaciers must have formed and existed in large quantities in the immense tracts of mountains, though at the present time the climate is too genial to allow the snow to remain even on the ridges and peak throughout the year. A short insight was afforded into the volcanic features of the place, and also into the mineralogical formation of the soil. From this it appeared that mines, if only persons were found enterprising enough to work them, might be opened which would yield a surprising amount of lead and a considerable quantity of silver, and would most likely prove very lucrative.

THE educational importance of our large schools, not only to their actual pupils but to the inhabitants of the surrounding neighbourhood, is being happily illustrated at Taunton. An able lecture on "The Theory of Musical Tone," was delivered last week to a large audience in the College School Dining-hall by Mr. E. B. Tylor, F.R.S. It was largely illustrated by experiments, and the valuable apparatus was left behind him by the lecturer as a present to the School. It is hoped that other lectures on Science, Art, and Literature, will succeed; and that gentlemen of eminence will be found to aid, by their presence and teaching, so praiseworthy an attempt. For some time past the Botanical Lectures at the School have been attended not only by the pupils, but by a considerable number of strangers; and a class of forty students will present themselves for the approaching South Kensington Examination in Botany. There is already a small Botanical Garden, well furnished and laid out, which will be largely increased when the funds of the School permit. *O, si sic omnes!*

MR. FAIRGRIEVE, successor to Mr. George Wombwell, is about to sell by auction his well-known menagerie. The catalogue comprises 186 lots, and includes a large number of monkeys, ten lions and lionesses of various ages, a tiger and tigress, a male and female elephant, three boa constrictors, and a large number of other animals, and appurtenances. The sale will take place at Edinburgh, and will commence on April 9, unless the whole menagerie is previously disposed of by private contract.

GENTLEMEN interested in the improvement of Geometrical Teaching may obtain a copy of the Association's Second Annual

Report (referred to in last week's NATURE) on application to the Hon. Secretaries, King Edward's School, Birmingham, or to the London Local Secretaries, Mr. C. W. Merrifield, F.R.S., South Kensington, and Mr. R. Tucker, University College School.

MR. I. LOWTHIAN BELL read a paper at the Institution of Civil Engineers on Tuesday evening, March 18, "On the conditions which favour and those which limit the Economy of Fuel in the Blast Furnace for Smelting Iron." A discussion on the paper was taken at the following meeting on Tuesday evening last.

MR. ALFRED SMEE, F.R.S., has in the press a volume entitled *My Garden*, in which he gives a complete description of his experimental garden at Beddington, in Surrey, and details the results of his experience in the culture of flowers and fruit: of these nearly 700 species and genera are described. The volume also treats generally of the natural history, geology, and antiquities of the neighbourhood. It is illustrated with about 1,000 wood engravings, executed expressly for the work. The volume will be published by Messrs. Bell and Daldy.

ANNUAL ADDRESS TO THE GEOLOGICAL SOCIETY OF LONDON, FEB. 16, 1872

BY J. PRESTWICH, F.R.S., PRESIDENT

IN looking at the labours of the Society during the past year, it is satisfactory to notice the same activity, the same wide range of subjects as ever, and the same independence of research for truth's sake which there ever should be. But, though good work has been done in special branches and the technical details of Geology, not so much progress has been made in its higher problems. I would, however, direct your attention to the steps made in grouping our volcanic rocks, and in the determination of the fauna of our Cambrian strata, which proves to be so much larger and richer than was anticipated a few years back. Both these subjects are in able hands, and cannot fail to yield important results, the latter especially in aiding to settle that interesting question—the true line of division between the Silurian and the Cambrian formations. On the subject of denudation and river-action, we have also had several excellent papers, and look forward with interest to the further development of the many original views which they have put before us.

The great question of the history of our globe during the Quaternary period seems also to be advancing towards more completeness. Many able observers, both in and out of our own Society, are engaged upon the subject, and various scientific periodicals and publications of our local societies are rich in contributions bearing upon this interesting subject. There is no more wonderful chapter in the earth's history than that which embraces the phenomena connected with the prevalence of great and exceptional cold immediately preceding our time,—the first dim appearance of man—his association with a race of great extinct Mammalia belonging to a cold climate—the persistent zoological characters of the one, so far as we have yet gone, in opposition to the variable types presented in geological time by the others—the search for connecting links, and the measure of man's antiquity,—all of which constitute theoretical problems of the highest interest, and are now occupying the attention of geologists of all countries. Allied also to this subject are the great questions relating to the form of our present continents—the elevation of the land—the origin of valleys and plains—and of all that which prepared this globe for the advent of man.

But while treating of these abstract and philosophical questions, geology deals also with the requirements of civilised man, showing him the best mode of providing for many of his wants, and guiding him in the search of much that is necessary for his welfare. The questions of water-supply, of building materials, of metalliferous veins, of iron and coal-supply, and of surface-soils, all come under this head, and constitute a scarcely less important, although a more special branch of our science than the palæontological questions connected with the life of past periods, or than the great theoretical problems relating to physical and cosmical phenomena. Looking at this triple division of geology, and seeing that the first, or applied geology, is, as it were, only incidental to our general studies, and therefore not often the topic of our discussions, notwithstanding its practical importance, I propose on this occasion to say a few words in connection with the two momentous subjects which, during the last few years, have been made the objects of investigation by two Royal Commissions,* on both of which the geological questions have received much and careful consideration. I shall here restrict myself to the more special geological bearings of the subject, extending them, however, in some directions beyond the scope of the original inquiries, and refer you to the Reports and Minutes of Evidence themselves for the many valuable economical questions and practical details which are there discussed.

Our Springs and Water-supply.

The site of a spring or the presence of a stream determined probably the first settlements of savage man; and his civilised descendants have continued, until the last few years, equally dependent upon like conditions—conditions connected first with the rainfall, and, secondly, with the distribution of the permeable and impermeable strata forming the surface of the country. Under ordinary circumstances, few large towns have arisen except where there has been an easily accessible localised water-supply, and where the catchment-basin on which depends the volume of the rivers has been large, and permeable strata prevail. Take, for example, London. Few sites could be more favourable in every respect. Beneath it are strata rich in springs, while at a distance there is that large development of those massive permeable strata so necessary to maintain a sufficient and permanent flow in our rivers. As the conditions exhibited in the London basin afford all the illustrations we need for our subject, I will confine myself in this address to that area alone.

London north of the Thames stands on a bed of gravel, varying in thickness from ten to twenty feet in round numbers, and overlying strata of tenacious clay from 100 to 200 feet. The former being easily permeable, the rain falling on its surface filters through it, until stopped by the impermeable London clay, where it accumulates and forms a never-failing source of supply to the innumerable shallow wells that have been sunk all over London from time immemorial, and which for centuries constituted its sole water-supply. Not only does it form an easily accessible underground reservoir, although of limited dimensions; but where the small intersecting valleys cut down through the bed of gravel into the London clay, a portion of the water in this reservoir escapes at the junction of the two strata, and gives rise to several springs formerly in much repute, such as those of Bagnigge Well, Holywell, Clerken-well, St. Chad's Well, and others.

The early growth of London followed unerringly the direction of this bed of gravel, eastward towards Whitechapel, Bow, and Stepney; north-eastward towards Hackney, Clapton, and Newington; and westward towards Chelsea and Kensington; while northward it came for many years to a sudden termination at Clerkenwell, Bloomsbury, Marylebone, Paddington, and Bayswater; for north of a line drawn from Bayswater by the Great Western station, Clarence Gate, Park Square, and along the north side of the New Road to Euston Square, Burton Crescent, and Mecklenburg Square, this bed of gravel terminates abruptly, and the London clay comes to the surface, and occupies all the ground to the north. A map of London, so recent as 1817, shows how well-defined was the extension of houses arising from this cause. Here and there only beyond the main body of the gravel there were a few outliers, such as those at Islington and Highbury; and there habitations followed. In the same way, south of the Thames, villages and buildings were gradually extended over the valley-gravels to Peckham, Camberwell, Brixton, and Clapham; while, beyond, houses and villages rose on the gravel-capped hills of Streatham, Denmark Hill, and Norwood. It was not until the facilities were afforded for an independent water-supply by the rapid extension of the works of the great water companies, that it became practicable to establish a town population in the clay districts of Holloway, Camden Town, Regent's Park, St. John's Wood, Westbourne, and Notting Hill.

On the outskirts of London a succession of villages grew up for miles on the great beds of gravel ranging on the east to Barking, Ilford, and Romford—on the north, following the valley of the Lea to Edmonton and Hoddesdon; and on the West, up the Thames-valley to Ealing, Hounslow, Slough, Hammersmith,

* Royal Commission on Water Supply, appointed April 1867. Report of the Commissioners and Minutes of Evidence and Appendix, June 1869. Royal Commission on Coal Supply, appointed June 1866. Reports of the Commissioners, Minutes of Evidence, Appendix, July 1871

and beyond; whereas, with the exception of Kilburn, hardly a house was to be met with a few years since between Paddington and Edgeware, or between Marylebone and Hendon; and not many even between the New Road and Highgate and Hampstead. As a marked case of the excluding effects of a large tract of impermeable strata close to a great city, I may mention the denuded London-clay district extending from a mile north of Acton, Ealing, and Hanwell, to Stanmore, Pinner and Ickenham, near Uxbridge. With the exception of Harrow (which stands on an outlier of the Bagshot Sands), and Perivale, and Greenford (on outliers of gravel), there are only the small villages of Northholt and Greenford Green. In the earlier edition of the Ordnance Maps, there was a tract of ten square miles north and westward of Harrow within which there were only four houses. Yet the ground is all cultivated and productive. But immediately eastward of this area, and ranging thence to the valley of the Lea, the ground rises higher, and most of the London-clay hills a e capped by gravel of an older age than that of the London valley, and belonging to the boulder-clay series. On these we have the old settlements of Hendon, Stanmore, Finchley, Barnet, Totteridge, Whetstone, Southgate, and others.

There is yet another very common source of well-water supply from beds of gravel directing population to low sites in valleys, which is this. Everywhere on the banks of the Thames and its tributaries there is a lower-lying bed of valley-gravel or of rubble on, and often passing beneath, the level of the river. This bed is supplied with water both by rain falling on it, by springs thrown out from the adjacent hills or by the drainage from those hills and in places by infiltration from the river, when, from any cause, the line of water in the gravel falls below that of the adjacent river; while, on the other hand, the surplus land-supplies find their way direct and unseen, from the bed of gravel to the river. A great part of London south of the Thames, Westminster, Battersea, and a number of towns up the Thames, as Hammersmith, Brentford, Eton, Maidenhead, and others, together with Newbury and several villages on the Kennet, also the towns of Ware and Hertford on the Lea, have this shallow well-supply. A great many towns and numberless villages along most of our river-valleys all through England, and on whatever formation situated, are dependent on this superficial source of supply, a supply much more permanent than the other shallow well-supplies, in consequence of the outside aid from springs and rivers. It is, however, only in case of exceedingly dry seasons or of excessive pumping, that the supply requires to be supplemented by the river-waters. As, in ground of this description, the land-water is generally dammed back by the stream, the level of the water in the wells, which are always shallow, varies with the level of the water in the streams, rising and falling more or less with them.

A few of the higher London-clay hills in the neighbourhood of London are also capped by outliers of the Bagshot Sands, as, for example, Harrow, Hampstead, and Highgate, all of which are sites of old habitations. The sands at these places attain a thickness of from 30 to 80 feet, are very permeable, and afford a sufficient water-supply by means of wells to a limited population. A number of well-known small springs are thrown out at the contact of the sands and the clay on the slopes just below and around the summit both of Highgate and Hampstead Hills. In some instances, owing to the presence of iron in the sands, they are slightly chalybeate. When the Baghshot Sands, further westward of London, attain their fuller development of from 300 to 400 feet, the depth to the water-level at their base becomes so great that the upper porous beds are left high and dry, and form uncultivated wastes, such as Bagshot Heath, Frimley Heath, and others; but on the outside of this area, where the sands become thinner, and the water-level more within reach, we find a number of villages, such as Englefield Green, Sunninghill, Brackwell, Wokingham, Alderstone, Esher, Weybridge, Woking, &c. There are also some thin subordinate beds of clay in the middle of the series which hold up a sufficient quantity of water for small local supplies, and give rise to small streams in the valleys of the Blackwater and of Chobham. The running nature of portions of these sands, and the presence of beds of ferruginous and green sands, often interfere much with the construction of deep wells, and the quantity of the well-water; and, externally, the mixed clay-and-sand character of the upper beds of the London clay fails to give any good retaining-line for the water, which therefore rarely issues as springs, but oozes out from the general surface of the intermediate spongy mass.

The 70 to 100 feet of sands and pebble-beds belonging to the lower tertiary strata under the London clay, and overlying the chalk, are also very permeable, and being intercalated with some beds of retentive clay, they give rise to one or two levels of water, affording wherever these strata form the surface, as at Blackheath, Bexley, Chiselhurst, and Bromley, a moderate water-supply to shallow wells. Where these sands dip under the London clay, and only present a narrow belt on the surface, a small valley is commonly formed into which the London-clay hills drain on the one side, and on the other the chalk dammed back by the Tertiary strata throws out its springs, and the sands are thus kept charged with water up to a short depth from the surface. As instances of the many places whose sites have been determined by these favourable circumstances, I may name Croydon, Beddington, Carshalton, Sutton, Cheam, Ewell, the villages between Epsom, Ashstead, and Leatherhead, to Guildford, and again between Old Basing and Kingsclere.

But besides furnishing a supply by ordinary wells to a number of villages on their line of outcrop, the Lower Tertiary sands have of late years contributed to the metropolitan supply, as well as to the supply of those adjacent districts where the surface is formed of tenacious clay, and water is scarce, by means of artesian wells. For along the line of country just named, and along a more irregular belt on the north of London, these sands pass beneath the London clay, so that the water they receive from rain and springs on the surface, passes underground, where it is prevented from rising by the impermeable superincumbent clay; consequently, as there is no outlet for the water below ground, these sand-beds are filled with water along their whole underground range, between their outcrop in Surrey and that in Hertfordshire.

I need not dwell here upon the constructions of Artesian wells, which have been explained by Hericart de Thury, Arago, Degousée and Laurent, Burnell, Hughes, myself, and others, beyond offering a few explanatory remarks on this particular case, which we shall also have to bring to bear upon the origin of springs.

The surface of the ground at the outcrop, just referred to, of the Lower Tertiary sands is about 100 ft. above the level of the Thames, whilst under London the sands are at a depth of from 100 ft. to 220 ft. below that level, thus forming the shell of a basin from 200 ft. to 300 ft. deep, the centre of which is filled with a depressed mass of impermeable clay, There is, however, a notch in the lip of the basin, where it is traversed by the Thames, at Deptford and Greenwich, which is at a lower level of 100 ft. than the rest of the rim. Below this level, as there is no escape for the water, the strata are naturally perpetually water-logged; and if any water is withdrawn from one part, it is, owing to the permeability of the strata, at once replaced from adjacent parts of the same strata. Early in the present century, bore-holes were made through the overlying London clay to the sands at depths of from 80 ft. to 140 ft., and the water from these deep-seated springs rose at once to a height of several feet above the level of the Thames, where it tended to maintain itself, and thus form, in the lower-lying districts, permanent natural fountains. But the ease and facility with which this abundant supply was obtained, led to the construction of so great a number of such wells that a time soon came when the annual rain outfall no longer sufficed to meet the demand, or, rather, it could not be transmitted fast enough to the central area of abstraction to replace the out-draught. The consequence was that, after some years, the water ceased to overflow, and the line of water-level has gradually sunk at London, until it now stands some 70 ft. or 80 ft. beneath the surface level. This, however, is not the case at a distance from London; and in many parts of Middlesex, and more especially in Essex, where Artesian wells are common, they have been found of very great service.

In order to supply the deficiency thus caused in the Lower Tertiary sand, most of the Artesian wells in London have of late years been carried down into the underlying chalk, which also extends beneath London at depths of from 150 ft. to 280 ft. Both formations are permeable, but in different ways. On both the rainfall is at once absorbed, but the transmission of it is effected in different ways. Through the sands it filters at once; but not so with the chalk. A cubic foot of the latter will hold two gallons of water by mere capillary attraction; but it parts with this with difficulty. Still in time it finds its way through the body of the chalk, aided by the innumerable joints, fissures, and lines of flints by which this formation is traversed; and, when once under the line of saturation, the water in these fissures circulates freely. This line of saturation is governed in this as well as in all other permeable formations, by the level of the lowest natural point of escape, which is either the coast-line if near, or the nearest river-valley. Below these levels permeable strata are always charged with water; consequently under London the chalk is everywhere water-bearing; but as the Lower Chalk is more compact than the Upper, and is less fissured, especially when covered by other strata, and as the more compact water-logged chalk delivers its charge with extreme slowness, it is not until a fissure is met with that a free supply of water is obtained. Further, as there is no law regulating the position of the fissures, the depth to which the chalk has to be traversed before meeting with a free supply of water is quite uncertain. It is a question of probability depending upon meeting with a fissure sooner or later—10 to 15 feet have sufficed in some of the deep London wells, whereas in others it has been necessary to sink to a depth of from 100 to 200 feet or more before hitting on the necessary fissures. Large as this supply is, the same causes which have operated in the case of the sands have told also on the chalk supplies (and, no doubt, there is some community between tbe two), and the great demands on it have occasioned a similar lowering of the water-line. At the same time this line also remains unaltered at a distance from London, and as with Tertiary Sands the mass of the chalk beneath intersecting the level of the river valleys remains constantly charged with water. Ordinary wells, therefore, sunk below this line of saturation into the chalk where it comes to or near the surface, are capable of yielding very large quantities of water. More than seven million gallons daily are in fact now so obtained from the chalk on the south-east of London.

Numerous and useful as the London Artesian wells are, they sink into insignificance when compared with the application of the same system in Paris. Our deepest wells range from about 400 to 500 feet, and the water comes from the chalk hills at a nearest distance of from 15 to 25 miles from London; whereas in Paris the well of Grenelle is 1,798 feet deep, and derives its supplies from the rain-water falling in the Lower Greensands of Champagne, and travelling above 100 miles underground before reaching Paris. The well of Passy, sunk also through the Chalk into the Lower Greensands at a depth of 1,923 feet, derives its supplies from the same source. The level of the ground above the sea at the outcrop of the Lower Greensands in Champagne averages about 350 feet, and the water at Grenelle well rises 120 above the surface, which is nearly the level of the Seine, there 89 feet above the sea-level. The water-delivery is large and well maintained. These results were considered so encouraging, that in 1865 the Municipality of Paris decided on sinking two Artesian wells of unexampled magnitude. Hitherto the bore-holes of such wells have been measured by inches, varying from 14 to 4 inches, that of Passy alone having been 4 feet at the surface and 2 feet 4 inches at bottom. But it was resolved to exceed even the larger dimensions of this well.

One of these experimental wells is in the north of Paris, at La Chapelle, St. Denis, 157 feet above the sea-level. A shaft, with a diameter of of 6½ feet, was first sunk through Tertiary strata to a depth of 113 feet. At this point the boring was commenced with a diameter of 5½ feet, and carried through difficult Tertiary strata to a depth of 450 feet, when the Chalk was reached. A fresh bore-hole was here commenced in August 1867, which in September 1870 had reached the depth of 1,954 feet. The works were stopped on account of the war until June 1871, when they were resumed, and the bore-hole has now reached the great depth of 2,034 feet, with a diameter still of 4 feet 4½ inches. It is now in the Grey Chalk, and it is calculated that the Lower Greensands will be reached at a depth of about 2,300 feet.

The other Artesian well is at the Buttes-aux-Cailles, on the south-east of Paris, at an elevation of 203 ft. above the sea. The Tertiary strata are there only 205 ft. thick. This well is not quite on so large a scale as the other, and is still, at the depth of 1,640 ft., in the White Chalk.

The discharge from these great wells will probably be equal to that of a small river. At Passy, notwithstanding some defective tubage, and the circumstance that the surface of the ground is there 86 ft. above the Seine, the discharge at the surface is equal to 3½ millons of gallons daily; and it has been above 5 millions, or enough for the supply of a town of 150,000 inhabitants.

The question may arise, and has arisen, why, with a like geological structure, should not like results be obtained at London as at Paris; and, to a certain extent, it has been answered. At Kentish Town an Artesian well was, in 1855, carried through 324 feet of Tertiary strata, 645 ft. of Chalk, 14 ft. of Upper Greensand, and 130ft. of Gault. Instead of then meeting with the water-bearing Lower Greensands which crop out from beneath the Chalk, both on the north and south of London, unexpected geological conditions were found to prevail, to which we shall have occasion to refer presently; and not only were these Greensands found to be absent, but likewise all the Oolitic and Liassic series. The bore-hole passed at once from the Gault into a series of red and grey sandstones, probably of Palæozoic age, and not water-bearing. The Chalk has more recently been traversed at Crossness, near Plumstead, where its base was reached at a depth of 785 ft., and the bore-hole carried 159 ft. deeper into, but not through, the Gault, when, owing to difficulties caused by the small size of the bore-hole, the work had to be abandoned. Although we were mistaken in our anticipations as to the results of the first of these works, still it is evident—as the Lower Greensands, with a thickness of 450 ft., pass beneath the Chalk and the Gault in a line from Farnham, Reigate, to and beyond Sevenoaks—and they again occupy the same position north of London, on a line from Leighton Buzzard to Potton—that it is only a question of how far they may be prolonged underground towards London. They have as yet been followed only 4 miles from their outcrop under the Gault in Buckinghamshire, and 1 mile in Kent; and no attempt has been made to follow them under the Chalk. It is therefore quite possible that they may extend to under Croydon, or even to Sydenham, or still nearer London; but this depends upon the width of the underground ridge of Palæozoic rocks, which has not been determined. It is a matter for trial. As the sands are from 200 to 500 ft. thick, and show no sign of an immediate approach to the old shore-line, there is every probability that in Kent and Surrey they extend at all events some miles northward, and in Bucks some miles southward, before they thin off against the underground ridge of old rocks, so that they might still be found available, as a supplementary source, for the water-supply of London.

Such is the geological structure of the ground on which this large city is dependent for its first and immediate water-supply by means of wells. The highest seam of water, that in the drift-gravel, extends almost everywhere under the streets and houses of London, at depths of from 12ft. to 25ft., forming what is called ground-springs. The Lower Tertiary sands, with their greater thickness, and their larger and distant area of outcrop, contain the second and larger underground body of water beneath London. The third underground reservoir is the Chalk, which, from its large dimensions—500ft. to 1,000ft. thick—and extensive superficial area, forms a still larger reservoir, and source of water-supply.

With the increase of population, however, the need for larger quantities necessitated the recourse to river-supply; and this supply, equally with the other, is regulated by geological conditions, only that in this case the question concerns those conditions which affect the strata throughout the catchment-basin of the river itself above the town which needs its supply.

(*To be continued.*)

PROF. SCHIAPARELLI'S RESEARCHES

THE following address was delivered by the president of the Royal Astronomical Society, Mr. William Lassell, February 9, 1872, on presenting the Gold Medal of the Society to Signor Schiaparelli:—

You will have learned from the Report just read, that your Council have awarded the Gold Medal this year to Signor Schiaparelli; and I regret to have to inform you that we shall be deprived of the pleasure of presenting it to him in person; as by a letter received from him a few days ago, I learn that his duties of Professor and Director of the Observatory at Milan will prevent his being able to undertake so long a journey.

The first notice I find of Signor Schiaparelli's labours is his discovery of the minor planet *Hesperia*, at the Observatory of Milan, on April 29, 1861, an indication that, besides his mathematical attainments in Theoretical Astronomy, he possesses industry and practical skill as an observer.

In the *Astronomische Nachrichten* of August 13, 1864 (No. 1487), is a purely mathematical paper by him, entitled "Théorèmes sur le mouvement de plusiers corps qui s'attirent mutuellement dans l'espace." Of this paper, not bearing immediately upon those labours of Signor Schiaparelli which have more especially called forth the award, I will only express the opinion of a friend of high mathematical attainments, who characterises it "as an elegant and probably original contribution to the theory of the orbits of bodies moving freely in space, and acted on only by their mutual attractions."

I come now to give some account of Signor Schiaparelli's principal discovery of the law of identity of meteors and comets, and of the observations and reflections which led him to that result, as contained in a series of letters to Father Secchi in the year 1866.

It appears from these that Signor Schiaparelli's study of this subject received a great impulse from his own observation of the meteors which fell on the nights of the 9th, 10th and 11th of August, 1866. He states that he was then confirmed in the opinion expressed three years before, that, of the meteors which usually fall on those nights, a great number are distinguished by their starting nearly all from one point. And, from the spasmodic fall of these meteors—more sometimes falling in one minute than in the next quarter of an hour—he inferred that their distribution in space must be very unequal. He also observed that those stars proceeding from one point were all of a fine yellow colour, and left behind them a fugitive but very sensible track; whilst the other meteors, proceeding from various points, offered every variety of colour and form. Hence he concludes that the meteors form a number of rings, and become visible when the earth traverses their orbit, as if shooting forth from one point in the sky. And he remarks that the observations of M. Coulvier-Gravier, and Professor Heis, and of our own countrymen, Professor Herschel and Mr. Greg, have shown that these radial points occur in every quarter of the heavens; therefore these rings or orbits must possess every possible degree of inclination to the ecliptic.

He then proceeds to inquire how such a mass of cosmical matter could become accumulated in the Solar System. This system seems to consist of two classes—the planets, characterised by but little eccentricity of orbit, slight variation of the plane of the orbit, exclusion of retrograde motion, and a tendency to take the form of a sphere (deviating from it only so much as is necessary to preserve the equilibrium of the body)—these characteristics applying also to the secondary systems, with the exception of the satellites of *Uranus*. The second class consists of cometary bodies, which are under no law as to the planes of their orbits, or the direction of their motions. The point most remarkable about them is the extreme elongation of their orbits, most of which are described in stellar space; which seems to show that they did not form part of our system when that was first constituted, but are wandering nebulæ picked up by our sun.

Signor Schiaparelli further observes that the velocity of the solar system through space has been shown by Otto Struve and Airy to be somewhat similar to that of the planets round the sun. Now if a nebulous body or comet in motion were to come within the action of the sun, it would go round the sun at such an immense distance from us, that it would remain invisible. Two circumstances might bring it within our range of vision—first, if the comet met the sun in almost a direct line; and secondly, if it were travelling in a direction parallel to the sun's motion.

If we suppose a cloud of cosmical matter formed of particles so minute and so widely separated as to possess scarcely any mutual attraction, to be brought within the power of the sun's influence, each particle would pursue an elliptic orbit of its own. Those particles which differed most in the *planes* of their orbits would however possess *nodes in common*, and, in consequence, the particles as they approached the sun would necessarily approach each other, and when separating again, after passing the node, would at their perihelion passage be still very much nearer than they were when brought first within the sun's attraction. Those particles which, lying in the same plane, presented a wide angle with respect to the sun, would form *ellipses*, the planes of which would be identical; though the positions of the major axes would diverge, and, as a result, the particles at their perihelion would pass in nearly the same orbit, but at different velocities, the originally foremost particle being overtaken by those behind it. Again, those particles which, being in the same *plane*, were also in the same *line with regard to the sun*—their separation consisting in the variation of their distance from the sun—would form *ellipses in the same plane*, and having a major axis in the same direction, but of different lengths,—the orbit of the particle *nearest* the sun being described *within* that of the farthest particle, the result of which would be a difference of speed, and an ever-widening distribution of the particles along the whole of the orbit. This reasoning is illustrated, in the second letter to Father Secchi, by a series of diagrams and figures; and then Signor Schiaparelli proceeds to give a recapitulation or summary of his principal propositions thus:—Celestial matter may be divided into the following classes, 1st, fixed stars; 2nd, agglomerations of small stars (resolvable nebulæ); 3rd, smaller bodies invisible except when approaching the sun (comets); 4th, small particles composing a cosmical cloud. This last class probably occupies a large portion of the celestial spaces, and the motion of these dust-clouds may be similar to that of the fixed stars. When attracted by the sun they are not visible unless they receive an orbit which is an elongated conic section.

Whatever may have been the original form of the cloud, it cannot penetrate far into our system without assuming the form of an elongated cylinder passing gradually into a stream of particles. The number of such streams seems to be very great. The particles are so scattered that their orbits may cross each other without interruption, and may possibly be always changing like the beds of rivers. The stream, after passing its perihelion, will be more diffuse than before; and, when passing a planet, may be so violently affected as to separate or break up, and even some particles may assume quite a new orbit and become independent meteors.

Thus meteors and other celestial phenomena of like nature, which a century ago were regarded as atmospheric phenomena —which La Place and Olbers ventured to think came from the moon, and which were afterwards raised to the dignity of being members of the planetary system—are now proved to belong to the *stellar regions*, and to be, *in truth*, *falling stars*. They have the same relation to comets as the asteroids have to the planets; in both cases their small size is made up by their greater number.

Lastly, we may presume that it is certain that falling stars, meteors, and aerolites, differ in size only and not in composition; therefore we may presume that they are an example of what the universe is composed of. As in them we find no elements foreign to those of the earth, we may infer the similarity of composition of all the universe—a fact already suggested by the revelations of the spectroscope.

Signor Schiaparelli further pursues the subject in another and later paper, published in No. 1629 of the *Astronomische Nachrichten*, entitled "Sur la Relation qui existe entre les comètes et les étoiles filantes." In this communication he refers to the letters to Father Secchi above referred to, in which he had endeavoured to bring together all the arguments in favour of the opinion of an analogy between the mysterious bodies known as shooting stars and comets.

Signor Schiaparelli, in this paper, proceeds to state that he is prepared to afford to this analogy a large amount of probability, since there is no doubt that certain comets, if not all, furnish the numerous meteors which traverse the celestial spaces. In proof of this Signor Schiaparelli quotes from a paper of Prof. Erman, in which he has pointed out the method of obtaining a complete knowledge of the orbit described by a system of shooting stars, when the apparent position of the point of radiation and the velocity through space of the meteors is known

Assuming from the necessity of the case that the orbit of the August meteors must be an elongated conic section, Signor Schiaparelli employs the method of Erman to calculate the parabolic orbit of those bodies; taking right ascension 44° and north declination 56° for the position of the point of divergence, according to the observations made in 1863 by Prof. A. S. Herschel. And he proceeds to give the following elements, assuming the maximum of the display of 1866 to be August 10, 18 hours. Comparing these elements of the orbit of the August meteors with those of the orbit of Comet II. 1862, calculated by Dr. Oppolzer, he exhibits the following remarkable coincidence in each element:—

	Elements of the Orbit of the Aug. Meteors.	Elements of the Orbit of Comet II, 1862
Perihelion Passage	23 July, 1862	22·9 Aug., 1862
	° ′	° ′
Longitude of Perihelion	343 28	344 41
Ascending Node	138 16	137 27
Inclination	64 3	66 25
Perihelion Distance	0·9643	0·9626
Revolution Period	105? years	153·4 years
Motion	Retrograde	Retrograde

Although the time of revolution of the August meteors is still doubtful, Signor Schiaparelli, on reference to the catalogues of Biot and Quetelet, deduces a hypothetic period of 105 years, which introduces but small changes in the elements—very inferior to the uncertainty of some of the data on which this determination is built.

In the letters above referred to, Signor Schiaparelli had given an orbit for the meteors of November, assuming the point of radiation as determined in America to be γ *Leonis*. But later observations made with much care in England have shown that this position is erroneous by several degrees, so that that orbit can only be termed a very rough approximation. Assuming, then, that the point of radiation is longitude 143° 12′ and latitude 10° 16′ north—that the maximum of the shower was November 13, 11h. G.M.T.—and that the period of revolution is $33\frac{1}{4}$ years, according to Prof. Newton—Signor Schiaparelli compared the following elements of the meteoric orbit, which he compared with those of the orbit of Comet I., 1866, calculated by Dr. Oppolzer.

	Elements of the Orbit of Nov. Meteors.	Element of the Orbit of Comet I, 1866.
Perihelion Passage	Nov. 10·092, 1866.	Jan. 11·160, 1866.
	° ′	° ′
Longitude of Perihelion	56 25·9	60 28
Ascending Node	231 28·2	231 26·1
Inclination	17 44·5	17 18·1
Perihelion Distance	0·9873	0·9765
Eccentricity	0·9046	0·9054
Semi-axis Major	10·340	10·324
Revolution Period	33·250 years	33·176 years
Motion	Retrograde	Retrograde.

The assumed position of the point of radiation of the meteors is the mean of 15 determinations obtained by Prof. A. S. Herschel, and given in the *Monthly Notices* of our Society, vol. xxvii. p. 19. If this point be advanced 2° in longitude, and 145° be taken in lieu of 143°, the difference of 4° in the place of the longitude of perihelion in the above elements will disappear.

Signor Schiaparelli then concludes his memoir in these remarkable words :—"These approximations need no comment—must we regard these falling stars as swarms of small comets, or rather as the product of the dissolution of so many great comets? I dare make no reply to such a question."

In venturing to offer a word or two of comment on this very imperfect *résumé* of the labours of Signor Schiaparelli, it appears to me that we can scarcely speak of them too highly, or overrate their importance. Granting that his hypotheses are correct,—of which indeed there seems to be a very high probability, some of the most difficult questions in the contemplation of the constitution of the universe seem at once, and as it were *per saltum*, to be solved. To have placed before our view so clear a history of those mysterious bodies—nebulæ, comets, and aerolites, and their several and intimate relations pointed out—is an advancement of Astronomical Science I at least individually had not ventured to anticipate. And a collateral advantage resulting from this splendid discovery, is the encouragement given to the careful and diligent observation of phenomena, even when the prospect of a fruitful result is by no means apparent. Had it not been for the patient, systematic, and intelligent observations of Prof. Heis, M. Coulvier-Gravier, Mr. Greg, and Prof. Herschel, Signor Schiaparelli would have wanted many valuable data required in his investigations.

I may finally remark that an important confirmation of Signor Schiaparelli's conclusions appears in a valuable paper of Prof. Adams, in our *Monthly Notices*, vol. xxvii. p. 247, in which from somewhat different data, including some observations of his own, he calculates elliptic elements of the November meteors generally very accordant with those above given.

SOCIETIES AND ACADEMIES

London

Geological Society, March 6.—Prof. Duncan, F.R.S., vice-president, in the chair.—(1.) "*Prognathodus Güntheri* (Egerton), a new genus of fossil Fish from the Lias of Lyme Regis." By Sir P. de M. Grey-Egerton, Bart, M.P., F.R.S. In this paper the author described a new form of fossil fish, having a broad premaxillary plate somewhat resembling the incisor tooth of a gigantic Rodent, a single auxiliary plate like that of *Callorhynchus*, and a mandibular dental apparatus closely resembling that of *Cochliodus*. For this form he proposed the establishment of the new genus *Prognathodus*, and named the species *P. Güntheri*. *Ischyodus Johnsoni*, Agassiz, also probably belongs to this genus, as it agrees with *P. Güntheri* in the characters of the premaxillary teeth. The author was doubtful as to the exact position of this genus, which had a head extended in a horizontal instead of a vertical plane, suggesting a resemblance to *Zygæna*, but covered with hard plates like the head of a sturgeon, and exhibited in the dental apparatus the curious combination indicated above.—Dr. Günther pointed out the interest attaching to the dentition of this fossil fish as proving the connection between the Ganoid and Chimæroid forms. The existence of three teeth instead of one on each side of the jaw, as in *Ceratodus* and others, presented in it a generic character; but the type was still the same. Mr. Etheridge made some observations as to the horizon in the Lias in which these fossil fishes occurred. He believed that nine out of ten of the Lower Lias Species came out of the upper part of the *Bucklandi* limestone series. Sir P. Egerton corroborated Mr. Etheridge's views as to the localisation of species of fish, and agreed with him as to the importance of recording the exact position of all such fossils.—(2.) "On two specimens of *Ischyodus*, from the Lias of Lyme Regis." By Sir P. de M. Grey-Egerton, Bart., M.P., F.R.S. In this paper the author noticed a new example of the greatly developed rostrum of a male Chimeroid, an inch shorter, more slender, and more attenuated at the apex, than that of *Ischyodus orthorhinus* Egerton, having a projecting median rib along the upper surface, and the tubercles of the lower part smaller and fewer than in *I. orthorhinus*. For this form the author proposed the name of *I. leptorhinus*. Also a dorsal fin-spine, with the cartilages to which it was articulated, showing the mechanism of its attachment very clearly. This spine differs from that of *I. orthorhinus* in being straighter and smoother, and having fewer and smaller tubercles. The author regarded it as probably belonging to *I. leptorhinus*—(3.) "How the Parallel Roads of Glen Roy were formed." By Prof. James Nicol, F.G.S. In this paper the author endeavoured to explain, in accordance with the marine theory of the origin of the Parallel Roads of Glen Roy, the coincidence of the level of these terraces with that of the different cols, and also how the same sea could have produced terraces at different levels in different valleys. He assumed that during the gradual elevation of the land, the gradual closing of the straits between its separate masses by the elevation of the cols above the surface would, by checking the eastward flow of the tidal current, cause the sea-level in the western bays to remain stationary relatively to the rising land; and during this period the marine erosion would take place along a line corresponding in level to the col. Hence, in Glen Gloy, which has only one col, the highest in the system, the highest road only was formed; and Glen Gloy remained unaffected by the stoppage of those cols which produced three roads at lower levels in Glen Roy, the lowest of them also extending round Glen Spean. Professor Ramsay entered into the history of the theories for accounting for the terraces, the first of which, that of Professor Agassiz (in 1840), accounted for them by a great glacier damming up the valley, and from time to time declining in height. The glacial theory, on which this view rested, had to some extent been doubted, but eventually had been almost universally accepted even by its first opponents. He next cited the works of the late Mr. Robert Chambers as to the existence of old sea-margins, pointing to a gradual sinking of the sea or a rising of the land. There could be little doubt that a great part of Scotland and of the northern part of England, had been at one time covered with glaciers, as had also been the case in other parts of Europe. Unless the whole country had been submerged, and then came up again by a succession of jerks, it seemed impossible that such terraces could have been formed by the sea and still have remained in existence. If, however, there had been great oscillations in temperature, it seemed possible that during the decline of some transverse glacier the varying levels of the lake might have left terraces, traces of which might still be preserved. Mr. Gwyn Jeffreys renewed his protest against regarding these beds as marine unless marine remains were found in them. In Prof. Nicol's former paper, mention, however, had been made of rolled boulders. These occurred at Glasgow, and elsewhere, covered with *Balani*. As, however, no marine remains had been found in Glen Roy, he adopted the freshwater theory. Mr. Evans regretted that no one else was present who would in any degree advocate the author's views. He pointed out that if the surface of the rocks below the detritus in Glen Roy was glaciated, the probability was in favour of the superficial drift being of marine rather than of subaërial origin. He much doubted whether Ben Nevis, or any of the mountains of the district, offered a sufficient gathering-ground for any such glacier as that supposed in the freshwater theory, assuming the climate to have been such as would have admitted of a large lake in Glen Roy. He suggested the possibility of the openings through which the sea would gain access to the district having at the time of the last submergence been to some extent choked with ice, which thus checked the tidal action inland from the present coast; and thought that possibly both glaciers and the sea had together contributed towards the formation of the terraces. These, he observed, were by no means confined to Glen Roy itself, but were to be seen on a large scale, and at a lower level in the valley of the Speam, if not elsewhere.

PARIS

Academy of Sciences, March 11.—The following mathematical papers were read:—On flattened curves, by Mr. A. Cayley, communicated by M. Chasles; on the determinaion of the characteristics of the elementary system of cubics, by M. H. G. Zeuthen, also presented by M. Chasles; and on a change of variables, which renders certain equations with partial derivations of the second order integrable, by M. J. Boussinesq, presented by M. de Saint-Venant. M. de Saint-Venant also presented the continuation of his memoir on the hydrodynamics of water-courses. — Papers on auroras were communicated by Marshal Vaillant, M. Vinson, M. H. de Parville, and M. H. Tarry. M. Vinson's communication, and two extracts from letters read by Marshal Vaillant, related to a magnificent Aurora Australis observed at the Island of Bourbon (Réunion) on the night of February 4-5.—M. C. Saint-Claire Deville presented a note by M. A. Houzeau on the proportion of ozone contained in the air of the country, and on its origin.—M. W. de Fonvielle presented a note in continuation of that read at the previous meeting on the means of protecting habitations against the perils of lightning strokes induced by gas-pipes, &c. — A report, by M. Coumbary, on the prediction of earthquakes, was read. — M. E. Becquerel presented a note by M. A Cazin on the quantity of magnetism of electro-magnets.—M. Delaunay communicated a paper by M. A. M. Mayer describing some experiments, showing that the translation of a vibrating body gives origin to a wave of different length from that produced by the same vibrating body in a fixed position.—A note by M. H. Resal on the geometrical theory of the movement of the planets was also presented by M. Delaunay.—A paper was read by M. Kolb on the densities of hydrochloric acid; it contained some useful tables.—M. Blanchard presented a note by MM. P. Fischer and L. de Folin on their dredgings in the fosse of Cap Breton during the year 1871. These dredgings were made at depths extending from 24 to 250 fathoms. The authors indicate the principal species of animals obtained by them.—M. de Quatrefages communicated a paper by M. E. Perrier containing a summary of his anatomical investigation upon the earthworm, and M. Coste a note by M. G. Pouchet on changes of colour produced in prawns to accommodate them to the colour of surrounding objects. This change of colour is prevented by removing the eyes of the prawns.—M. A. Leymerie described some geological peculiarities in the lower Pyrenees.

March 18.—M. Serret presented some remarks on the note by Mr. Boussinesq, read at the last meeting of the Academy, and stated that M. Boussinesq was long since anticipated by Lacroix in the transformation proposed by him. — M. Serret also presented some remarks by M. E. Combescure, upon an analytical memoir by Legendre, on the integration of certain equations with partial differences.—M. de Saint-Venant read a continuation of his memoir on the hydrodynamics of water-courses.—M. H. Saint-Claire Deville communicated a note by M. D. Gernez, on the absorption spectra of the vapours of sulphur, selenious acid, and hypochlorous acid. The author finds that coloured vapours in general absorb rays of irregularly variable refrangibility. Vapour of sulphur at first produces a gradual extinction of the spectrum, except the red part a little beyond line C of the of the solar spectrum; with an increase of temperature the rest of the spectrum reappears with very distinct bundles of lines in the violet and blue, and returning into the green. Vapour of selenious acids produces a very distinct system of absorption-bands in the violet and blue, and the absorption-spectrum of hypochlorous acid is identical with that of hypochloric and chlorous acids.—M. H. Tarry presented a note on the extraordinary extension of the zodiacal light, and its coincidence with the periodical reappearance of auroras; and the aurora of the 4th of February was the subject of notes by MM. Denza, Mohn, and Coumbary.—M. Tarry and M. Denza also noticed the sand rains of the South of Europe.—M. C. Sainte-Claire Deville also presented some remarks on a note on the theory of auroras, read at the last meeting by Marshal Vaillant.—The papers on chemical subjects were particularly numerous. M. Chevreul read a memoir on a phenomenon in the crystallisation of a very concentrated saline solution.—A paper on the formation of chloral by MM. A. Wurtz and G. Vogt was read.—The question of the preservation of wine by the application of heat was further discussed by MM. de Vergnete-Lamotte and Pasteur.—M. Wurtz presented a note by MM. C. Friedel and R. D. Silva, on the isomers of trichlorhydrine and the reproduction of glycerine; a note by M. G. Bouchardat on the transformation of acetone into hydride of hexylene (dipropyle); and some facts with regard to diphenylamine by MM. C. Girard and G. de Laire.—M. C. Robin presented some observations by M. E. Ritter, on colourless bile, in which the author stated that in all cases where colourless bile occurred the liver presented more or less fatty degeneration.—A note was read by M. Duclaux on the influence of the cold of winter upon the seeds of plants.—M. Decaisne presented a note by M. E. Bornet, on the gonidia of Lichens, in which the author supports the curious opinion put forward by M. Schwendener, that the lichens are complex organisms, formed by the association of certain low algæ with fungi or other plants. He regards the connection as one of parasitism.—A note was read by M. S. Meunier on the discovery of an abundant deposit of *Hemirhynchus Deshayesi* in the Calcaire Grossier of Puteaux.—M. C. Bernard presented a note by M. Ollier, on cutaneous grafts, and a note by M. Guibert on the beneficial results obtained by the combined action of morphine and chloroform in surgery.

BOOKS RECEIVED

ENGLISH.—Space and Vision: W. H. S. Monck (Dublin, McGee).—Practical Physiology: E. Lankester; 5th edition (Hardwicke).—Moth and Rust: by M. L. (W. Tegg).

FOREIGN.—Corso di Geologia, Vol. i.: A. Stoppani (Milan, Bernardoni).

DIARY

SATURDAY, MARCH 30.

CHEMICAL SOCIETY, at 8.—Anniversary Meeting.

MONDAY, APRIL 1.

ENTOMOLOGICAL SOCIETY, at 7.
ROYAL INSTITUTION, at 2.—General Monthly Meeting.
VICTORIA INSTITUTE, at 8.—On Force: Dr. M'Cann.

TUESDAY, APRIL 2.

SOCIETY OF BIBLICAL ARCHÆOLOGY, at 8.30.—Notice of a Curious Myth respecting the Birth of Sargina, from the Assyrian Tablets containing an Account of his Life: H F. Talbot, F.R.S.L.—The Assyrian verbs "Basu," to be, "Qabah," to say, and "Isu," to have, identified as variant forms of verbs having the same significations in the Hebrew language: R. Cull, F.S.A.—On the Origin of Semitic Civilisation, chiefly upon Philological Evidence: Rev. A. H. Sayce, M.A.

WEDNESDAY, APRIL 3.

SOCIETY OF ARTS, at 8.
MICROSCOPICAL SOCIETY, at 8.
PHARMACEUTICAL SOCIETY, at 8.

THURSDAY, APRIL 4.

LINNEAN SOCIETY, at 8. —On the Geographical Distribution of Compositæ: G. Bentham, President (concluded).
CHEMICAL SOCIETY, at 8.

CONTENTS

THURSDAY, APRIL 4, 1872

THE FOUNDATION OF ZOOLOGICAL STATIONS

II.—The Aquarium at Naples

When I wrote the first article on the "The Foundation of Zoological Stations,"* I desired to bring before the general public the idea of extending the principle of co-operation in Science in general, and in Biology in particular. I now propose to give a sketch of the internal organisation of a zoological station as it presents itself to my mind. It is natural that in doing this I give more or less a picture of what I intend to produce at the station which is at present being erected under my superintendence at Naples.

The building occupies an area of 7,000 square feet, and is situated at a very short distance—100ft.—from the sea. It forms a rectangle 100ft. long and 70ft. broad, with a height of 40ft. The building is divided into two parts, the lower part being occupied by the tanks of the great aquarium, which is to be open to the public; the upper part containing twenty-four rooms of different sizes for laboratories, a library, and collections, and for lodging the three or four zoologists who will be constantly occupied in managing the station.

I will not speak here of the manner in which the technical parts of the aquarium are to be arranged, as this would scarcely interest my readers. What I should like to specialise a little relates more to the facilities for scientific study which the station will afford.

Let me speak first of the lower part of the building, the great public aquarium. It will contain fifty-three tanks of different sizes, one of them 32ft. long, 10ft. broad, and 3ft. to 6ft. deep; twenty-six 6ft. 6in. long, and equally broad; and twenty-six others 3ft. long and 3ft. to 6ft. broad. These tanks will contain marine animals of all kinds, either isolated or more or less mixed, according to the investigations that are to be made.

I imagine now that in one of these tanks a number of Medusæ and Salpæ are together, and the problem is to know how they will behave in so close a union. This can be solved only in such a tank, and it will be a very easy study, as the naturalist has only to occupy a movable chair, which is placed before the tank, and which hides him and the tank by special precautions completely from the general public. At a certain moment you can put into the tank some rapacious fishes, or some of the swift and warlike Crustaceans of the Palæmon tribe, and wait for the movements and actions of the Medusæ as well as the Salpæ. You may repeat these observations, and add other different species; and if you have patience enough, you cannot fail to discover facts about the general habits of the animals in question, and the functions of their organs, which were unknown before, and which may yield, perhaps, valuable arguments to establish a theory on the manner in which they originated from other animals. As it is, we hardly know anything about the life of Medusæ or Salpæ, and our ignorance of the habits of other marine animals is equally great.

* Nature, vol. v. p. 277.

Let us take another example. I was present when half a dozen stone crabs (*Lithodes Maya*) were brought from Norway to the Hamburg Aquarium. Mr. Lloyd, at that time the Director of the Aquarium, distributed them in several tanks. It happened that one of them found itself in company with a number of *Crenilabrus norwegicus*, a swift and clever little fish. These at once began to attack their new companion. With considerable skill they tried to hurt the eyes of the crab, which on their long stalks presented, of course, the most vulnerable part of the clumsy and spinous animal. After half an hour's continued attacks the fishes actually succeeded in tearing out one of the eyes. This fact made me investigate at once the mode of protection with which Nature had furnished the eyes of Crustacea, and I collected a considerable number of observations, which, if completed and worked out, would possibly form a very interesting chapter in our knowledge of the progress of Natural Selection.

I shall adduce a third instance for the necessity of facilitating observations of this kind. In his excellent refutation of some of Mr. Mivart's objections to the theory of Natural Selection, Mr. Darwin relates ("Origin of Species," 6th Edition, p. 186) some observations made by Malm on the way in which the eyes of the Pleuronectes get both on one side of fish. The following are his words:—

"The Pleuronectidæ, whilst very young and still symmetrical, with their eyes standing on opposite sides of the head, cannot long retain a vertical position, owing to the excessive depth of their bodies, the small size of their lateral fins, and to their being destitute of a swim-bladder. Hence, soon growing tired, they fall to the bottom on one side. Whilst thus at rest they often twist, as Malm observed, the lower eye upwards, to see above them; and they do this so vigorously that the eye is pressed hard against the upper part of the orbit. The forehead between the eyes consequently becomes, as could be plainly seen, temporarily contracted in breadth. On one occasion Malm saw a young fish raise and depress the lower eye through an angular distance of about seventy degrees. We should remember that the skull at this early age is cartilaginous and flexible, so that it readily yields to muscular action. Besides, Malm states that the newly-hatched young of perches, salmon, and several other symmetrical fishes, have the habit of occasionally resting on one side at the bottom; and he has observed that they often then strain their lower eyes so as to look upwards; and their skulls are thus rendered rather crooked. These fishes, however, are soon able to hold themselves in a vertical position, and no permanent effect is thus produced. With the Pleuronectidæ, on the other hand, the older they grow the more habitually they rest on one side, owing to the increasing flatness of their bodies, and a permanent effect is thus produced on the form of the head and on the position of the eyes."

I think observations of this kind ought to speak so much in favour of a great observatory for marine animals, that it would be superfluous to add any more instances for its necessity. I hope the Naples Institution will rapidly produce a great number of similar observations, and thus render one of the most important services to the still utterly neglected knowledge of the animal life of the ocean.

Let us now ascend the staircase from the lower part of the future Zoological Station to the upper floor. We pass through a series of rooms on the north side, the first of which is occupied by the chief zoologist. Before the window a table for microscopical work is placed, surrounded by small tanks for breeding eggs and keeping alive larvæ and other smaller animals. Each tank is furnished with a continuous current of fresh sea-water, which can be weakened or strengthened, or completely stopped, as it pleases the zoologist. The rest of the room is reserved for the business matters of the station. Next to it comes the library-room, large enough to keep a library of 25,000 volumes. Two tables for microscopical work placed near one another occupy the place near the window, some tanks of different sizes, completely furnished with tubes, &c., are placed at the disposal of those who occupy the tables. Next follows the great laboratory. In the centre of the room we find at least twenty to thirty tanks of different sizes, each of them with its own current of sea-water; the two great front windows afford light for four working tables placed near them. The walls may be occupied by physiological instruments and by other apparatus which will be required. Galleries on the walls and across the centre of the room yield enough space for placing all sorts of collections and other things on them without hindering the free passage in the laboratory. The last room on this northern side will be occupied by the first assistant zoologist, and be furnished, like that of the chief zoologist, with working table and tanks. Both the corners of the house are occupied by towers, and these towers contain two small chambers of nine feet square; they are also to be furnished with tables and some tanks, so that in all ten zoologists may, at the same time, find complete accommodation for their work.

The south side of the upper part of the station will be occupied by four rooms, sufficiently large to allow the collections to increase for many years, and the laboratory to take possession of double the space it will occupy at the beginning. The west and east side afford some private rooms for the use of the naturalists employed in the management of the station. Under the roof eight other smaller rooms complete the whole disposition of the space inside the building.

Now let me say some words on the functions these organs of the Zoological Station are to exhibit in future. There are first to be noticed the great advantages which will be offered to the single student. Whoever works with marine animals will be painfully acquainted with the difficulty of preserving them alive longer than two or four days. They almost invariably die, and decompose very soon. If one now considers that anatomical and stil more embryological problems are only to be solved during weeks or months of undisturbed and indefatigable exertion, it is quite evident what enormous advantage must result from the possibility of keeping these animals alive during weeks. And this will be effected by the help of tanks with a continuous stream of sea-water. The sea being always in motion, caused either by the waves or still more by the vast number of currents, makes the constant alternation of fresh and aërated sea-water necessary for the life of the animals. The imitation of these currents and the artificial injection of air into the tanks will render it possible to keep even embryos and larvæ alive, which formerly could never be studied on account of their early death.

Besides, everybody knows how often fishermen bring eggs or larvæ which are completely unknown to the zoologist. They are, perhaps, highly interesting; perhaps belonging to animals whose eggs have never been seen before, as they deposit them far off in the open sea or on the bottom. The single zoologist in his small room in a Naples Hotel, with some bottles or basins at his disposal, puts them into a tumbler, changes the water regularly, and thus succeeds in keeping them alive for a week, but he forgets the changing once, and to-morrow they are dead. A good many will even not live in spite of the changing of the water, because they require the constant stream running over them. The single zoologist in the station, on the other hand, puts them into a tank, sets the stream in motion, and has nothing to do but to watch their development, and the final disclosure of the embryo, or the metamorphoses of the larvæ, and may completely succeed in getting a key to their nature and their relation to other animals.

Considering now the all-importance of embryology and development in the present state of zoology, it is easy to recognise in the continuous stream of the sea-water in the station a fundamental novelty in the conditions for the progress of scientific zoology. Go a little further. It is rarely advisable to work with one subject alone when on the sea-coast. There are so many incidents that change the conditions of the work you have in hand, that you are much wiser to have, whilst working at one chief problem, one or two smaller ones with it. But chance is often a paradoxical thing; it will entirely inundate you one day with excellent material for all these problems, and cause you great embarrassment as to what to take first; and another day it will yield you nothing whatever, so as to force you to idleness. Now again with a series of tanks and streaming sea-water you can pursue everything quite at your leisure, stop one investigation when you like, or take up another, or drop them both, and work for one day with some interesting novelty, without being afraid of spoiling the material of the old objects, and losing the opportunity of getting through it. And everybody knows what a consolation it is to be always capable of taking your principal line of work up again, whilst you are not forced to deny yourself the chance of taking some notice of new arrivals, if it even were only for a little instructive side glance of some hours.

These are some illustrations of the great facilities and advantages of the station, yielding thus in future to scientific workers immense economy of time, money, and power. But this is not all that the station will do. Every well-instructed biologist is aware of the great step anatomical science made when first Cuvier created and afterwards Johannes Müller reformed Comparative Anatomy. The description of the different types, the organs and their homologies, their histological constitution, similarity and dissimilarity, became well worked out, and extended the range of our insight over almost all living animals.

Physiology ought to have gone the same length, following exactly the lines of anatomical research, to tell us something about the functions of all the organs and structures through the whole range of animal life. But physiology did not do so ; it got into another line, investigating with the utmost care, and also with splendid success, the nervous functions of the higher vertebrates, developing theories on the physical agency of these functions, and trying to verify these theories by experiments. It went also into chemical researches, trying to get clear insight into the chemical processes of digestion and the nourishment of the body of the higher vertebrates. In consequence of this one- or rather two-sidedness, it has happened that physiology appears to be very indifferent to the great overthrow of our views regarding the organic world, caused by the doctrine of evolution. Indeed, celebrated physiologists even go so far as to deny the truth of that doctrine altogether. Now nothing can be a stronger proof that there is something amiss in the state of physiology, and this something consists in the complete want of Comparative Physiology. If we cannot understand the anatomical constitution of men and the higher animals without the study of comparative anatomy and embryology, we can equally as little understand their physiological components if we do not follow them up through the whole series of animal life. It is utterly deplorable that so very little has been done in this immense department of Science. What do we know of the functions of such all-important organs as the so-called segmental organs of Annelids, which in the further development of other classes of the animal kingdom grew into some possessing the highest functions? Nobody doubts that Amphioxus is a Vertebrate ; but has any one yet tried to make physiological experiments with that animal, though it is one of the most hard-living of all marine animals ? And is there in any way a base laid for the physiology of fishes, which must yield results of the utmost importance ? Does the academical physiology of modern times do the least to unveil the mysteries of generation, of growth, of degeneration ? Are these departments, perhaps, less interesting, less important, less accessible than Nervous Physiology or the Physiology of Digestion ? There is apparently a lack of idea in this great department of Biology, an overgrowing influence of Physicists, and a want of morphological knowledge among Physiologists. What would have been the fate of Physiology if, unfortunately, Johannes Müller had not died in the same year when the "Origin of Species" came out ? He was the man to create at once the study of Comparative Physiology, and his spirit must again come over physiologists to enable them to perceive the immense field of action before them, and the neglect with which they treat it.

Now, I can only say that it is one of the great objects of the Naples station to do all in its power to carry on a fair commencement of Comparative Physiology. Whatever money may be spared, whatever pains bestowed, it will willingly be given to so important a duty, and it would be considered a great good fortune should a thoroughly instructed physiologist make up his mind to accept a post in the station in order to establish and carry on a Physiological Laboratory.

To all the possible advantages of the station for the intermittent action of single naturalists alluded to above, unite now the great advantage from the fact that such isolated action will be quite superseded. A station like that of Naples wants at least three well-trained zoologists to conduct it properly. One of the greatest privileges for these zoologists will certainly be that teaching forms no essential part of their duties. Whoever knows by experience what a loss of energy and of time is caused to all those original workers who are bound to teach daily on elementary topics, what great relief vacations form in the life of university professors and privat-docents (who generally proceed with original work daily during their vacations), will be aware of the exceeding value of paid places where teaching is no necessity, and is only admitted for single and special puposes. The comfortable system of English fellowships, granting money to young gentlemen who are supposed to merit special rewards by having undergone some examinations, will, in fact, be united to the principle of Continental academies, of paying men of scientific reputation, that they may go on at their leisure with original scientific work. The zoologists in the stations will be selected from the number of young professors or privat-docents, who, as a matter of course, are supposed to be ambitious to do some good things in science, even at the risk of sacrificing comfort and agreeable social life. They will be sufficiently paid, and their payment even raised so as to equal that of a moderately-paid German university professor ; though perhaps not approaching the level of the payment of a young Oxford or Cambridge Fellow. Nevertheless, they will be put in a position to balance that inferiority by making themselves known as workers, and adding to the storehouse of science facts and observations which may secure to them, if not a comfortable position in life, yet at least applause and respect from the eminent men of their science.

And these zoologists, having at their disposal a laboratory of the perfection and extent of the future Naples one, being aided by the possession of an all but complete biological library, and having before their doors the immense storehouse of the Mediterranean Sea, cannot fail to effect a great step in organising the progress of biological work. Let us suppose the question arose whether Cephalopods preceded in geological time other Molluscs, or were a higher developed offspring of them. The problem would be completely insoluble to University zoologists. But the three zoologists of the station at Naples would at once proceed with a solution in working out the embryology of the seven or eight species occurring in the Gulf, communicating and controlling each other's observations and conclusions. Some foreign zoologists might join their labours for half a year, and Science would be at once in possession of some thoroughly worked out contributions to the Comparative Embryology of the Cephalopods. Apply the same system of co-operation to other problems. for instance to one the solution of which is so much longed for, as the Embryology of Sharks. Years will not enable a single worker to go through that enormous task, with the sole aid of his individual opportunities. But suppose the leading zoologist of the station got the plan into his head to carry out the solution of this problem. He invites some excellent zoologist who completely understands the problem to come to Naples, to bring with him two or three assistants who have already beforehand been made acquainted with the object of the inquiry and the chief difficulties of the observation, and to set to work from the very first day of their arrival. He himself will do all in his power to procure every day fresh material of all kinds; by the help of the small steam yacht of the station he may succeed in carrying over to the station sharks which were taken two hours before, so as to secure the life of the embryos without any danger of destruction. Then he can isolate and feed them, and make them live as long as he wants. Any one who knows the fauna of the Mediterranean knows also what a large number of different species of rays and sharks arrive in it, and all these could be readily placed at the disposal of the embryologists, thus enabling them to overcome at once immense difficulties which have hitherto been almost completely unassailable.

The station will have several people, fishermen or guards, who by-and-by will be completely acquainted with the fauna of the bay, and will be able to collect whatever is necessary. As very often rare or much-wanted animals come in with some current in great quantities and disappear even the next day, such animals may at once be taken in great numbers and distributed through a great number of tanks, so as to keep them alive for future time.

Very often zoologists from the Universities have just four or six weeks' leisure, and would very much like to do some original work on the shores of the Mediterranean. But to go there for so short a period, to lose so much time in getting up all the necessary arrangements, and spend so much money for so small and uncertain scientific profit, is rather inadvisable for those who have to live on small incomes. But suppose the station is ready, zoologists announce some weeks beforehand their intention to come to Naples, and to work with this or that object, what is easier and what more comfortable than to arrive at the fixed date, to find lodging, laboratory, library, and material all ready and in the very best state, and to go over a ground of scientific work in six weeks, which otherwise would, perhaps, have occupied three months.

And will not the establishment of the Naples Station enable even those to come and work there, who (like many of the very best German and foreign zoologists) do not command means large enough even for a stay of two or three months at their own expense? Will not the constant presence and the collected experience of the station-zoologists save the foreign naturalists all the trouble and annoyance which inevitably result to every one who is not well acquainted with the ways and modes of life and customs of a place so complicated, and in every way so strange, as Naples? And, on the other side, will not the presence of the three station zoologists guarantee Science that it shall not lose the fruits of all that work which was begun but could not be finished by foreign zoologists, since their teaching duties forced them to go home and leave it uncompleted behind? Easily enough one of the station zoologists takes it up and carries it on to a point where it may be fit for publication, thus preserving the labour and energy spent on it.

But I could continue preaching and preaching on a chapter which ought to be clear to every one who understands the progress of Science. I trust that what has been said is sufficient to procure the assistance of all those who think it a pity that whilst millions and millions are accumulated for the pleasure of individuals who very often do not care a bit for the welfare or the progress of their fellow creatures, schemes like the present, so evidently adapted for throwing open new lines of inquiry into the mystery of the universe, and by that means adding to human progress and happiness, should be abandoned to chance and to isolated individual goodwill and effort.

Naples, March 9 ANTON DOHRN

SCROPE ON VOLCANOS

Volcanos. By G. Poulett Scrope, F.R.S., &c. Second Edition revised and enlarged. With Prefatory Remarks. (London: Longmans, 1872.)

THE subject of volcanos is one which possesses a popular as well as a purely scientific interest, and the more so of late years, since it seldom happens that the foreign mails come in without bringing us tidings of volcanic outbursts or earthquake shocks, often fearfully disastrous, which have occurred in some one or other part of the globe; so that it is but natural to expect that the appearance of a revised and enlarged reissue of the second edition of the well-known work on volcanos by the distinguished and veteran geologist Mr. Poulett Scrope, will attract the attention, not only of geologists, but of the scientifically inclined public in general.

It is not saying too much, when we express our opinion that no geological library can be considered complete without Mr. Poulett Scrope's work; but at the same time it is fairly open to question as to whether this volume in its present form can in 1872 be regarded as an improvement upon what it was before in 1862; since, with the exception of a list of the earthquakes and volcanic eruptions which have occurred since the year 1860, the additional matter, introduced into it as a sort of postscriptum preface, is of a purely discursive and theoretical character, and for various reasons not likely to meet with that general acceptance, from those posted up to date in the subject, which the mass of excellent observational and descriptive matter embodied in the book itself is fully entitled to.

To render full justice to Mr. Poulett Scrope as a vulcanologist, we must, however, carry ourselves back nearly half a century, to the time when the first edition of this work appeared in print; for it is only by so doing that we can be enabled to thoroughly appreciate the importance of his labours in the study of these wonderful phenomena, or to understand how largely they contributed to bring about the substitution of sounder doctrines concerning the formation and structure of volcanos, instead of the very erroneous, yet all but universally received hypotheses, which at that time were taught in the schools of natural science.

If now we proceed to analyse the contents of the volume before us, its perusal will soon show that it devotes itself exclusively to the consideration of the subject treated only from a purely physical and geographical point of view, and as such, it must be admitted to be a most elaborate digest of what is known relating to what may be termed the mechanics of volcanos, their physical structure, and their local distribution over the surface of the earth; whilst at the same time the very excellent descriptions of the phenomena attendant on volcanic outbursts in their different phases, and the building up of cones and mountain chains, are of the greatest value to the student, and the more so from their being, in many instances, founded upon the personal experiences of the author, whose accuracy as an observer in the field can only be fully appreciated by those who, like the writer of this notice, have had an opportunity of following in his footsteps, and examining on the spot localities which Mr. Poulett Scrope has so well described in his memoirs.

In the present volume, the illustrations which are so necessary to a work of this character are not only ample, but are in many instances particularly well selected, so as to express exactly what the author intends to convey. As an example, the following woodcuts, Figs. 1 and 2, borrowed

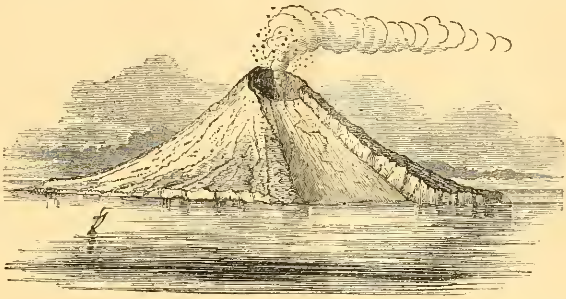


FIG. 1.—VIEW OF STROMBOLI, FROM THE NORTH

from page 31 of the volume, which represent, in elevation and plan, the volcanic Island of Stromboli, or so-called Lighthouse of the Mediterranean, convey to the mind at a glance the main features of a volcanic cone with its crater, of which, as is so common, the one side of the lip has given way. We may also refer especially to two other woodcuts, Figs. 60 and 61, page 232, as an instance of the extremely happy way in which a comparison is made visible to the eye between the principal features of a region of terrestrial volcanic activity, and those of a portion of the visible surface of the moon, in order to point

FIG 2.—PLAN OF THE ISLAND OF STROMBOLI

out in the words of the author (p. 231) that "the analogy is so close, that it is impossible for a moment to doubt the volcanic character of the lunar enveloping crust."

The perusal of this volume, however, also shows that the mineralogy or petrology of volcanos is but barely touched upon, and that the work in reality treats only of one half of the subject under consideration, giving only the purely physical or mechanical, whilst it leaves out of consideration the other half, or equally important chemical one, in which so much has been done during the last twenty years, and without the due consideration of which, it is self-evident that no confidence can or ought to be placed in conclusions drawn as to the causes, probable seat of, or many other questions relating to volcanic action, or to the nature of the interior of the earth itself, which is so intimately connected therewith; and it is on this account that we have purposely abstained in the present notice from criticising the theoretical views and deductions of the author.

In conclusion, whilst we, for the reasons before mentioned, heartily recommend Mr. Poulett Scrope's "Volcanos" to the mature consideration of every English student in this branch of geology, we at the same time advise that it should be studied in connection with the admirable memoirs of Bunsen, v. Waltershausen, and others, which have of late years thrown so much light upon the nature of volcanic phenomena, in order that by making himself conversant with the two great forces in Nature, physical and chemical, he may be the better enabled to arrive at sound conclusions.

DAVID FORBES

OUR BOOK SHELF

Quarterly Weather Report of the Meteorological Office. Part III. July to September 1870. (Stanford, 1872.)

THIS, the new number of the *Quarterly Weather Report*, is in point of care the equal, in some minor details of execution the superior, of all former numbers. The method of showing the wind's velocity by a shaded curve, which has been adopted since the first part of this series, adds much to the ease with which the graphic representation can be read, and is a decided improvement; so is the introduction into the margin of the miniature charts of barometric pressure during strong winds. The engraving too is clearer and finer than in some of the past numbers, and is perhaps as nearly perfect as can be. After a few years the accumulated numbers of these reports will form a most valuable record. There are many students of meteorology still impressed with the idea that, with a correct knowledge of what has been, we may be able to form an opinion of what is to be. It seems to us by no means improbable that with more accurate information, such as this now being stored for future use, we may before long arrive at the power of foretelling the general character of seasons, in regard to their being wet or dry, hot or cold, stormy or gentle; but we see no reason to believe that any amount of study of the past will ever enable us to predict in detail for any length of time in advance, though it may and must lead us to a better capability of rightly interpreting the atmospheric changes going on, of detecting them at their earliest beginning, of judging their probable effects, and thus of extending the period for which "storm warnings" may be made available. With increased experience new power will be gained, new methods will be learned and proved. Even now, the spectroscopic observations by Commander Maclear, to which he called our attention in these columns only a few weeks ago, seem to point hopefully towards a new path in meteorological research; for it is not only in the widely different climate of the Bay of Biscay, the Red Sea, and the Indian Ocean, that he observes the differences in the spectrum which he has spoken of in the article just referred to; he informs us that his later observations lead him to believe that the changes in the atmospheric humidity distinctly correspond to changes in the solar spectrum; that, for instance, an increasing humidity manifests itself by a shortening in of the blue, and by a well marked development of aqueous bands in the red and yellow. Whether further examination will confirm this belief or not it is at present impossible to say, but the spectroscope has done so much towards teaching us the constitution of other atmospheres, that we may fairly entertain a hope that the time has come for it to teach us something about the distant and outlying parts of our own. J. K. L.

Index of Spectra. By W. M. Watts, D. Sc. (London: Henry Gillman.)

ALL workers with the spectroscope must have felt the great inconvenience arising from the employment of numberless different scales in the mapping of spectra. It is to be hoped that at some future time there will be more uniformity, and that authors, when publishing original memoirs, will reduce their measurements to a definite and recognised system. It is clear that such a method must be perfectly independent of the spectroscope and its concomitant parts; the position of each line can therefore only be expressed by its colour, or, in other words, by the length of the wave of light which produces this colour. Dispersion spectra, obtained by the use of prisms of different materials, vary greatly in the relative breadth of the respective colours; thus in the spectrum from crown-glass the red end is larger and the blue end shorter than in the spectra obtained from flint-glass, carbonic disulphide, and by diffraction. It is therefore necessary in spectroscopic researches to record the positions of numerous well-known lines as observed in the instrument that is used. In a diffraction spectrum, however, the position of the lines is dependent solely on their colour, and is precisely the same by whatever method the spectrum is obtained. For the results of different observers to be accurately comparable, the readings obtained by dispersion must either be expressed in wave-lengths, or the spectra must be obtained by diffraction. The wave-lengths of the Fraunhofer lines of the sun have been accurately determined by several observers. The author has adopted as the basis of his work the measurements made by Angström, as these appear to exceed in accuracy all similar measurements at our disposal. When the wave-lengths of a number of lines are known, it is easy to calculate the wave-lengths of the lines of any new spectrum, either by the interpolation formula given by W. Gibbs *Phil. Mag.* [4] xl.157) or by the method of graphical interpolation, both of which methods are explained in the volume before us; all that is required is to have the wave-lengths of two known lines, between which the line to be measured falls. By the aid of Angström's measurements the author has reduced the measurements of the bright lines of all the elements whose spectra have been carefully investigated, and also of air lines as mapped by Thaler, Huggins, and Plucker. These tables will therefore assist materially in the work of reduction, by serving as landmarks from which to calculate the wave-lengths of new lines. The attention that the author has bestowed on this work is the best guarantee of the accuracy of the numbers given. In the lithographic plates at the end of the tables, a drawing of the spectrum of each element is given on the plan proposed by Bunsen, in which the intensity of a bright line is indicated by the height of the line representing it; a chromo-lithograph is given of the double spectra of nitrogen, sulphur, and carbon, and another plate, showing two spectra obtained by Wülner from aluminium, and three from hydrogen at different powers. Dr. Watts is deserving of the best thanks of all those interested in spectroscopic work, for it is to be hoped that his "Index of Spectra" may contribute to the adoption of a uniform scale of measurement, and thus facilitate the advance of the science. A. 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.*]

The Adamites

PHILOLOGISTS will notice with regret a paper bearing the above title in the late number of the *Journal of the Anthropological Institute.* The author appears to have taken up, without proper study, that difficult and dangerous line of argument, the comparison of historical names, and has naturally fallen into the network of delusive fancy which in past generations entangled Jacob Bryant and Godfrey Higgins. Modern philology has abundantly proved that slight, loose, and occasional correspondences in proper names are deceptive as evidence, even among languages of the same family, much more among languages of different families. It is a fair sample of the present paper, that it argues an affinity between the peoples of the Old and New Worlds on the basis of a connection between various names of the Deity, among which are the Russian *Bog*, the Mantchoo *Ab-ka*, and the Hottentot *Teqoa*. The special purpose is to prove that nations are shown by their names to trace descent from an ancestor called *Ad*—"*Adam*, or Father *Ad*." Thus "the great Hamitic race of *Akkad*" is interpreted by the aid of Welsh *ach*—root, lineage," so as to mean "sons or lineage of *Ad*;" and the name of *Ta-ata*, the Polynesian First Man, is "that of the mythical ancestor of the Adamites, reversed, however, and with the addition of *ata* (*aka*), spirit"! It is obvious, though unaccountably overlooked in the paper, that two of the clearest cases of the theory may be found near home. The descent of two nations from Father Ad is perfectly recorded by ourselves, when we call the representative of one a *Paddy*, clearly *Ap-Ad* (from *Ap*, "used in the sense of son"), while the other's Adamite ancestor is commemorated by calling his descendant a *Ta-ffy*.

It is not necessary to give the name of the author of this unlucky paper. Everybody is liable to slips, great or small; and a man may have done work worth doing in one line, but turning suddenly to another, may come to grief utterly. But the Council of the Anthropological Institute should have consulted their own interest and that of their contributor by declining to print the present essay. It is the duty of a learned society to examine even a hasty and ill-considered idea brought forward by one of its members, but not to put it on public record against themselves and him. M. A. I.

The Segmentation of Annulosa

IN the extract from his Address to the Entomological Society, given in NATURE, February 29, Mr. Wallace remarks that Mr. Spencer's views have not been so much as once alluded to in the discussion of the Origin of Insects. The general question of the Annulosa obviously includes that of Insects, and I therefore desire to correct this statement, and to refer your readers to a paper by me on Chœtogaster and Æolosoma, published in the "Linnean Transactions," vol. xxvi. (read Dec. 1867), in which I have more than alluded to Mr. Spencer's views, and have offered some suggestions on the morphology of the head, and as to the unisegmental Annulose ancestor. Mr. Wallace quotes from this paper in reference to Chœtogaster, though from the context it would appear that he is quoting from Professor Owen.

Since the researches which have rendered Mr. Wallace's name one of the first among living zoologists have not led him into practical anatomical and embryological studies, I may venture to add one or two strictures upon his statements relating to such matters. In the first place, those who are engaged in the study of insect embryology are not ignorant of Mr. Spencer's or similar views; the wide-spread study of his works in England and America, and of Haeckel's general morphology in Germany, is sufficient guarantee of this. But even if it were as Mr. Wallace supposes, he has not, in the extract given in NATURE, shown at all how Mr. Spencer's views on aggregation are to influence the study of the embryology of insects. Of course, the general theory of somites has immense importance in all studies relating to the Annulosa, but in what way the particular form of it, due to Mr. Spencer, can influence conclusions drawn from the observation of the manner in which insects develop from the egg, Mr. Wallace does not explain. Whether, admitting or denying the truth of Mr. Spencer's or Prof. Haeckel's views, it would be equally conceivable, did the observed facts point in either direction—that the ancestry of insects is to be traced to a simple nauplius-form or to a multi-segmental Annelid-like progenitor, the question of segmentation is not finally settled, though it is largely elucidated by the doctrine of Mr. Spencer. It is no doubt an instructive point of view to take—that segmentation is an arrested production of zooids, but it is equally true that the production of zooids is an exaggerated segmentation. We have no grounds for assuming the one more than the other as the essential process; they are both phases of the same process. The fact appears to be that in certain masses of organised matter, on their reaching a certain limit of growth, "polarities," which were hitherto held in one system, break up into two and so on. The simplest case of this is cell-division, but whether the systems separate entirely, as in simple fission, or remain associated, as in the cleavage of the egg and in the segmentation of the Annulosa, depends on another factor, a cohesive or integrating force proper to the growing mass.

In the present state of knowledge upon the subject, the assumption adopted and held of so much importance by Mr. Wallace—that the Vertebrata do not exhibit a segmentation of the same kind as that of the Annulosa, is by no means justified. Though much of their jointed iterative structure may probably be due to that kind of adaptation which Mr. Spencer so justly distinguishes as "superinduced segmentation," yet that there is a fundamental bud-segmentation, or segmentation of growth identical with that of Annulosa, is in the very highest degree probable. And even as to the Chiton, which Mr. Wallace quotes from Mr. Spencer as quite certainly an example of superinduced segmentation, I think that had he examined the grounds for making such a statement, he would have hesitated. The larva of Chiton is identical with that of an Annelid, and its segmentation makes its appearance in the same way. Why should there not be segmented molluscs? It is necessary most constantly to bear in mind, when considering this matter of segmentation, the possibility of the partial or complete obliteration of segmental characters due to tertiary aggregation, and their modification in most various ways in the evolution either of an individual or of a group.

Further, as to Mr. Wallace's expressions with regard to the segmentation of insects. From what was said above as to the relation of segmentation and zooid production, it follows that the conception of segmentation is erroneous which leads to ascribing to insects peculiar physiological or psychical properties on account of their being composed of "a number of individualities fused into one." This expression should not be allowed to lead to wider conclusions than those it formulates. As a matter of fact, insects are not a number of individualities fused into one, but rather one individuality partially (and as a reminiscence rather than actually) broken up into many, this partial breaking up being due to the mechanical properties of its tissues at a certain period of development.

If, by the "spiracles" of Annelids, Mr. Wallace means the segmental organs, it should be clearly stated that the identity of these with the tracheæ of insects has not yet been in any way proved. The comparison of the mode of development of these two sets of organs is just one of the points upon which embryologists are now at work.

Lastly, the researches of the last fifteen years do not, I venture to submit, lead to the conclusion adopted by Mr. Wallace, that the parthenogenesis of the higher Annulosa is analogous to or identical with gemmation as opposed to sexual reproduction or digenesis, but to the conclusion which is exactly opposed to this, namely, that it is identical with digenesis in all particulars but the absence of the male element.

Naples E. RAY LANKESTER

Adaptive Coloration, Phosphorescence, &c.

NO one who has watched a very young hare stealing from a green covert to brown soil, and observed its cunning movements there when alarmed, can for a moment doubt the value of colour as a protection to the higher animals.

The remarks by Mr. E. S. Morse in NATURE of last week bring to my recollection a good instance (among invertebrates) which occurs on the reddish granite of Cobo Bay, Guernsey. There *Trochus lineatus* especially abounds on the bare parts of the rocks between tide-marks; and every observer must be at once struck by the remarkable fitness of the mollusk for its peculiar site.

Mr. Darwin in truth says,* "It would not, for instance, occur to any one that the perfect transparency of the Medusæ or jelly-fishes, was of the highest service to them as a protection; but when we are reminded by Häckel that not only the Medusæ, but many floating mollusca, crustaceans, and even small oceanic fishes, partake of this same glass-like structure, we can hardly doubt that they thus escape the notice of pelagic birds and other enemies;" but he makes no mention of the gorgeous colouring of some of these swimming jellies, nor is there any allusion to their remarkable property of phosphorescence. The transparency of the British Salpæ does not prevent their being attacked by sea-birds, which hover in multitudes over them, masses of Medusæ and other Hydrozoa, and a few minute fishes.

If instead of promulgating the visionary idea that the abysses of the ocean depended for their light on phosphorescent animals, the dredgers† in the *Porcupine* had applied the notion that the various luminous marine animals used their light to attract each other, so that the most luminous might have a better chance of continuing the race, they would have been able to say more in its favour, without, at least, running counter to established facts.

Murthly, March 26 W. C. McINTOSH

The Aurora of February 4 ‡

AN aurora of a very unusual splendour for the latitude was seen here on Sunday evening February 4, 1872. The sky, extending in azimuth over 197° from N.E. to nearly W.S.W., was generally illuminated. The brilliance of the glow varied considerably in different directions from time to time during the night. On the south horizon there was a bright bluish segment of light, whose position in azimuth and brilliance varied slightly from time to time. The streamers were well seen, and their convergence towards the point to which the south pole of a magnet is directed could be most distinctly traced. The streamers extended at about nine o'clock to the constellation Orion, and Sirius was well within the auroral glow. With a spectroscope I saw one bright line in the spectrum of the auroral light, but the spectrum was too faint to allow of any successful attempt to determine the refrangibility of the light. Unfortunately our magnetical equipment is such that I can give no information respecting the extent of the magnetical disturbance at the time. The aurora was seen as far north as Bloemfontein, latitude 29° 8′ south. A faint aurora was seen here in October 1870, but no such aurora as that of February 4, 1872, appears to have been visible for at least fifty years. The aurora was well seen over a large portion of the colony, and considerably frightened the natives.

E. J. STONE

Royal Observatory, Cape of Good Hope, Feb. 19

* "Descent of Man," vol. i., p. 322.
† Not, however, Mr. Jeffreys
‡ Communicated by the Astronomer Royal.

SEEING your account of the aurora of February 4 in NATURE of the 22nd, reminds me that on the evening of the 4th I was riding from Cambridge to Coldwell, in Ohio, and between six and seven o'clock saw a most brilliant display of auroral light in the southern quarter of the sky. Brilliant streamers shot up past the zenith, while the whole southern portion of the sky was brightly illuminated with a corruscating rose-coloured light.

Marietta, Ohio., March 15 A. J. WARNER

Morse on Terebratulina

I HAVE just read the very kind notice of my paper* in the pages of your journal from the pen of Mr. E. R. Lankester. I hasten, however, to remove one impression conveyed in the following sentence, respecting the opinions I hold as to the Annelidan affinities of the Brachiopods:—

"We are not sure whether Mr. Morse adheres to this startling proposition."

I trust the long delay in publishing the results of my studies on this interesting class will lead no one to suppose that I have yet seen reason to modify the position I took two years ago regarding their position in the animal kingdom. On the contrary, continued investigation has brought out many new points of interest, and now I hope, ere my paper is published, to present the embryology of some one of them.

I had studied our native Terebratulina, its structure, as well as its early stages, and through the kindness of Prof. Verrill, had studied *Discina lævis* (upon which I hope soon to publish).

Mr. Lankester, as the author of many valuable memoirs requiring much skill and patient labour, will fully appreciate the time and care necessary in work of this kind.

As to my being unduly impressed at the sight of living Lingulæ, I may say, in justice to myself, and my friends will testify to it, my opinions were fully formed before I ever saw Lingula at all. With the caution that is requisite for every one, if he does not wish to supplement his paper with a correction of errors, a way of doing things altogether too frequent in this country, I deemed it important to study living Lingula before publishing. It was impossible for me to go half-way round the world for it. And as three specimens of another species have been found on the coast of North Carolina, I determined to go there. A trip of nearly a thousand miles brought me to its waste of drifting sands.

Thoroughly convinced as to the correctness of my views, and these views of sufficient strength to convince my co-labourers, Mr. Lankester will understand my enthusiasm when, after a week's fruitless search under a blazing sun, and an almost hopeless task, I found Lingula, not as we have always supposed attached by its peduncle, but living in the sand, precisely like many tubicolous worms, building a true sand tube, and when liberated from it crawling and burrowing by means of its setæ, and with all these welcome characters it should greet me with red blood. Not that I lay great stress on any one of these characters, but having made my deductions from the most common form, Terebratulina, one can readily understand the bearing of such unexpected characters in this little Lingula.

Mr. Lankester will admit that the Vermian lumber-room has some orderly compartments; into one of those I place the Brachiopods far away from all Molluscan odours.

The distinguished naturalist, Prof. Steenstrup, informs me that he has long taught his classes at the University of Copenhagen that the Brachiopods were true Annelids, and that my views are thoroughly endorsed by him. To him, therefore, and not to me as had been supposed, belongs the priority of this discovery.

I only ask a little patience till my complete paper is published on the Brachiopods as a division of Annelida, in which I shall give appropriate figures, and my reasons in full for the position I have taken. EDWARD S. MORSE

Salem, Mass., U.S.A., March 14

* "Early Stages of Terebratulina."

On the Colour of a Hydrogen Flame

WHEN hydrogen and oxygen are burned together, it is well known that the flame produced is almost non-luminous; it always, however, exhibits an unmistakeably blue tinge.

The small illuminative power is generally referred to the "absence of solid particles." This view, it appears to me, draws a too rigid line of demarcation between the atoms of carbon in an ordinary gas-coal flame and the atoms of hydrogen in that of the oxyhydrogen. The cause of the phenomenon does not depend so much on the *solidity* as it does on the time of oscillation of the particles which constitute the flame. Water particles in all their states of aggregation preserve the same time of oscillation—extra red; hence a hydrogen flame should be perfectly invisible whatever may be the "solidity" or density of its particles.

But the flame is not invisible, and, what is still more remarkable, the colour which it does exhibit is found to belong to the most refrangible end of the spectrum. To explain this strange phenomenon, it appears to me that it is necessary to invoke a state in the ether particles similar to that which Helmholtz has shown to exist in air; and which is this:—A tuning-fork "vigorously struck against a pad emits the *octave* of its fundamental note." Now, the first overtone of a tuning fork is produced by vibrations about $6\frac{1}{4}$ times as rapid as the fundamental; the octave, therefore, is not an overtone of the fork—it is produced solely in consequence of the fact that the initial disturbance is great in proportion to the distance of the air particles from one another, secondary waves being produced whose periods are twice as rapid as those of the fundamental.

The amplitude of the particles in a hydrogen flame is known to be very great, and hence it seems probable that an effect may result from the disturbance thus created in the ether, analogous to that in the case of air, *i.e.*, associated with the fundamental vibrations of the hydrogen flame we have their *octave*, which would obviously be within the visual range, and correspond very closely, if not *exactly*, with the colour actually observed.

Should this surmise prove correct we have plainly an easy means by which we can determine the wave-length of those extra-red rays which are absorbed by water.

A. G. MEEZE

Hartley Institution, Southampton, March 26

P.S.—May not the great *actinic* power of the electric light be due in a great measure to the secondary waves produced by the magnitude of the disturbing force?

VESTIGES OF THE GLACIAL PERIOD IN NORTH-EASTERN ANATOLIA

ATTENTION was drawn to this subject in a lecture given on March 25 at the Royal Geographical Society by the Eastern traveller Mr. W. Gifford Palgrave, at present British Consul for the northern coast of Asia Minor. The facts which he mentioned had been principally observed by him during a tour on duty to the interior about two years ago; and the line of route lay from the town of Trebizond on the sea coast to that of Erzinghian on the Upper Euphrates.

The phenomena themselves were divided into two classes: the one referable to the highlands which he had then traversed, the other to their marginal region.

These highlands are situated on or near the 40th parallel of latitude, and extend between the 37th and 44th of longitude, east and west; their average breadth being about fifty miles, and their elevation varying from 3,000 to 9,000 feet above the sea. They constitute the great watershed of Eastern Anatolia; the rivers to the south of them flowing into the Persian Gulf, and those to the north into the Black Sea. To the west is the basin of the Halys, to the east that of the Caspian.

The road leading across this plateau towards Erzinghian, mounts up to it by a defile named "Ketcheh-Dereh," or "Goats' Valley." Here, at a height of about 5,400 feet above the sea, Mr. Palgrave came on the lower extremity of a large moraine, piled up to a height of more than twenty feet, and broad in proportion. Following it for a distance of nearly half a mile, he found that when it had reached between 400 and 500 feet higher up the slope, it forked into two lesser branches, continued each a good way further into the rising undulations of the table-land.

The plateau itself bore every mark of having lain under a thick ice-coating; its eminences and irregularities all bearing the "moutonnée" character impressed by glacial action; while it was also frequently strewn with detached boulders and pieces of rock, scratched and scored with the unmistakeable lines that glaciers alone produce. These phenomena he observed to be repeated, or rather continued, throughout the highland, which he crossed three times at intervals, including above 100 miles of its length.

About the midmost of the plateau stands a solitary, dome-like eminence, nearly 8,000 feet above the sea level, and rounded off in every direction. On the west side of this mountain, now known as "Yelish Dagh," near its base, Mr. Palgrave found a second moraine, consisting of a single stone bank five or six hundred yards in length, stretching down to a valley below: its higher extremity was at about 6,500 ft. And lastly, at the great cleft about fifty miles distant, called the Cherdakh Pass, and leading downwards from the plateau into the Euphrates valley, he observed a third moraine, larger than either of the two former, and extending over a slope of fully 2,000 ft., its base being only about 4,500 ft. above the sea.

From these and similar indications, Mr. Palgrave conjectured that during the glacial period an ice-cap of fifty miles in average breadth, and many hundred in length, must have covered this table-land from a height of 6,000 ft.. or rather less, upwards; while some of the more advanced glaciers may have reached to a far lower level, seemingly 4,000 ft.

Such were the most remarkable surface-phenomena of the plateau itself. But on its margin, whether north or south, and connected with it, were other indications of an analogous character. These consisted in the traces afforded by broad and deep ravines and neighbouring river beds, much too wide for the streams that flow through them; all affording evidence of a past epoch when the water supply was on a far more copious scale than it is now. Thus the valley of the Euphrates itself, which takes its rise in this very plateau, is, in its evenly-scooped extent of three and even four miles across, out of all proportion with the comparatively little and feeble stream that now meanders along it; and the same must be said of most of the aqueous modifications imprinted in the lower mountain ranges, and in the plains at their feet.

But of all the phenomena of this kind none is more remarkable than that inspected by Mr. Palgrave near the sea-end of the great valley by which the river, once Pyxartes, now "Deyermend-Dereh," or "Mill Stream" enters the Euxine, close by Trebizond. This river, whose waters are derived from the central table land, is now so shallow as to be readily fordable at almost every season of the year, and brings down with it just enough pebble and soil to form a little bar at its mouth. Half a mile, however, from the present beach the river valley, here about a third of a mile in width, is in its greater part crossed by a huge bar of rolled stones, at least forty feet in eight, and eighty or a hundred yards in thickness at its base, evidently formed here by the joint action of river and sea. The stones, many of which are of great size, belong to Jurassic or Plutonic formations, such as compose the plateau inland, whereas the coast-rock is entirely volcanic. But the flood of water requisite to bring them from such a distance is now wholly wanting. Nor can its diminution be ascribed to the extirpation of forest wood, for the mountain chain is still as densely clothed with trees as it could ever have been in remote times; nor yet to an alteration in the course and dip of the valleys that unite to send their supplies hither, for there is no trace of any great geological change hereabouts within the epoch to which the bar itself is referable. One only cause there could have been capable of furnishing so impetuous a stream, namely, the periodical melting of great masses of ice and snow on the mountains behind, now unusually bare of snow from June till November, and absolutely denuded of anything approaching to a glacier. When these icy reservoirs ceased the abundance of the river ceased also, leaving the bar alone as a monument of its former strength.

T. P.

THE INHABITANTS OF THE MAMMOTH CAVE OF KENTUCKY

Crustaceans and Insects

THE following account of the inhabitants of the Mammoth Cave of Kentucky is abridged from the *American Naturalist*. To the courtesy of the editors of that journal we are further indebted for the accompanying illustrations:—

After the adjournment of the meeting of the American Association for the Advancement of Science, held at Indianapolis in August last, a large number of the members availed themselves of the generous invitation of the Louisville and Nashville Railroad Company, to visit this world-renowned cave, and examine its peculiar formation and singular fauna.

The cave is in a hill of the subcarboniferous limestone formation in Edmondson County, a little to the west and south of the centre of Kentucky. Green river, which rises to the eastward in about the centre of the State, flows westward, passing in close proximity to the cave, and receiving its waters, thence flows north-westerly to the Ohio. The limestone formation in which the cave exists is a most interesting and important geological formation, corresponding to the mountain limestone of the European geologists, and of considerable geological importance in the determination of the western coalfields.

We quote the following account of this formation from Major S. S. Lyon's report in the fourth volume of the "Kentucky Geological Survey," pp. 509, 510:—

"The sinks and basins at the head of Sinking Creek exhibit in a striking manner the eroding effects of rains and frost—some of the sinks, which are from 40 ft. to 190 ft. deep, covering an area of from 5 to 2,000 acres. The rim of sandstone surrounding these depressions is, generally, nearly level; the out-cropping rocks within are also nearly horizontal. Near the centre there is an opening of from 3 ft. to 15 ft. in diameter; into this opening the water which has fallen within the margin of the basin has been drained since the day when the rocks exposed within were raised above the drainage of the country, and thus, by the slow process of washing and weathering, the rocks which once filled these cavities have been worn and carried down into the subterranean drainage of the country. All this has evidently come to pass in the most quiet and regular manner. The size of the central opening is too small to admit extraordinary floods; nor is it possible, with the level margin around, to suppose that these cavities were worn by eddies in a current that swept the whole cavernous member of the subcarboniferous limestone of western Kentucky; but the opinion is probable that the upheaving force which raised these beds to their present level at the same time ruptured and cracked the beds in certain lines: that afterwards the rains were swallowed into openings on these fractures, producing, by denudation, the basins of the sinkhole country, and further enlarging the original fractures by flowing through them, and thus forming a vast system of caverns, which surrounds the western coalfield. The Mammoth Cave is at present the best known, and therefore the most remarkable."

So much has been written on the cave and its wonders, that to give a description of its interior would be superfluous in this connection, even could we do so without unintentionally giving too exaggerated statements, which seems to be the natural result of a day underground, at least so far as this cave is concerned, for, after reading any account of the cave, one is disappointed at finding the reality so unlike the picture.

We are indebted to Prof. Alexander Winchell, of the University of Michigan, for the following abstract of his views concerning the formation of the cave:—

"The country of the Mammoth Cave was probably dry land at the close of the coal period, and has remained such, with certain exceptions, through the Mesozoic and Cænozoic ages, and to the present. In Mesozoic times, fissures existed in the formation, and surface waters found

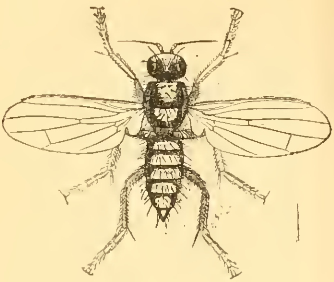


FIG. 1.—Anthomyia.

their way through them, dissolving the limestone and continually enlarging the spaces. A cave of considerable dimensions probably existed during the prevalence of the

FIG. 2.—Phora.

FIG. 4. Adelops hirtus.

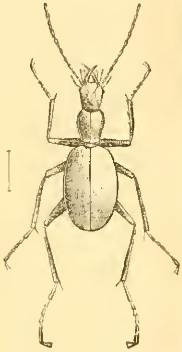


FIG. 3.—Anophthalmus Tellkampfii.

continental glaciers over the northern hemisphere. On the dissolution of the glaciers, the flood of water which swept over the entire country, transporting the materials

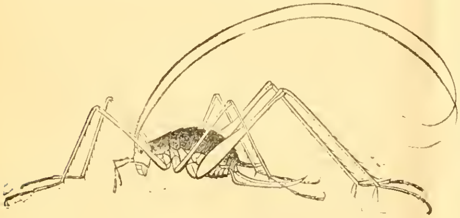


FIG. 5.—Hadenœcus subterraneus.

which constituted the modified drift, swept through the passages of the cave, enlarging them, and leaving deposited in the cave some of the same quartzose pebbles which characterise the surface deposits from Lake Superior to the Gulf of Mexico. Since the subsidence of the waters of the Champlain epoch, the cave has probably undergone comparatively few changes. The well, 198 ft. deep, at the farther end of the cave, shows where a considerable

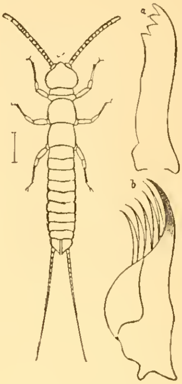


FIG. 6.—Campodea.

volume of the excavatory waters found exit. The Mammoth Dome indicates probably both a place of exit and a place of entrance from above. So of the vertical passages in various other portions of the cave."

We believe that the views of Prof. Winchell are in harmony with those of the other eminent geologists of the party; and when it is considered that the geologists of this excursion stand in the front rank of the most eminent scientific men of the world, their views upon this interesting subject are well worthy of attention.

FIG. 7.—Anthrobia monmouthia.

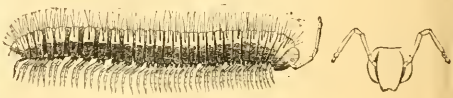


FIG. 9.—Spirostrephon Copei.

With these general remarks on the cave, we give a brief account of its interesting fauna, comprising representatives of the insects and crustaceans. No molluscs or radiates have as yet been discovered; but the lower forms of life have been detected by Tellkampf, who collected several species belonging to the genera *Monas*, *Chilomonas*, and (?) *Chilodon*.

Representatives of all the grand divisions of the insects and crustaceans have been found in this cave, and if no worms have yet been detected one or more species would undoubtedly reward a thorough search.

We will enumerate what have been found, beginning with the higher forms. No Hymenoptera (bees, wasps, and ants) or Lepidoptera (moths) are yet recorded as being peculiar to caves. The Diptera (flies) are represented by two species, one of Anthomyia (Fig. 1), or a closely allied genus, and the second belonging to the singular and interesting genus Phora (Fig. 2.) The species of Anthomyia usually frequent flowers; the larvæ live in decaying vegetable matter, or, like the onion fly, attack healthy roots. It would be presumptuous in the writer to attempt to describe these forms without collections of species from the neighbourhood of the cave, for though like all the rest of the insects they were found three or four miles from the mouth, yet they may be found to occur outside of its limits, as the eyes and the colours of the body are as bright as in other species.

Among the beetles, two species were found by Mr. Cooke. The *Anopthalmus Tellkampfii* of Erichson, a Carabid (Fig. 3), and *Adelops hirtus* Tellkampf (Fig. 4), allied to Catops, one of the Silphidæ or burying beetle family. The Anopthalmus is of a pale reddish horn colour, and is totally blind;* in the Adelops, which is greyish brown, there are two pale spots, which may be rudimentary eyes, as Tellkampf and Erichson suggest. No Hemiptera (bugs) have yet been found either in the caves of this country or Europe. Two wingless grasshoppers (sometimes called crickets) like the common species found under stones (*Ceuthophilus maculata* Harris), have been found in our caves; one is the *Hadenœcus subterraneus* (Fig. 5 nat. size) described by Mr. Scudder, and very abundant in Mammoth Cave. The other species is *H. stygia* Scudder, from Hickman's cave, near Hickman's landing, upon the Kentucky river. It is closely allied to the Mammoth Cave species. According to Mr. Scudder the specimens of *H. stygia* were found by Mr. A. Hyatt "in the remotest corner of Hickman's Cave, in a sort of a hollow in the rock, not particularly moist, but having only a sort of cave dampness. They were found a few hundred feet from the sunlight, living exclusively upon the walls." Even the remotest part of that cave is not so gloomy but that some sunlight penetrates it.

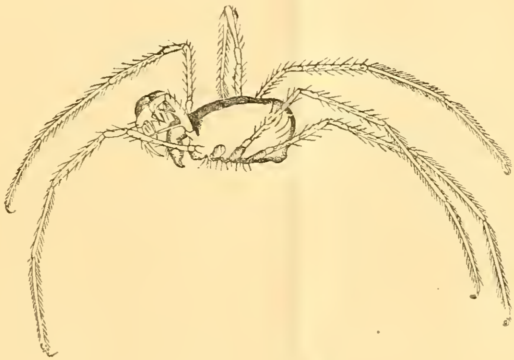


FIG. 8 —Acanthocheir armata.

The other species is found both in Mammoth Cave, and in the adjoining White's Cave. It is found throughout the cave, and most commonly (to quote Mr. Scudder) "about 'Martha's Vineyard" and in the neighbourhood of 'Richardson's Spring' where they were discovered jumping about with the greatest alacrity upon the walls, where only they are found, and even when disturbed, clinging to the ceiling, upon which they walked easily; they would leap away from approaching footsteps, but stop at a cessation of the noise, turning about and swaying their long antennæ in a most ludicrous manner, in the direction whence the disturbance had proceeded; the least noise would increase their tremulousness, while they were unconcerned at distant motions, unaccompanied by sound, even though producing a sensible current of air; neither did the light of the lamp appear to disturb them; their eyes, and those of the succeeding species (*H. stygia*), are perfectly formed throughout, and they could apparently see with ease, for they jump away from the slowly approaching hand, so as to necessitate rapidity of motion in seizing them."

The Thysanurous Neuroptera are represented by a species of Machilis, allied to our common *Machilis variabilis* Say, common in Kentucky and the middle and southern States. So far as Tellkampf's figure indicates, it is the same species apparently, as I have received numerous specimens of this widely distributed form from Knoxville, Tennessee, collected by Dr. Josiah Curtis. It was regarded as a crustacean by Tellkampf, and described under the name of *Triura cavernicola*.† He mistook the labial and maxillary palpi for feet, and regarded the nine pairs of abdominal spines as feet. The allied species, *M. variabilis* Say, is figured in vol. v. pl. 1, figs. 8, 9.

* In Erhardt's cave, Montgomery Co, Virginia, Prof. Cope found "four or five specimens of a new Anopthalmus the *A. pusio* of Horn, at a distance of not more than three hundred feet from its mouth. The species is small, and all were found together under a stone. Their movements were slow, in considerable contrast to the activity of ordinary Carabidæ." Proc. Amer. Phil. Soc. 1869, p. 178.

† Prof. Agassiz, in his brief notice of the Mammoth Cave animals, does not criticise Tellkampf's reference of this animal to the crustacea; and so eminent an authority upon the articulates as Schiödte remarks, while "Dr. Tellkampf's account affords us no means of forming any conclusion as to its proximate relations," that, however, it "appears to belong to the order of Amphipoda, and to have a most remarkable structure" Tellkampf's figure of Machilis is entirely wrong in representing the labial and maxillary palpi as ending in claws, thus giving the creature a crustacean aspect, and, indeed, he describes them as true feet!

An interesting species of Campodea, of which the accompanying cut (Fig. 6) is a tolerable likeness, though designed to illustrate another species (*S. staphylinus* Westw.) was discovered by Mr. Cooke. Both the European and our common species live under stones in damp places, and the occurrence of this form in the water is quite remarkable. The other species are blind, and I could detect no eyes in the Mammoth Cave specimen.

A small spider was captured by Mr. Cooke, but afterwards lost; it was brown in colour, and possibly distinct from the *Anthrobia monmouthia* Tellkf. (Fig. 7), which is an eyeless form, white and very small, being but half a line in length. The family of Harvest men is represented by a small white form, described by Tellkampf under the name of *Phalongodes armata* (Fig. 8), but now called *Acanthocheir armata* Lucas. The body alone is but half a line long, the legs measuring two lines. It should be borne in mind that many of the spiders, as well as the Thysanura, live in holes and dark places, so that we could naturally find them in caves. So, also, with the Myriopods, of which a most remarkable form (Fig. 9 front of head) was found by Mr. Cooke three or four miles from the mouth of the cave. It is the only truly hairy species known, an approach to it being found in *Pseudotremia Vudii* Cope. It is blind, the other species of this group which Prof. Cope found living in caves having eyes. The long hairs arranged along the back seem to suggest that they are tactile organs, and of more use to the thousand legs in making its way about the nooks and crannies of a perpetually dark cave than eyes would be. It was found by Mr. Cooke under a stone.

Prof. Cope has contributed to the "Proceedings of the American Philosophical Society" (1869, p. 171) an interesting account of the cave mammals, articulates, and shells of the middle states. He says that "myriopods are the only articulates which can be readily found in the remote regions of the caves (of West Virginia) and they are not very common in a living state." The *Pseudotremia cavernarum* which he describes, "inhabits the deepest recesses of the numerous caves which abound in Southern Virginia, as far as human steps can penetrate. I have not seen it near their mouths, though its eyes are not undeveloped, nor smaller than those of many living in the forest. Judging from its remains, which one finds under stones, it is an abundant species, though rarely seen by the dim light of a candle even after considerable search. Five specimens only were procured from about a dozen caves." The second species, *P. Vudii* Cope, was found in Montgomery Co., and he thinks it was not found in a cave. Prof. Hyatt informs me that he saw near the "Bottomless Pit" in Mammoth Cave, a brownish centipede-like myriopod, over an inch in length, which moved off in a rapid zigzag motion. Unfortunately, he did not capture it.

A. S. PACKARD, JUN.

(*To be continued*)

NOTES

WE have received information of a most munificent act on the part of that veteran in Geological Science, Sir William E. Logan, in supplementing, by the handsome gift of 18,000 dols., the sum of 2,000 dols. given by him and his brother, Mr. Hart Logan, last year towards the endowment of the Chair of Geology in M'Gill University, Montreal. The "Logan Chair of Geology" will be at once a commemoration of Sir William's name in connection with the higher education of our colonists, and a means of perpetuating the teaching of the Science for which he has done so much, as well as of securing the training of a succession of young men who may worthily follow up his investigations in the wide field of Canadian Geology. Principal Dawson, who at present occupies the Chair of Geology, will be the first "Logan Professor," and it is intended that the endowment shall, as soon as possible, be made the means of relieving him from the teaching of some other portions of natural science, in order that he may more fully devote his time to Geology and Palæontology.

PROF. HUXLEY is now on his way home to England, having been last heard of from Naples. His health is very greatly restored by his absence from work, and the effects of the Egyptian climate.

DR. M'NAB, Professor of Botany and Geology at the Royal Agricultural College, Cirencester, has been appointed Professor of Botany to the Royal College of Science and Art, Dublin, in the place of Prof. Thiselton Dyer. The appointment is a good one, on which we congratulate the Science and Art Department. The lectureship at the Cirencester College is now vacant.

THE death of the Swiss palæontologist, M. Pictet de la Rive, Professor in the Academy of Geneva, which we noticed last week, took place on the 15th ult. at the age of sixty-two, and was occasioned by fever induced by a severe accident.

DR. GEORGE BURROWS, F.R.S., has been re-elected President of the College of Physicians.

PROF. HUXLEY was defeated by a small majority by Lord Neaves in the election for the Rectorship of St. Andrew's University. Prof. Sylvester was also, we regret to say, unsuccessful in his candidature for the School Board for Marylebone. We understand, however, that there will shortly be another vacancy on the Board, when we trust Science will once more put in its claim.

THE Brighton Aquarium was formally opened to the public on Saturday last.

IT has been decided to give a private view and evening reception in the Picture Galleries of the London International Exhibition of 1872 before the 1st of May, to which distinguished foreigners and holders of season tickets will be invited.

AT Rugby Mr. Wilson and Mr. Seabroke have tried the experiment of giving regular lectures on Astronomy to a class consisting of volunteers from the school and residents in the town. Note-books were shown up and corrected, and an examination held at the end. About seventy attended, twenty being members of the school; thirty showed up note-books, and eighteen presented themselves for examination. Advocates of women's education will be pleased to learn that the two best note-books were written by girls, and that in the examination, which was a stiff one (we have seen the paper), girls held the second, third, and fifth places. The proceeds are for buying books for the Temple Observatory.

AN organisation, entitled the Bloomington Scientific Association, was instituted at Bloomington, Illinois, in 1871, having for its object the diffusion and popularising of science in that State. The officers are Prof. B. S. Perry, Mr. R. H. Holder, Dr. Vasey, and Mr. J. A. Jackman. The society has already a large number of members, and meets frequently.

THE great depression of temperature during November and the early part of December, was followed by an extraordinarily long period of more than three months' remarkably mild weather. For the ninety-seven days from December 13 to March 18, Mr. Glaisher's Greenwich tables, recorded weekly in the *Gardener's Chronicle*, show that the temperature was above the average on eighty-nine, and below the average on only eight days, the mean excess for the whole period being 5°·1. During the whole of this period the thermometer fell below the freezing point on four nights only, viz., January 15 and 16, and March 10 and 11; the lowest temperature recorded being 28°·3 Fahr. on the first and last of these dates.

February, it will be seen, was entirely free from frost, the minimum for that month being 32°·4, on the 28th. The warmest period was from March 1 to 8, when the maximum temperature ranged each day from 57°·1 to 60°·8. It will be interesting to know whether so long a period of exceptionally high temperature, including fifty-three consecutive days entirely free from frost, has ever been recorded before in the depth of winter. On March 19 the average temperature of the day fell below the mean, and continued so for nine days, till the 27th. The minimum temperature for March was on the 21st, 26°·2 Fahr., being the lowest recorded since Dec. 9. There were nine frosty nights in March, against the two in the whole of the two preceding months. For the week ending March 26 the mean temperature was 34°, or 16° lower than the mean for the week ending March 7.

A CORRESPONDENT of *The Blue* strongly urges the desirability of the formation of a Natural History Society at Christ's Hospital; and the editor of that magazine promises his assistance to the proposal. We heartily wish it success.

THE proposed Act for appropriating the Yellowstone Park for public purposes (to which we recently referred), has passed the Congress of the United States. The following are extracts from the Bill :—"That the tract of land in the territories of Montana and Wyoming (as already described) is hereby reserved and withdrawn from settlement, occupancy, or sale under the laws of the United States, and dedicated and set apart as a public park or pleasuring-ground for the benefit and enjoyment of the people. That said public park shall be under the exclusive control of the Secretary of the Interior, whose duty it shall be, as soon as practicable, to make and publish such rules and regulations as he may deem necessary or proper for the care and management of the same. Such regulations shall provide for the preservation from injury or spoliation of all timber, mineral deposits, natural curiosities, or wonders within said park, and their retention in their natural condition. The Secretary may, in his discretion, grant leases for building purposes for terms not exceeding ten years, of small parcels of ground, at such places in said park as shall require the erection of buildings for the accommodation of visitors; all of the proceeds of said leases, and all other revenues that may be derived from any source connected with said park, to be expended under his direction in the management of the same, and the construction of roads and bridle paths therein. He shall provide against the wanton destruction of the fish and game found within said park, and against their capture or destruction for the purposes of merchandise or profit." Such a step in the interest of science deserves more than a passing recognition from this side the water.

THE *British Medical Journal* prints the following admirable reply to the extraordinary article which appeared in the *Saturday Review* of the 16th ult., on Dr. Liebreich's lecture on "Turner and Mulready," which we gave last week :—"It is not, of course, always to be expected that *Saturday* Reviewers should have a very profound knowledge of their subjects; but it might be thought advisable that an analysis of an optical argument should not be publicly undertaken by a gentleman who is ignorant of the first rudiments of the subject, and so little acquainted with even the alphabet of its language as the gentleman who discusses, in the last *Saturday Review*, the visual defects of Turner and Mulready. He pronounces a 'verdict of not proven' on Mr. Liebreich's argument; and his fitness for appreciating a discussion of the effects of yellow discoloration of the lens, occurring with advancing old age, on Mulready's perception of colour, may be estimated by the following sentence: 'Let us suppose a person to put on a pair of yellow spectacles. The effect is *assumed* to be, and we think *correctly*, that the *yellow* in a landscape or in a picture, unless extra strong, would be *scarcely recognised;* and that the *blues also*, unless very decisive, would be *neutralised*. The consequence seems to follow, that the painter would throw *ultra force into both yellow and blue:* though against this supposition it must not be forgotten that the spectacle or the crystalline lens, as the case may be, would discolour precisely in *the same degree the tones in nature and the pigments on the palette*.' The italics are ours. There is scarcely a word in this astonishing statement which is not entirely a mistake. It was not assumed, but it is known, that, seen through a yellow glass, the yellows in a landscape are seen relatively more strongly, while the blues are partly neutralised. It was not assumed that the effects of viewing a landscape and a picture through a yellow lens or glass are the same; but, on the contrary, it was stated, as the result of experiment, that they are entirely different. The retina becomes presently so far accustomed to the yellow medium, that the strong lights reflected from blue surfaces in nature overpower the yellowness of the medium, and the blues of a landscape are presently but little neutralised. The reflections from pigments, poor imitations as as they are, at the best, of nature, have not the same power; a large part is neutralised by the yellow glass or lens: and to produce with pigments, on a canvas, blues which satisfy his eye as comparable with those which he sees in nature, the painter—who in old age has the pigment-yellowness of senile change in the lens—employs much deeper blues than he would have done in youth, or than impress youthful eyes as representing the natural tints truthfully. That is why, on Liebreich's theory, Mulready, in painting the same picture in old age which he had painted in middle life, introduced ultramarine into the flesh tints—painted a linen smock of the brilliancy of a glittering silk; and that is the key which he affords to the prevailing excess of purple tints which the official catalogue describes as characterising the latest works of this great artist. The great master himself was, in his later life, dissatisfied for this reason with the colour of his earlier works; he thought them too brown, and used to warn his pupils to paint with stronger blues, especially in the grey shadows.'

IN a letter addressed by von Heuglin to Middendorff, of the St. Petersburg Academy, we find the fullest details of the explorations instituted by that eminent traveller during the past summer in the Nova Zembla seas. In this he remarks that the original plan included a visit to the mouths of the Obi and Yenisei, perhaps even extending to the island of New Siberia. This, however, was found to be impracticable on account of unseasonable weather, as it was not till the 6th of August that they reached the Straits of Matotschkin. Up to that time they had met with no ice; but after passing the straits to the east there was very much drift ice from the sea of Kara so as almost to bar their way. Finding that the northern coast of the island was entirely embargoed by ice, they turned to the south, and in passing visited the Straits of Kostin and the Nechwatowa, then Waigatsch, and finally arrived at the Straits of Jugorsky on the 1st of September. Here the expedition did not make any better progress than in the Straits of Matotschkin, and fearing that they might be shut in by the ice for the winter, they returned to their starting-place. Among the more important results of the voyage were numerous soundings and measurements of deep-sea temperatures, as also various geographical determinations; while large collections of specimens of natural history were brought together. Among these the most interesting was the discovery of two different species of lemming in Nova Zembla, and it was thought possible that in Southern Nova Zembla still a third species might be met with. The same animal was also found in Spitzbergen. Numerous birds were obtained in Nova Zembla and Waigatsch; among them the Mandt's Guillemot. Of fishes, some species of cod, cottus, and salmon were obtained, and about one hundred species of plants.

INDIAN papers give the following additional accounts of the aurora of February 4:—Such a phenomenon has not been observed in the Punjab, or perhaps in India, within the memory of man, and in consequence the remarks made by the natives and others born in the country were rather curious. A curious circumstance took place at Raikote. About 100 Kooka families turned out in the most excited state, and commenced those wild demonstrations from which the name *Kooka* is derived. The men tore off their turbans, unloosed their hair, and began dancing and waving their arms about, and shouting that this was a token that Ram Singh had returned to his home. They were much disappointed to learn that they were mistaken. At Sealkote many thought that the red in the sky was the reflection of the blaze of some hill forest on fire, and one individual at Jhelum suggested that it must be caused by some volcanic eruption in the Himalayas. In another place a commissariat officer was thrown into an agony of terror, thinking it was his haystacks on fire. A correspondent, writing from Madhopore, says:—"On the night of the 4th instant, between 11 and 12 o'clock, there appeared in the sky a clear bright light, like fire, which lasted about fourteen minutes. It was so bright that we were able to see even the minutest objects; owing to its red colour the river appeared as though it were blood. The atmosphere for days has never been clear of clouds, and it seems as if a storm were portending. The lightning injured some natives on the 5th inst."

A CORRESPONDENT suggests that the memory of Dr. Priestley will not be so worthily honoured by a bad statue as by a thoroughly well-appointed School of Science to be called "The Priestley Institution," or whatever other name be thought fitting. Science is much needed to supplement the technical skill employed in the industries of the Black Country, and is not in that district so well provided for as to render the establishment of such a school unneedful. Or if that undertaking be thought too vast, he proposes the endowment at the Newcastle College or elsewhere of a scholarship of Physical Science, to provide young aspirants from the Midland Counties with opportunities of scientific practice and culture. Or if this suggestion do not find favour, possibly the ingenuity of the committee can devise some scheme of a similar sort, so that thus the funds subscribed for this memorial may be used for science.

WE note the proposed formation of a National Swimming Baths Company (Limited), to provide good and cheap swimming baths in the Thames.

ACCORDING to a communication to the Geological Society of Hungary, the remains of a man, associated with post-tertiary remains of mammalia, together with a stone hammer, have lately been discovered in the loess deposits of Hungary, in the neighbourhood of Brux, in Bohemia. These were in nearly a complete condition. The cranium strongly resembles in its characteristics the well-known fragment from the Neanderthal, although differing in certain peculiarities mentioned in the article. The skeleton was found lying with the head raised, in a sand-bed of diluvial age, at a depth of two feet from the surface.

IN making an excavation on the banks of the Amoor River, *Harper's Weekly* states that a stone axe of nephrite, or jade, and beautifully finished, was found at a depth of about three feet. This fact is the more interesting as it bears upon the question in regard to the celebrated stone-tipped arrows which were used by the primeval inhabitants of Mantchuria as late as the twelfth century. It was with arrows winged with eagles' feathers and tipped with nephrite points that this people paid their tribute to China while they were under its dominion. The precise locality of nephrite in Mantchuria is unknown, although it is stated by some to have been on a mountain to the north-west of that country.

THE Perthshire Society of Natural Science held its Annual Meeting on March 7, when Colonel H. M. Drummond Hay was elected president in the room of Dr. Buchanan White, who has held the office for five years. This enterprising society must be congratulated on the work it has done in the exploration of the natural history of the county, and in the commencement of the publication of so valuable a work as the *Fauna Perthensis*, and the promotion of so useful a periodical as the *Scottish Naturalist*. Botany seems, however, up to the present time, to have been neglected by the Society, which is to be regretted in a county with so rich and interesting a flora. The Society has also held "a meeting for investigation into the qualities, as articles of food, of certain Perthshire animals," commonly known as a "Frog-supper." Among the articles of the bill of fare were—Pâté d'Ecureuil, Matelot de Grenouille, Alouette à la Crapaudine, Ecureuil au naturel.

AN Act passed by the Governor-General of India in Council in October last, with a view to provide for the ultimate adoption of a uniform system of weights and measures of capacity throughout British India, has been laid before the House of Commons. The Act directs that the unit of weight shall be a "ser," equal to the French kilogramme, and the unit for measures of capacity, a measure containing one such ser of water at its maximum density, weighed in a vacuum. Other weights and measures of capacity, to be authorised under this Act, are to be integral multiples or sub-multiples of their units, the sub-divisions to be expressed in decimal parts unless otherwise ordered. When proper standards have been provided for verification of these weights and measures to be used by any Government office, municipal body, or railway company, the Governor-General in Council may direct that the weights and measures as authorised shall be used in dealings by such office, body, or company. The local Government may prepare tables of the equivalents of other weights and measures in terms of the weights and measures so authorised.

DR. W. LAUDER LINDSAY announces as in preparation, "Mind in the Lower Animals," a popular exposition of those traits in the habits of animals that illustrate their possession of the higher as well as the lower faculties of mind, as it exists in man. Dr. Lindsay has already written extensively on the subject in the *Journal of Mental Science* and the *British and Foreign Medico-Chirurgical Review*.

THE second enlarged and improved edition is published of Dr. O. W. Thomé's "Lehrbuch der Botanik," intended especially for elementary classes of botany in gymnasia and public schools. Although some portions of the work, especially the systematic, are open to exception, yet it presents the elements of the different departments of botanical science in a more compact form, and at a lower price (3*s.*) than probably any other work. It is illustrated by nearly 900 woodcuts.

MR. SHIRLEY HIBBERD has in the press a volume entitled "The Ivy, a Monograph," which will shortly be published by Messrs. Groombridge and Sons.

A USEFUL catalogue is published at Ghent, entitled, "Nomenclature usuelle de 550 Fibres Textiles, avec indication de leur provenance, leurs usages," &c., by the conservator of the commercial-industrial museum in that city.

MESSRS. GROOMBRIDGE AND SONS are preparing a new edition, with coloured plates, of Mr. Lambton J. H. Young's "Sea Fishing as a Sport."

MR. B. S. LYMAN, mining engineer to the Public Works Department of the Government of India, reprints from the "Transactions of the American Philosophical Society" an account of the Punjab Oil Region, accompanied by a geological and topographical map.

ANNUAL ADDRESS TO THE GEOLOGICAL SOCIETY OF LONDON, FEB. 16, 1872

By J. Prestwich, F.R.S., President

(*Continued from page* 433.)

IT has been already mentioned that below a certain level permeable strata are necessarily always saturated and water-logged, and that any additional quantity added to this constant quantity cannot be held permanently. It follows that wherever, in all water-bearing strata, after allowing for any abstraction, usually but comparatively small, by wells, the surplus rainfall must, when the stratum is full, find its escape by natural means, *i.e.*, by means of springs. The power and size of these are necessarily dependent upon the dimensions of the strata by which they are supplied. In the gravel they are small, in the Lower Tertiary sands moderate; while in the Chalk they are very large. The permanence of the spring depends on the lithological character as well as on the dimensions of the strata. Thus, in sands, where the water can permeate the mass, the stores are large, and the delivery moderately quick; in Limestones, where the water is confined to cracks and fissures, the delivery is quick and not lasting, though often large; in rubbly Oolites, which are also practically porous, the springs are well maintained; while in Chalk, owing to the characters before named, the water-delivery is slow, and the springs are large and very permanent.

At the same time the storage-capacity increases with the resistance. Taking the extreme case of the Chalk, the transmission of the rain-water is so slow, that, on the chalk hills, it takes four or six months to pass from the surface to the line of water-level at the depth of 200ft. to 300ft., so that the heavy rainfall of winter is not felt in the deep springs until the summer, and Mr. Beardmore estimates that the minimum effect of a hot dry summer and autumn is not reached until at the end of about sixteen months, or that the storing-power of the chalk is of sixteen months' duration. To estimate this power, we have to take the height and extent of the hills, and to note the lithological characters of the permeable strata. If these latter are underlaid by impermeable strata at above the level of the rivers in two adjacent valleys, then the base of the underground water-store will be coincident with the level of the impermeable strata, and its surface-line will rise, as it recedes within the hill, in proportion to the resistance offered to the water's escape by the character of the permeable strata, and it will thus form a curve between those two points, the height of which will vary in proportion to the rainfall. When, on the other hand, the permeable strata continue down to a greater or less depth beneath the surface of the adjacent rivers, then, as there is no underground escape for the stored water, the line of water-level in those permeable strata will rise to, and be always maintained at, the level of the rivers, and therefore all the additional supplies furnished by the rain must, after traversing the interior of the hills, find an escape along the bottom of the valleys, and by the side or in the bed of those rivers In the dry upland valleys of the Chalk and Oolites, the underground water, dammed back by the streams in the nearest river-valley, passes under those valleys at depths varying with the resistance offered by the lithological character of the formation, and by the gradient of the valley as it runs into the hills.

When again, as in the case of the chalk downs and oolite hills, the exterior outcrop of the permeable strata rests on impermeable strata at a height above the river-levels, and in the other direction inwards they pass below similar levels, then the springs partake of the same divided character—the one smaller set flowing out on the sides of the hills, and the stronger and more lasting springs issuing, as it were, at the foot of the incline on the level of the rivers. In any case, it is the distance between the two points of escape that gives us one measure of storage. If the distance is reckoned by miles, then the rise of the water-level may be measured by tens of feet. It is highest when both the distance from the adjacent river-valleys, and at the same time the height of the hills is greatest. In some instances, the crown of the arch formed by it will rise to a height of from 60 ft. to 80 ft. above its chord.

This curve is subject to great fluctuation, varying according to the seasons and amount of rainfall. Mr. Clutterbuck has shown that, in the chalk hills of Hertfordshire, its height varies as much as 30 ft. or 40 ft. From the crown or centre of its summit it decreases at a rate varying generally from 3 ft. to 30 ft., or even more, per mile to all parts of the circumference. The height of the arch and the breadth of the base-line, taken together, give therefore the head of water supplying the large springs of the Chalk—such as those of Chadwell, Hoddesden, Otter, Carshalton, Leatherhead, Ospringe, and others. But, besides these, there are innumerable smaller ones, not so easily seen, flowing out on the sides, or in the beds of the rivers traversing the great permeable formations, as the many along the Thames from Greenhithe to Faversham, on the Upper Lea and its tributaries, and on the Medway and the Darent, where they traverse the chalk hills. This class of springs has especial geological bearings, which we shall hereafter have occasion to dwell upon.

The same general rules govern the springs of all the more varied strata of the upper part of the Thames basin, where, in place of the Cretaceous and Tertiary series, we have a series of Jurassic and Liassic strata. Omitting the drift or gravel beds, the following are the average dimensions, character, and superficial areas of each of these formations in that area :—

Strata of the Thames Basin above Wallingford

	Area.	Average Thickness.	
	Square miles.	Permeable strata.	Impermeable strata.
Chalk (above Kingston 1047)	60	1000	—
Upper Greensands ...	62	100	—
Gault	129	—	130
Lower Greensands ...	23	200	—
Purbeck and Portland beds	46	60	—
Kimmeridge Clay ...	132	—	300
Coral Rag and grit ...	103	40	—
Oxford Clay	307	—	400
Great and Inferior Oolites...	327	450	—
Fuller's Earth	16	—	40
Lias ...	170	—	500

But although many of these water-bearing strata are of large dimensions and well stored in the upper part of the Thames basin, none of those below the Gault, except the Lower Greensand, are available for a well-supply at London. The Upper Greensand, so important in Wiltshire, is reduced to a few feet of comparatively impermeable argillaceous sands under London. The Oolitic series, so rich in springs in the district of the Cotswold Hills, have been ascertained to thin off as they range eastward; and Mr. Hull has shown that the inferior Oolite and underlying sands in particular die out, in all probability, under the Oxford clay about the centre of Oxfordshire. Even apart, therefore, from the discovery made at Kentish Town, we should now have excluded the Oolitic series as a possible source of supply to deep wells in the London district; although, as sources of springs' supplies, they contribute so important a share to the maintenance of the Thames. Few of those strata are, however, so homogeneous as the Chalk and the London Clay. The permeable formations often contain subordinate impermeable clays—seams which form water-levels of more or less importance, whilst the impermeable clays sometimes contain subordinate beds of sand or of rock which constitute small local water-bearing beds. It is for the geologist to assign its relative value to each of these subordinate features, and to distinguish the minor from the major sources.

Taking the Thames basin above Kingston, there is, according to Mr. J. D. Harrison, an area of 1,233 square miles of impermeable strata, and of 2,442 miles of permeable strata, and the mean annual rainfall in that district amounts to about 27 inches. From the impermeable strata the rain flows off immediately as it falls, and is carried at once to sea; whereas a large portion of that which falls on the permeable strata is, as we have shown, stored for a greater or lesser time, and discharged in perennial springs. It is these which give permanence to our rivers. The evidence taken before the Commission showed that the daily discharge of the Thames at Kingston, even in the driest season after weeks without rain, never falls below 350,000,000 gallons, while the average for the year gives, according to Mr. Simpson and Mr. Harrison, 1,353,000,000 gallons, or, according to Mr. Beardmore's longer observations, 1,145,000,000 gallons daily, the mean of 1,250,000,000 gallons being equal to a fall of about 8 in., or rather less than one-third of the annual quantity, the other two-thirds being lost by evaporation and absorbed by the vegetation. This seems the proportion usual under the like general conditions in these latitudes. M. Belgrand has shown, in "La Seine," that in the upper basin of the Seine there are 19,390 square kilometres of impermeable, and 59,210 of per meable strata; and careful measurements have proved that the discharge at Paris is also equal to about one-third of the rainfall. The exact proportion of the rainfall passing into the different permeable strata, and given out again in the form of springs, has yet to be accurately determined. Mr. Harrison estimates it in the Thames basin at about one-sixth of the rainfall.

In districts where impermeable strata predominate, the total water delivery, therefore, will be greater; but it follows close upon the rainfall; whereas, where the permeable strata predominate, a large portion of the rainfall is stored in the hills, and its delivery is thereby spread over a greater or lesser period of time, according to the dimensions of those hills. This is well exemplified in the case of the basins of the Thames and the Severn, which latter is formed in large part by the slate rocks of Wales. The former has an area above Kingston of 3,670 square miles, with an annual rainfall of 27 inches; whereas that of the latter above Gloucester has an area of 3,890 miles, with an average rainfall of probably not less than 40 inches, and the mean daily discharge for the year is for the Thames of 1,250,000,000 gallons, and for the Severn about 1,600,000,000 gallons. Yet the summer discharge of the Thames averages 688,700,000 gallons daily, against 297,599,040 gallons of the Severn; and while the minimum discharge of the Thames in the driest seasons never falls below 350,000,000 gallons, that of the Severn falls below 100,000,000 gallons. Again, in the case of the Lea, where there is a still larger proportion of permeable strata, the daily discharge at Broxbourne for the year is, according to Mr. Beardmore, 108,000,000 gallons, while for the summer months it remains as high as 71,000,000, and in the driest seasons does not fall below 42,000,000 gallons.

Let us now look at one of the geological questions dependent upon the solvent action of the water on the strata it traverses. The analyses, made for the Commission by Drs. Frankland and Odling, of the waters of the Thames and its tributaries in the Oolitic and Chalk area, show that every 100,000 parts or grains of rainwater has taken up a quantity varying from 25·58 to 32·95 grains of solid residue, or an average of 29·26, which is equal to 20·48 parts or grains per gallon; another analysis of the Thames water at Ditton gives 20·78 grains per gallon of solid residue. It was also shown by Drs. Letheby and Odling and Prof. Abel that the unfiltered waters of the Thames Companies, which take their supplies above Kingston, contained 20·82 of solid residue. If from the average of 20·68 we deduct 1·68 grain for organic and suspended matter, we have 19 grains of inorganic residue for every gallon of water flowing past Kingston. This is of course apart from the sediment carried down in floods. The ordinary monthly analyses, conducted by the same eminent chemists during the course of several past years, show that this quantity is liable to very little variation, the only difference being that it is somewhat larger in winter and less in summer.

Some general estimates have already been made by Profs. Ramsay and Geikie of the quantity of mineral matter carried down in solution by the Thames; but the more exact data supplied to the Commission enable us to make some additions to previous results. Taking the mean daily discharge of the Thames at Kingston at 1,250 million gallons, and the salts in solution at 19 grains per gallon, the mean quantity of dissolved mineral matter there carried down by the Thames every twenty-four hours is equal to 3,364,286 lbs. or 1502 tons, or 548,230 tons annually. Of this daily quantity about two-thirds, or 1,000 tons, consist of carbonate of lime, and 238 tons of sulphate of lime, while limited proportions of carbonate of magnesia, chlorides of sodium and potassium, sulphates of soda and potash, silica and traces of iron, alumina, and phosphates, constitute the rest. If we refer a small portion of the carbonates, and the sulphates and chlorides chiefly, to the impermeable argillaceous formations washed by the rain water, we shall still have at least 10 grains per gallon of carbonate of lime, due to the Cretaceous and Oolitic strata and Marlstone, the superficial area of which, in the Thames basin above Kingston, is estimated by Mr. Harrison at 2,072 square miles. Therefore the annual quantity of carbonate of lime carried away from this area by the Thames is 290,905 tons, or 797 tons daily, which gives 140 tons removed yearly from each square mile; or extending the calculation to a century we have 14,000 tons removed from each mile of surface. Taking a ton of chalk as equal to 15 cubic feet, this is equal to a removal of $\frac{9}{100}$ of an inch from the surface in the course of a century, so that in the course of 13,200 years a quantity equal to a thickness of about one foot would be removed from our Chalk and Oolitic districts.

I had some faint hope that this wear might furnish us with a rough approximate measure of time in reference to some of the phenomena connected with the Quaternary period; but we are not in a position to apply it. Those curious funnel-shaped cavities, called sand and gravel-pipes, so common in many chalk-districts, are the result of slow solution of the chalk by water at particular spots, whereby the superincumbent sand and gravel have been let down into the cavity so produced. Some of them are but a few feet deep, while others attain dimensions of 80 feet in depth by 15 to 20 feet in diameter at top, tapering irregularly to a point at bottom. It is, however, evident from the variation in size that the wear has been unequal; and it is also clear that the surface-waters have been conducted through these particular channels, where they existed, to the underground water-level, in preference to passing through the body of the chalk, so that the ratio of wear at these points is in excess. Nor can I see at present how otherwise to apply this measure. If it were possible to find a spot where the exposed surface of the chalk has been worn uniformly, and, from the quantity of flints left after the removal of the chalk and the known distance apart there of the seams of flint, to determine the number of feet or inches removed, we might have a base to proceed upon, provided all the quantities remained constant. But such is not the case. Also, although the annual rainfall in the Thames now averages 27 inches, and has probably not varied much from this amount during the present period, it was evidently much greater during the Quaternary period; for I have elsewhere shown that, in the South of England and North of France the rivers of those areas with the same catchment-basins were of much greater size than at present; and Mr. W. Cunnington had before pointed out the same fact in the upper part of the basin with respect to some of the rivers of Wiltshire. M. Belgrand has made an attempt to estimate this quantity with reference to the Seine and its tributaries, and he arrives at the conclusion that, during the Quaternary (or, as he considers it, the Glacial) period, the rainfall was so heavy, that the discharge of the river was from 20 to 25 times greater than at present. I do not altogether concur in this view, but I can conceive that our rivers formerly were of five or six times the size they now are. This is an important element to be considered in all questions bearing on the denudation of land-surfaces.

There is yet another point which, although not in our direct field of research, yet depends so essentially upon the geological conditions we have discussed, and is one, in a public point of view, of such paramount importance, that I will, with your permission, say a few words on the subject. In an uninhabited country, the rain passes through the soil and issues as springs, bearing with it a certain proportion of mineral matter, and only traces of such organic matter as existed on the surface. This would be solely of vegetable origin, and the proportion would be in most cases very small. As man appeared, those conditions would be at first but little altered, for animal matters exposed on the surface rapidly decay and pass away in a gaseous form; but with increasing civilisation and fixed residences the necessity of otherwise getting rid of all refuse would soon be felt. I have shown how population followed the range of shallow permeable strata and the course of valleys, so as to obtain readily that indispensable necessity of life, a sufficient water supply. But with the art of well-digging it soon became apparent that, let the well be carried down but half way to the level of ground-springs, it would remain dry, and that then, so far from holding water, any water now poured into it would pass through the porous strata down to the water-level beneath, keeping the shallower well or pit constantly drained. So convenient and ready a means of getting rid of all refuse liquids was not neglected. Whilst on one side of the house a well was sunk to the ground-springs, at a depth, say, of twenty feet, on the other side a dry well was sunk to a depth of ten feet, and this was made the receptacle of house-refuse and sewage. The sand or gravel acting as a filter, the minor solid matter remained in the dry well, while the major liquid portion passed through the permeable stratum and went to feed the underlying springs. What was done in one house was done in the many; and what was done by our rude ancestors centuries back has continued to be the practice of their more cultivated descendants to the present day, with a persistency in the method only to be attributed to the ignorance of the existence of such a state of things among the masses, and to the ignorance of the real conditions and actual results of perpetuating such an evil—an evil common alike to the cottages of the poor and, with few exceptions, to the mansions of the rich.

Instances occur from time to time to point out isolated consequences of this pernicious practice, but I believe no one who has not gone into the geological question can realise its magnitude. It is not confined to one district or to a few towns or villages. It is the rule, and only within the last few years have there been any exceptions. The organised supply of water now furnished by companies in all large towns has, to a great extent, done away with the evil in those situations (though the root of the mischief has too often been left unextracted); but in villages and detached houses, great or small, it remains untouched and unchecked. Not a county, not a district, not a valley, not the smallest tract of permeable strata, is free from this plague-spot. It haunts the land, and is the more dangerous from its unseen, hidden, and too often unsuspected existence. Bright as the water often is, without objectionable taste or smell, it passes without suspicion until corrupted beyond the possibility of concealment by its evil companionship. Damage, slight in extent, or unimportant possibly for short use, but accumulative by constant use, may and does, I believe, pass unnoticed and unregarded for years. Nevertheless the draught, under some conditions, is as certain in its effects, however slow in its operation, as would be a dose of hemlock. Go where we may, we never know when the poisoned chalice may be presented to our lips. The evil is self-generating; for the geological conditions supplying our necessities lend themselves to its maintenance and extension. The knowledge necessary to remedy it is of very slow growth, and the too frequent want of that knowledge, or disregard of the subject, even amongst able architects and builders, is such that, without legislative enactment, I do not see how the evil is to be eradicated for many a long term of years.

This also is only one form of the evil—it is that where the water-bearing strata are thin and the wells do not exceed a depth of thirty feet. It was the one which prevailed in London, and in towns similarly situated, up to a very few years back. It even still lingers on in some private wells, and is moreover fostered among us by the bright-looking and cool water of too many of our public pumps; for not only does the ground still suffer from the effects of the original contamination, but also from much, almost inevitable, obnoxious surface-drainage, much gas escape, much rainfall on old open churchyards, which find their way to the one level of water supplying in common all these shallow wells. The evil still exists also, although to a less extent, in towns where the wells have to be carried to much greater depths; its effects varying according as the depth, and as the volume of the springs is to the sewage-escape; it is, however, only a question of degree.

But even our deeper and apparently inaccessible springs have not escaped contamination. As before mentioned, the underground water will, when tapped by artesian wells, rise to or above the surface, according to the relative height of the surface of the ground at the well, and of the outcrop of the water-bearing bed or beds, so that if the former is higher than the latter, or if by artificial means the line of water-level in a given area becomes lowered, then the surface of the water belonging to those great underground natural reservoirs will be established accordingly at a certain fixed depth beneath the surface. As each well deriving its supply in a stratum of this description represents a column of water communicating with one common reservoir, it follows that any cause permanently lowering the level of one well will tend to lower the level in the other wells in proportion to their number and distance. Further, it has been discovered that a well of this class can absorb a quantity of water equal to that which it can furnish; and as these wells give greater supplies than shallow wells, the absorbing wells of the same class are alike powerful in proportion to the others. The perverse ingenuity of man has here, again, taken advantage of these conditions to get rid of offensive waste waters by diverting them into such deep wells, whence they pass away in hidden underground channels, unseen and unsuspected, and mingle with those deep-seated water-sources feeding the artesian wells dependent upon them for their supply.

In Paris, where there are several alternating beds of permeable and impermeable strata, and the depth to reach them is not very great, this system of absorbing wells connected with factories became, until regulated by the municipality, very common, to the great injury of many of the underground springs. From this and the other causes before alluded to, a great number of shallow wells have there become so contaminated as to necessitate their abandonment. Our own system of surface-drainage is generally too good, and the depth to the lower water-bearing strata too great, to have rendered the use of such wells here equally advantageous; nevertheless, I have reason to believe that they do exist, and that the sources even of our deep well-water supply in the Lower Tertiary Sands and in the Chalk are thus to some extent polluted and injured.

Nor do the great and perennial springs supplying our rivers altogether escape the evils arising from these obnoxious practices. On the high Oolitic ranges and amongst the undulating Chalk hills, the line of water-level is often so deep below the surface, that only in few cases are wells made—the population being generally dependent on rainwater for their water-supply. But this does not prevent the construction of dry wells for the disposal of sewage and refuse. It is true that the population in these hills is sparse—here and there a farm, a few cottages, and scarcely a village. Still as the ground is everywhere absorbent, and there are no streams even in the valleys (I am now speaking of the higher districts), every dwelling contributes its quota; for the rain and all liquid matter absorbed in these strata necessarily pass down to the great underground reservoirs of water feeding the springs thrown out in the deeper river-valleys. In these cases, however, the thickness of strata through which any liquid has to pass before reaching the line of water-level is such as to produce a more or less efficient filtration and complete decomposition; and as the injury caused is in proportion to the relative volumes of the water-sources and to the artificial additions, the great extent and dimensions of these water-bearing strata and the scanty population of such districts reduce it to a minimum.

Owing to these conditions, great as the evil is, experience teaches that it has, in some cases, its vanishing-point. It may be considered at its maximum in some of the wells of Paris; our own London shallow-well pumps follow next in order; in our river-waters away from towns it is but slight; in some of the springs of the Chalk and Lower Greensands it is hardly appreciable, while in the deep well-waters, especially those of Caterham and Grenelle, it sinks to the minimum attained by any potable waters, with the exception of rain-water. It is also a fortunate circumstance that the wonderful powers of oxidation possessed by air and water, and the powers of absorption and decomposition by soils and earths, are such as, even in the surcharged gravel-bed of London, to remove all the more offensive characters, and leave its spring-waters at all events limpid and bright; whilst the quick eddy, the moving ripple, the bright sunshine, the brisk breeze, the living organisms, are ever at work in our rivers, destroying the almost inevitable accompaniments of the presence of man, and restoring the waters to that original state of purity so essential to his health and welfare.

It was on considerations of quantity of supply thus dependent on geological conditions, and of quality as dependent jointly on geological and artificial conditions, that the Commission was mainly so long and assiduously engaged. With regard to the character of waters as dependent on the geological nature of the strata, while the evidence showed that the waters flowing off hard and insoluble rocks were, from their much greater freedom from mineral matter, more economical for many domestic and manufacturing purposes, yet that for drinking purposes, waters such as those derived from our Chalk and Oolitic districts were, on the whole, as good and wholesome as those from any other sources; while as regards quantity and permanence, the conditions presented by a large catchment basin of a varied geological structure presented the most favourable conditions for the large and maintained supply so essential for a great city. And if, from any cause, it should at some future time be thought desirable to have a supply of a yet more assured and undoubted quality than a river supply, the large springs of the chalk and the Lower Greensand, or the great underground reservoirs of the most efficiently filtered water stored in those formations in Surrey and Hertfordshire, might, I believe, be resorted to with advantage, by means of ordinary and artesian wells, as auxiliary sources of supply for domestic and drinking purposes, supposing the engineering difficulties connected with a double water-supply could be overcome—a difficulty which it, however, seems to me would possibly be less one of construction to our engineers than of cost to the public. But in a great health-question there are other considerations than these which are of more primary importance.

(*To be continued.*)

SCIENTIFIC SERIALS

Journal of the Franklin Institute, November 1871.—The editorial notes in this number are as usual very instructive;

amongst them we must notice Young's catalogue of the bright lines observed in the chromosphere of the sun, which have already reached a goodly number. Under Civil and Mechanical Engineering there are several useful and interesting articles, such as "On Woodworking Machinery," "On the Flow of water in rivers and canals," &c.—Prof. Cooke contributes the first of a series of papers "on the chemical theory of the Voltaic Battery." The present communication, however, deals with preliminary matters; it discusses molecules, atoms, and the quantivalence of elements. The paper which follows is "On some improvements in reflecting Telescopes," by J. A. Hill. The author proposes, in the first instance, to reflect the light from a movable plane mirror placed in the axis of the speculum, which receives the reflected rays; the convergent beam from the speculum passes through an aperture in the centre of the plane mirror, and can be received in a suitable eye-piece; no tubes are used, so that by this method it would be as easy to handle a mirror of 1,000 feet focal length as one of the same size of 50 feet focal length. The observer, too, would remain stationary, and need not be hoisted into mid-air.—Prof. Young continues his Spectroscopic Notes; this month's contribution is "on the construction, arrangement, and best proportion of the instrument, with reference to its efficiency." Under this head come the best angle and material for the prisms, the means of testing for flatness of surface and homogeneity of substance, and the number and arrangement of the prisms; there are also two other sections, "on dispersive efficiency and on luminous efficiency." A suggestion of a new form of chemical spectroscope is given, the dispersive part of this consists of two prisms, which are each concave on one side, and are cemented to the convex object-glasses of the collimator and observing telescope. By this it is hoped to save both material and light.

THE *Geological Magazine* for March (No. 93) opens with a new species of *Rostellaria* (*R. Pricei*) from the Grey Chalk of Folkestone, by the editor, Mr. H. Woodward.—Mr. A. H. Green communicates a paper on the method of formation of the Permian beds of South Yorkshire, in which he discusses the general arrangement and palæontology of these beds, and deduces from them a confirmation of Prof. Ramsay's theory that the Magnesian Limestone and associated beds of this part of England were formed in part by chemical precipitation in an inland sea.—Prof. H. A. Nicholson records the occurrence of the Cephalopod *Endoceras proteiforme* Hall, in Britain; the specimen described and figured was discovered by the author in the mudstones of the Coniston series near Ambleside, a set of rocks in which scarcely any fossils, except Graptolites, have hitherto been found.—Mr. James Geikie gives a fourth paper on Changes of Climate during the Glacial Epoch, in the conclusion of which he sums up his views as to the sequence of climates at this time as follows:—1. A succession of alternate glacial and temperate conditions, but associated with the great Continental ice-sheets; 2, a temperate climate, with removal of the ice-sheets from low grounds; 3, a period of subsidence, with temperate climate, and much denudation of moraines; 4, a period of emergence, with arctic conditions, floating ice dispersing erratics, and deposition of clays with arctic mollusca; and, 5, a period of local glaciers in Britain and Ireland, with gradual amelioration of climate. In future papers the author proposes to discuss the cave-deposits and older river-gravels of England. The post-glacial geology and physiography of West Lancashire and the Mersey estuary, form the subject of an interesting paper, by Mr. T. Mellard Reade; and Prof. T. Rupert Jones and Mr. W. K. Parker give us the corrected nomenclature of the Foraminifera from the English Chalk, figured by the Rev. Henry Eley in 1859.—The number also contains an abstract of an address on subsidence as the effect of accumulation, read before the Liverpool Geological Society, by Dr. Charles Ricketts.

THE *Journal of Botany* for March contains only one original article bearing specially on British Botany, Notes on the British *Ramalinæ* (a genus of Lichens) in the Herbarium of the British Museum, by the Rev. Jas. Crombie. We find also, "On *Symea*," a new genus of triandrous *Liliaceæ* from Chili, by Mr. J. G. Baker, with a plate; recent researches into *Diatomaceæ*, by the Rev. E. O'Meara; and *Castanea vulgaris* grown in Southern China, by Dr. Hance. Mr. Carruthers contributes his important Review of the Contributions to Fossil Botany published in Britain in 1871; and the editor commences in this number a valuable list of the articles contained in the German botanical journals for January.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, Feb. 29.—"On the Relative Power of Various Substances in arresting Putrefaction and the Development of Protoplasmic and Fungus Life;" by Dr. F. Crace-Calvert, F.R.S.

March 14.—"Contributions to the History of the Opium Alkaloids," part iv.; by Dr. C. R. A. Wright.—"The Decomposition of Water by Zinc in conjunction with a more Negative Metal;" by J. H. Gladstone, F.R.S., and Alfred Tribe, F.C.S.

March 21.—"On some Heterogenetic Modes of Origin of Flagellated Monads, Fungus-germs, and Ciliated Infusoria," by Professor H. Charlton Bastian, F.R.S. In this communication Dr. Bastian announces results which, whilst confirming the previous observations of MM. Pineau and Pouchet, considerably extend our knowledge concerning the heterogenetic changes liable to take place in the pellicle (composed of aggregated Bacteria) which forms upon an infusion of hay. He describes all the stages by which certain Fungi, Flagellated Monads, and Ciliated Infusoria are produced, as a result of changes taking place in the very substance of the pellicle. Most of the observations were made under a magnifying power of 1,670 diameters, and, although more extensive, are confirmatory of others published in NATURE, No. 35. Dr. Bastian says, "I now wish to describe other allied processes, and the means by which I am enabled to obtain, almost at will, either animal or vegetal forms from certain embryonal areas which are produced in the pellicle." The simplest mode of origin of Fungus-germs and Monads is thus described:—"The pellicle which formed on a filtered maceration of hay during frosty weather (when the temperature of the room in which the infusion was kept was rarely above 55° F., and sometimes rather lower than this) presented changes of a most instructive character. On the third and fourth days the pellicle was still thin, although on microscopical examination all portions of it were found to be thickly dotted with embryonal areas. Nearly all of them were very small; but a few areas of medium size were intermixed. The smallest were not more than $\frac{1}{4000}''$ of an inch in diameter, and these separated themselves from the pellicle as single corpuscles; slightly larger areas broke up into two or three corpuscles; and others, larger still, into 4—10 corpuscles. In most of these small areas, the corpuscles were formed with scarcely any appreciable alteration in the refractive index of the matter of which they were composed; this simply became individualised, so that the corpuscles separated from the surrounding pellicle and from their fellows, still presenting all the appearance of being portions of the pellicle, and exhibiting from 4 to 10 altered *Bacteria* in their interior. In some cases the products of segmentation soon developed into actual flagellated Monads in a manner presently to be described; whilst in others they seemed to remain for a longer period in the condition of simple motionless corpuscles. Other solitary corpuscles or small areas began to form in the pellicle in precisely the same manner, though they speedily assumed a highly refractive and homogenous appearance. Why some should undergo such a change, and not others, seems quite impossible to say. One can only assert the fact, and add that these highly refractive ovoid corpuscles were, for the most part, more prone to produce Fungus-germs than Monads. Many of them soon grew out into dissepimented fungus filaments, which rapidly assumed the *Penicillium* mode of growth. The spores, which were abundantly produced in terminal chaplet-like series, were, however, small, homogeneous, spherical, and colourless." In other cases Monads and Fungus-germs are produced from the pellicle in precisely the same manner as that by which they arise within the terminal chambers of certain Algæ or Fungi—that is to say, they result from the segmentation of a mass of homogeneous protoplasm.

In speaking of such a mode of origin of Monads, Dr. Bastian says:—"Contrasting with the very pale fawn-colour of the evenly granular pellicle, there were numerous areas of a whitish colour, refractive, and more or less homogeneous. These areas differed very much in shape and size; some were not more than $\frac{1}{1000}''$, whilst others were as much as $\frac{1}{150}''$ in diameter. Their shape was wholly irregular. As in the instances previously recorded, the first appreciable stage in the formation of an embryonal area in the pellicle was a local increase in the amount of gelatinous material between the units of this portion of the pellicle, so that they became more distinctly separated from one another than in adjacent parts. Gradually these particles became less sharply defined, and at last scarcely visible, in the midst of a highly refractive protoplasmic mass which began to exhibit traces of segmentation. Masses of this kind were seen, which had been resolved by such a process of segmentation into a number of spherical corpuscles about $\frac{1}{4500}''$ in diameter. These were at first highly refractive, though they gradually became rather less so, and revealed the presence of two or three minute granules in their interior. In other adjacent areas, a number of densely-packed, pliant, and slightly larger corpuscles were seen actively pushing against one another. When they separated, they were found to be active ovoid specimens of *Monas lens*, about $\frac{1}{3300}''$ in length, and provided with a vacuole and a rapidly lashing flagellum."

In other cases embryonal areas of the same nature were formed, which went through similar processes of segmentation; although the units produced, instead of developing into Monads, were seen to become transformed into brown vesicular bodies, which subsequently germinated into Fungus filaments. Whilst affirming that he is now able to determine pretty surely the occurrence of either one of these phenomena, Dr. Bastian says:—

"Experience has shown me, that, if an infusion has been heated for a time to 212° F., the pellicle which forms on its surface very frequently never gives rise to an embryonal area. If the infusion has been prepared at a temperature of 149° — 158° F., the embryonal areas which form will give origin to Fungus germs; whilst in a similar infusion prepared at 120° — 130° F., the embryonal areas, which seem at first to be in all respects similar, break up into actively moving Monads."

Dr. Bastian then proceeds to give an account of the origin of *Paramecia*, laying stress upon the fact that, in order to obtain such organisms, it is necessary to employ a filtered infusion made with cold water. His observations on this subject were, in the main, confirmatory of those of M. Pouchet. Thousands of egg-like bodies, varying in size from $\frac{1}{300}''$ to $\frac{1}{350}''$ were seen developing throughout the whole substance of a thick pellicle. He says: "It seemed to me that the differentiation took place after a manner essentially similar to that by which an ordinary 'embryonal area' is formed. The small embryos did not appear to represent the earlier stages of large embryos; and it seemed rather that spherical masses of the pellicle of different sizes began to undergo molecular changes, which terminated in the production of *Paramecia* of a correspondingly different bulk. Just as in the previously described embryonal areas masses of different sizes began to exhibit signs of change, so also here, spherical portions of the pellicle, differing within the limits above mentioned, began to undergo other heterogenetic changes. This was first indicated by an increased refractiveness of the area (especially when seen a little beyond the focal distance); and almost simultaneously a condensation of its outer layer seemed to take place, whereby the outline became sharply and evenly defined. At this stage an actual membrane is scarcely appreciable, and the substance of the embryo (when examined at the right focal distance) scarcely differs in appearance from the granular pellicle of which it had previously formed part. So far as it could be ascertained, the individual embryos did not increase in size, although they went through the following series of developmental changes. The contained matter became rather more refractive, and the number of granules within diminished considerably, whilst new particles after a time seemed gradually to appear in what was now a mass of contractile protoplasm. These new particles were at first sparingly scattered, though as they were evolved they continued to grow into biscuit-shaped bodies, which sometimes attained the size of $\frac{1}{10000}''$. All sizes were distinguishable; and many of them moved slowly amongst one another, owing to the irregular contractions of the semi-fluid protoplasm in which they were embedded. Gradually the number of homogeneous biscuit-shaped particles increased; and at last a large vacuole slowly appeared in some portion of the embryo. It lasted for about half a minute, disappeared, and then, after a similar interval, slowly reappeared. Much irregularity, however, was observed in this respect. The next change that occurred was the complete separation of the embryo from the cyst which it filled, and the commencement of slow axial rotations. These rotations gradually became more rapid, though they were not always in one direction. The mass became more and more densely filled with the large biscuit-shaped particles, and at last the presence of cilia could be distinctly recognised on one portion of the revolving embryo. Then, as M. Pouchet stated, the movements grew more and more irregular and impulsive, so as at last to lead to the rupture of the thin wall of the cyst—when the embryo emerged as a ciliated and somewhat pear-shaped sac, provided with a large contractile vesicle at its posterior extremity. . . . On emerging from the cyst, all the embryos, although differing somewhat in size, were of the same shape. This closely corresponded with the description given of *Paramecium colpoda* in Pritchard's 'Infusoria,' namely:—'Obovate, slightly compressed; ends obtuse, the anterior attenuated and slightly bent like a hook.' Cilia existed over the whole body, though they were largest and most numerous about the anterior extremity. No trace of an actual buccal cleft could be detected; and (except in the posterior portion of the body, where a large and very persistent vacuole was situated) the organism was everywhere densely packed with the large, homogeneous, biscuit-shaped particles. For many days these most active Infusoria seemed to undergo little change, though afterwards the number of the contained particles gradually began to diminish, whilst the body became more and more regularly ovoid, and a faint appearance of longitudinal striation manifested itself, more especially over its anterior half. At the same time a very faint and almost imperceptible mass ('nucleus') began to appear near the centre of the organism; and when examined with a magnifying power of 1,670 diameters, a lateral aperture (mouth) $\frac{1}{8000}''$ in diameter was seen, which was fringed by short active cilia, arranged like the spokes of a wheel. These peculiarities correspond very closely with those of an embryo *Nassula*. Very many were seen with similar characters; and multitudes existed in all conditions intermediate between this stage and that of the simpler organism which first emerged from the cyst."

Dr. Bastian concludes by saying:—

"It will, of course, be seen that the phenomena which I have described as taking place in the 'proligerous pellicle' may be watched by all who are conversant with such methods of investigation. We do not require to call in the aid of the chemist; we need exercise no special precautions; the changes in the pellicle are of such a kind that they can be readily appreciated by any skilled microscopist.

"Just as I have supposed that living matter itself comes into being by virtue of combinations and re-arrangements taking place amongst invisible colloidal molecules, so now does the study of the changes in the 'pellicle' absolutely demonstrate the fact that the visible new-born units of living matter behave in the manner which has been attributed to the invisible colloidal molecules. The living units combine, they undergo molecular re-arrangements; and the result of such a process of heterogenetic biocrasis is the appearance of larger and more complex organisms; just as the result of the combination and re-arrangement between the colloidal molecules was the appearance of primordial aggregates of living matter. Living matter is formed, therefore, after a process which is essentially similar to the mode by which higher organisms are derived from lower organisms in the pellicle on an organic infusion. All the steps in the latter process can be watched; it is one of synthesis—a merging of lower individualities into a higher individuality. And although such a process has been previously almost ignored in the world of living matter, it is no less real than when it takes place amongst the simpler elements of not-living matter. In both cases the phenomena are essentially dependent upon the 'properties' or 'inherent tendencies' of the matter which displays them."

Mathematical Society, March 14.—W. Spottiswoode, F.R.S., president, in the chair.—The President made a statement to the effect that it had been desirable to apply for a Charter, and that he had taken the requisite steps for ascertaining the right mode of procedure. The proposal made by the President being unanimously agreed to, the matter dropped.—A vote of thanks was passed to Mr. S. M. Drach for his present to the Society of two early and interesting works by Vieta and Ubaldi respectively.—The papers read were:—Prof. Clifford, "On a new expression of Invariants and Covariants by means of alternate numbers;" Hon. J. W. Strutt, "On the Vibrations of a gas contained within a rigid spherical cone." The former paper was concerned with methods given in "Vorlesungen über die complexen Zahlen und ihre Functionen," by Dr. Hermann Hankel (1867). In the latter paper the problem discussed was one referred to in a paper on the "Theory of Resonance," Phil. Trans., 1871. It is the only case of the vibration of air within a closed vessel which has hitherto been solved with complete generality. A result arrived at was that the pitch is about a fourth higher for the sphere than it is for a closed cylindrical pipe, whose length is equal the diameter of the sphere.—

Mr. A. J. Ellis, F.R.S., communicated a question which had been forwarded to him by Prof. Haldeman, of Columbia, Pennsylvania, U.S., "The number of lines in a rhymed stanza being given, how many variations of rhyme-distribution does it admit of, suppose no line to be left without a rhyme?"

Victoria Institute, March 18.—Mr. Charles Brooke, F.R.S., in the chair.—Dr. Bateman on "Darwinism tested by recent Researches as to the Localisation of the Faculty of Speech." Having called attention to Mr. Darwin's statement, that the difference between man and the higher animals was only one of degree, and not of kind, he proceeded to show that such could not be the fact, and instanced the faculty of articulate language, a distinctive attribute of which there was no trace in the ape or other animals. After defining articulate language, he demonstrated that it was exclusively man's prerogative, and there was no analogy between it and the forms of expression common to the lower animals. He then stated that it had been thought that a particular part of the brain was the seat of language, and, if it were so, the Darwinian might contend that, as there was a certain similarity between the brain of man and of the ape and other animals, that they had the germs of the faculty. He then cited many cases which had been brought under the notice of German, French, American, English, and other surgeons, to show that even where various portions of the brain had been injured or destroyed, the faculty of speech remained He concluded by stating that the faculty of articulate speech seemed to be an attribute, the comprehension of which was at present beyond us.

GLASGOW

Geological Society, February 8.—Sir William Thomson, LL.D., was elected president; Messrs. E. A. Wünsch, John Young, and James Thomson, F.G.S., vice-presidents.—Professor Young, the retiring president, delivered an address on "Rock Formations in relation to Geological Time." He concluded by expressing the pleasure he felt in resigning the chair to one so eminent in the walks of science as Sir William Thomson, whose contributions to theoretical geology had been of the utmost importance.—The President, in taking the chair, briefly thanked the members for the honour they had conferred upon him, and hoped he might be of some service to them in the prosecution of geological inquiry.

DUBLIN

Natural History Society, March 6.—Professor E. Perceval Wright, president, in the chair.—The President delivered his inaugural address. He gave an interesting account of the history of the society from its commencement in 1838, when their meetings were held in Suffolk Street, and the opening address delivered by Mr. O'B. Bellingham. "There were then 104 members, and in 1840 the number had increased to 150. In 1844 the museum so increased that Mr. M'Coy was appointed curator, and he in 1845 laid a catalogue of the Irish animals in the museum before the society. This catalogue was printed and appended to the report for 1845-46. During these years many records of species new to Ireland were made. Very many valuable and interesting papers on zoological subjects were read. Many of these are to be found in full in the *Annals and Magazine of Natural History*. It is strange in looking over some of these to be reminded how great has been the development of some branches of natural science since they were written. Friends of many of us here—friends still living—many of them by no means yet full of days, yet wrote before the developmental stages of the crustacea were known, and could write of Spongilla as undoubtedly allied to the Diatomaceæ. About 1851 a few students in college, including myself, determined to form the University Natural Science Association, which is now amalgamated with the present society. Ere ceasing to speak of the College Society, let me pay a passing tribute to the memory of those who were our strong support, and who freely and generously held out to us that helping hand, and who have now left us for ever—Robert Ball, W. H. Harvey, A. H. Haliday, and A. Furlong; nor would it be seemly to forget all the encouragement and assistance given to us by the authorities of the College and the Regius Professor of Physic, or the loss we sustained when Allman, our Professor, counsellor, and friend was, by a hard fate, moved to succeed Forbes in Edinburgh."

PAMPHLETS RECEIVED.

ENGLISH.—The Dolmen Mounds and Amorpholithic Monuments of Brittany: S. P. Oliver, R.N. Remarks on the successive Mining Schools of Cornwall: J. H. Collins.—The Unity of Man's Being: A. Diesterweg.—Modern Examples of Road and Railway Bridges, Part I.: Maw and Dredge—Transactions of the Institution of Engineers and Shipbuilders in Scotland—Quarterly Weather Report of the Meteorological Office, July-Sept, 1870.—Annual Report of the Geologists' Association, 1871.—Modern Science and the Bible; their Positive and Direct Antagonism.—The Study of Economic Botany: Jas. Collins.—Lord Derby on the United Kingdom Alliance.—Statistics of the Liquor Traffic: Rev. D. Burns.—19th Report of the Executive Committee of the United Kingdom Alliance.—The Deviation of the Compass in Iron Ships: W. H. Rosser.—Proceedings of the Geologists' Association.—Report of the Committee on Ships of War.—Report of the Case of H.M.S. *Megæra*.—Journal of the Iron and Steel Institute, February.—Catalogue of Microscopical Preparations of the Quekett Microscopical Club.—On the Mechanical Impossibility of the Descent of Glaciers by their Weight only: Canon Moseley.—French Farmers' Seed Fund Reports.—Eastbourne Natural History Society Report.—Journal of the Royal Dublin Society, No. 40.—Quarterly Journal of the Meteorological Society.

AMERICAN & COLONIAL.—Hinrichs' School Laboratory of Physical Science, Nos. 3 and 4.—Experimental Steam Boiler Explosions: Prof Thurston.—Observations on Encke's Comet: Prof. C. A Young.—The Phœnix, for January, 1872.—Smithsonian Contributions to Knowledge; Converging series expressing the ratio between the diamater and circumference of a circle: W. Ferrel.—7th Annual Catalogue of the Massachusetts Institute of Technology.—The Lens, No. 1.—Proceedings of the American Philosophical Society, July-Dec, 1871.—Lecture on Water: C. F. Chandler.—Inaugural Lecture of the Department of Practical Science in M'Gill University: G. F. Armstrong.—Lectures delivered at the Industrial and Technical Museum at Melbourne during the Autumn Session of 1871.

FOREIGN.—Bericht der Kaiserliche Akademie der Wissenschaften zu Wien.—Bulletin de l'Académie Impériale des Sciences de St Petersbourg.—Karte der Alpen in 8 ko'orirten Blättern: Mayr u Berghaus.—Die Centralen Ortler-Alpen; nebst einem Anhange zu der Adamello-Presanella-Alpen: J. Payer.

DIARY

THURSDAY, APRIL 4.

LINNEAN SOCIETY, at 8.—On the Geographical Distribution of Compositæ: G. Bentham, President (concluded).

CHEMICAL SOCIETY, at 8.

FRIDAY, APRIL 5.

GEOLOGISTS' ASSOCIATION, at 8.—On the Excavations on the Site of the Law Courts: Wilfrid H. Hudleston, and F. G. H. Price.—On Columnar Basalts: John Curry.

ARCHÆOLOGICAL INSTITUTE, at 4.

MONDAY, APRIL 8.

ROYAL UNITED SERVICE INSTITUTION, at 8.30.—H.M.S. *Agincourt* on, and off, the Pearl Rock: Commander R. H. Boyle, R.N.

ANTHROPOLOGICAL INSTITUTE, at 8. Notes on the Hair of Oceanic Races: Dr. B. Davis.—Note on the Hair of a Hindostanee: Dr. H. Blanc.—On the Descent of the Esquimaux: Dr. Rink.

TUESDAY, APRIL 9.

ROYAL INSTITUTION, at 3.—Statistics and Social Science: Dr. Guy.

PHOTOGRAPHIC SOCIETY, at 8.—M. Merget's Mercury Process.

WEDNESDAY, APRIL 10

GEOLOGICAL SOCIETY, at 8.—Notice of some of the Secondary Effects of the Earthquake of 10th January, 1869, in Cachar: Dr. Oldham, Calcutta, and Robert Mallet, F.R.S.—Notes on Atolls or Lagoon Islands: S. J. Whitnell. On the Glacial Phenomena of the Yorkshire Uplands: J. R. Dakyn.—Modern Glacial Action in Canada: Rev. W. Bleasdell, M.A.

SOCIETY OF ARTS, at 8.

THURSDAY, APRIL 11.

ROYAL SOCIETY, at 8 30.

SOCIETY OF ANTIQUARIES, at 8.30.

ROYAL INSTITUTION, at 3.—Heat and Light: Dr. Tyndall.

MATHEMATICAL SOCIETY, at 8.—On the Mechanical Description of certain Sextic Curves: Prof. Cayley, V.P., F.R.S.

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NOTICE

We beg leave to state that we decline to return rejected communications, and to this rule we can make no exception. Communications respecting Subscriptions or Advertisements must be addressed to the Publishers, NOT *to the Editor.*

THURSDAY, APRIL 11, 1872

NEWSPAPER SCIENCE

WHETHER some knowledge of Science or some love for scientific truth will ever penetrate the masses, may well be questioned when we read such an article as the following, which appeared in the daily paper boasting the largest circulation in the world, and which we reprint almost entire as a curiosity of newspaper literature :—

"What is a Joule ?—or who is he, if a Joule is a human being, and not a vegetable—a weapon of offence, or something to drink, or a Phantom ? And if Joule be human, why did he not consider that human reason is fallible, and human patience exhaustible, when he penned, or got somebody else to pen, a maddening article which has appeared in the *Nautical Magazine*, from which we gather that the transformations of energy are in their nature similar to the operations of commerce ; but with this difference, that in thermodynamics the relative values never vary. This, it seems, is the universal theorem of a Joule ; and a red-hot poker must always bear the same relation to sixpence as the contents of a tea-kettle at boiling point bear to a five-pound note. . . . Under the new dispensation the sovereign, 'to which all other forms of energy can be referred,' is to be an unit of heat. On the obverse is stamped 'Joule's equivalent,' and on the other side is inscribed 772 foot-pounds. One unit of heat is the amount required to raise the temperature of one pound of water one degree, and the equivalent for this coin is 772 foot-pounds of work—that is, the work required to be expended to raise one pound weight 772 feet. . . . But what is the new 'Joule's equivalent' to be made of ?—cobwebs, leather, or fresh butter ?—and who wants to raise a pound weight 772 feet? As a problem of proportion, the theory is, of course, philosophical enough ; but it would be just as easy to fix a unit of cold as well as a unit of heat ; and, under any circumstances, until Joule comes into the open and tells us who he is, what he means, and when his equivalents are to be put into circulation, society, we fear, will decline to recognise a sovereign as a Joule, or thirty shillings as a Joule and a half."

Now, with the mental condition of the man who could pen such an article as this we have nothing to do ; he may go on writing according to his lights every day of the week, and no one but his own friends need interfere to stop him. But there are one or two considerations which arise from the perusal of it not without their importance.

In the first place, bearing in mind the contempt for Science so often apparent in the public utterances of men of high calibre—instances occur to us as we write, and probably will to our readers, of men of the highest culture in literature or art, who never allude to scientific work or to scientific teachers without a scarcely disguised sneer at the inferior part which they play in the national economy—we may, after all, be content that Science is alluded to at all in a paper possessing so large a circulation. The next consideration is one to which we attach the highest importance.

Surely it is now time that scientific men themselves should take a little more trouble than they do—we know it is asking a good deal from them—in the matter of bringing their own work, and the importance of it to the community, before such audiences as the daily papers afford. Were they to do this, the labours of our great scientific teachers our Huxleys, Tyndalls, and Carpenters—would be enormously lightened. If we hear of an attendance of several thousands at a penny lecture by Huxley at Manchester, or a Sunday afternoon lecture in St. George's Hall by Carpenter, we fancy a love of science is spreading with rapid strides ; but the fact is that the strides are not so rapid as they might be, because the labourers on whom progress depends are so few and the area of their lecture work is restricted, whereas many newspapers, on the other hand, number their readers by hundreds of thousands. Until scientific men do this, we must be content with the present state of things. It is in no spirit of invidious comparison that we may remind our readers of the frequent extracts which appear in our columns from *Harper's Weekly*, a political and general paper of very large circulation in the United States, the scientific department of which, containing information of the highest value, is edited by one of the most eminent scientific men of America. But what is the present state of things with us ? In the main it is one in which the public is informed of scientific work by others than the doers of the work ; and the labour of classifying these writers is not difficult.

In the first place we have, we are thankful to say, a small though gradually increasing number whose labours leave nothing to be desired, who, being men of scientific culture themselves, take a pleasure in their work, and to whom the friends of Science in this country cannot be too grateful. As an illustration of the labours of this class of writers, designed to present to the non-scientific public an account of remarkable scientific phenomena, in popular and yet accurate language, we may refer to one of the most recent publications of this class, an article entitled "A Voyage to the Sun" in the March number of the *Cornhill Magazine*, which we commend to the notice of all aspirants after scientifico-literary fame. The play of fancy which invests with an attractive grace a subject that would appear dry to many, is combined with a happy art of describing scientific phenomena in clear and exact language, in a manner that we have seldom seen equalled. It is impossible to overrate the labours of these gentlemen in the present condition of Science in England.

Secondly, we have a still larger class where the intention is good, but in which the culture, scientific and otherwise, is not so high. In the writings of these Science is apt to run wild : accuracy gives place to imagery, and the would-be learners, after an hour's attempt at gaining knowledge, rise from it, knowing rather less than they did before, and looking upon Science as a fearful and wonderful thing with which the less they have to do the better.

We have next a third class, composed of writers as widely different as the poles, but we place them together because the harm they both do is incalculable. The writer who is anxious to know what a "Joule" is may be taken as the type of one division. Grossly ignorant of all kinds of Science, it is nothing to him that he should bring it into discredit ; he is doubtless paid for his work, and we need say no more about him. In the second division we find sometimes high culture, but the writing is not written for Science' sake. It is entirely a personal affair. The advancement of Science gives way to that of the individual and his friends, and any subject written upon is seen through a fog of personality and advertisement.

On the whole we prefer the author of "What is a Joule?" to such a man as this, because we believe he does less harm, and is less likely to mislead "able editors."

There is one grain of comfort even in the imbecilities and inanities of would-be humorous writers in newspapers, that at least they have woke up to the idea that a scientific discovery is worth laughing at. This is a step gained. Twenty years ago, even ten years ago, the name of even so distinguished a scientist as Dr. Joule would have been utterly unknown to the herd of newspaper writers. We must be thankful for even this much; and look hopefully forward to the good day coming when Science will take her place by the side of her sisters, Art and Literature, as equally deserving of popular culture.

GRISEBACH'S VEGETATION OF THE GLOBE

Die Vegetation der Erde nach ihrer klimatischen Anordnung: ein Abriss der vergleichenden Geographie der Pflanzen; von A. Grisebach. 2 vol. (Leipzig: Engelmann, 1872.)

THIS important contribution to a branch of the science which, since the publication of A. de Candolle's "Géographie Botanique" and the promulgation of the Darwinian theories, has been daily acquiring greater value in the minds of philosophical naturalists, is the result of long study and persevering accumulation of data on the part of the learned author. Prof. Grisebach had already, in the "Linnæa" for 1838, given his first views on the limitation of natural floras by climatological influences; and since 1840 he has, in his periodical reports on the progress of geographical botany, entered more or less into the principles and conclusions which he has successively entertained or matured. He now supplies us in these volumes with a methodical digest of the facts he has collected, and of the conclusions he would draw from them. The result is a rich store of materials, which future investigators of the subject must necessarily have recourse to, and the arrangement adopted is perhaps the one best calculated to illustrate that branch of it which is more especially indicated by the title, the influence of climate and physical conditions on the stations and areas of species. But to the general naturalist the value of the work as a book of reference is much diminished by two great deficiencies; there is no summary of the conclusions he would draw from the facts he has detailed, and no index to enable the reader to turn to any individual fact, argument, or deduction, which may have struck him in the perusal of above 1,200 closely printed pages.

The question of the Origin of Species is not entered into, for the author believes that acknowledged facts prove nothing more than the production of varieties through climatological or other influences, but that "however interesting speculations on the genetic connections of organisms may appear, we abandon the territory of facts when we indulge in conjectures on the origin of more widely separated forms or races, of species, genera, or families of plants or animals." "That the limits between a species and a variety are not always to be strictly defined, is no reason," he observes, "why we should ascribe to both an identical process of formation, or that we should regard the forces by which the gradual variations of forms are effected as the only ones by which the multiplicity of nature has been produced."

As far as we have been able to collect the professor's views, his idea seems to be that, whatever may have been its origin, every species now existing on the globe was at some given (or uncertain) time "produced" in one particular spot, the centre of the species, from whence it has, from the natural tendency to multiplication inherent in every organised race, spread in every direction where its progress has not been checked by extraneous causes, generally by climatological or other physical opposing influences, sometimes by the mere struggle with competing races. Wherever a considerable number of species appear to have had their centres within a limited area, that area is termed a centre of vegetation (*Vegetations-centrum*); where the migration of plants from one or more centres is limited by physical obstructions, by mountain chains, seas, adverse climate, &c., the space thus enclosed is the province (*Gebiet*) of a natural flora. For the "centres of vegetation," the author had originally made use of the term "centres of creation" (*Schöpfungs-centren*), which he has now abandoned on account of the objections made to it as expressing some definite process of production. "I, at least," he adds, "under an act of creation, never understood anything else than the operation of certain laws of nature, the further knowledge of which is, as yet, withheld from us. Bentham prefers for the term 'centres of vegetation' that of 'areas of preservation,' when they remain in their original state, as in oceanic islands, a mode of expression to which we might well be reconciled" (p. 523). With regard to the term *Gebiet*, the natural translation would be *region*, but in this instance, with the facility enjoyed by Germans of adopting words of foreign languages, the word "Region" is made use of for areas limited by altitude within the *Gebiet*.

The twenty-four botanical provinces of natural floras which Grisebach had already sketched out in Petermann's *Mittheilungen* are here necessarily taken in detail, investigating under each one—(1) the climate; (2) the prevailing plant-forms; (3) the prevailing plant-formations; (4) the regions, chiefly as to altitude; and (5) the centres of vegetation included in the province. For the "plant-forms" he has carried out a classification founded on that of Humboldt, distributing plants under seven heads—(1) woody plants; (2) succulent plants; (3) climbers; (4) epiphytes; (5) herbs; (6) grasses—including sedges, reeds, &c.; (7) cellular plants: each one subdivided into minor groups. The "plant-formations" are tracts of country whose general aspect is characterised by their vegetation, such as forests, heaths, scrubs, deserts, cultivated tracts, &c.

The two provinces worked out with the greatest care, and for which the materials here collected are perhaps the most deserving of study, as being the most ample, and in both cases checked by the personal experience of the author, are the Forest-province (Waldgebiet) of the eastern continent (the greater part of Europe and temperate Asia), and the Mediterranean region; the one characterised by its vast uniformity, the other by its broken diversity; in both of which the complicated influences of climate, configuration, and soil, have been more carefully observed, recorded, and studied, than in any other quarter of the globe. The Mediterranean region is particularly instructive, not only from the richness of the flora, but from the large number of endemic monotypes (monotypic genera or sub-genera, or widely distinct species), confined to very restricted areas, and of disjointed species—identical species in widely dissevered areas. With regard to the species of narrowly confined stations, Prof. Grisebach believes that the considerations he has brought forward tell decidedly in favour of the conclusion "that monotypes and other rare organisms are not—or, at least, are not generally—to be regarded as the surviving remains of earlier creations, but as evidences of the productive power of the localities where they are now to be found, and from whence no means of migration are within their reach" (p. 364). He can, however, scarcely have paid attention to the various proofs recorded of the gradual reduction of the areas of several Mediterranean species, even within historical times, and still more since an immediately preceding geological epoch—that of the formation of the tufas of the south of France. He does not, indeed, seem to be aware of the instructive memoirs on this subject of Gustave Planchon (see Nat. Hist. Review, 1865, p. 202).

The "disjointed" species, on the other hand, appear to have puzzled Prof. Grisebach, as they have done and will continue to puzzle all speculators on Geographical Botany. Grisebach endeavours to reduce their number as much as possible; sometimes by the discovery of intermediate stations; then, again, by presumed colonisation through man or other agencies; or by showing that supposed identical forms in distant areas are really distinct species, and, therefore, beyond the scope of inquiries limited to the age of now-existing species. But yet, in the Mediterranean as in the Japanese provinces, he is obliged to admit some which occupy two limited areas separated by enormous intervals. Thus, although he supposes that the appearance of *Rhododendron ponticum* on the coast of Portugal may have been the result of introduction by the Arabs, that *Geum heterocarpum*, now only known from the mountain regions of S. Spain and of Elborus in Persia, may yet be found in intermediate localities; yet such suppositions, he admits, can in no way account for the disseverence of the Cedar in the Atlas, the Lebanon, and the Himalaya, or of the *Pinus excelsa* in the mountains of Macedonia and the Himalayas. Unwilling to admit that these and other instances (far more numerous than acknowledged by Grisebach) of widely dissevered stations may be the remains of once continuous areas, he suggests the possibility of the transference of seeds by winds, birds, &c. Birds are, indeed, probably powerful assistants in the migrations of plants. But the effect of winds has been much overrated, as shown for instance by Kerner in a paper recently published in the *Zeitschrift des Deutschen Alpenvereins*, and is made more of perhaps by Grisebach in the present work than by any other observer, and not always on the safest data. Thus he attaches (p. 389) great importance to an "unpublished memorandum of Berthelot's," that is to a label to a specimen of *Erigeron ambiguus*, bearing the words "cette composée, qui a quelques rapports avec les Conyza, est devenue très-commune sur toutes les côtes de Ténériffe après le dernier ouragan." This memorandum is amplified into "On the Canary Islands whose flora was so well known to him, this traveller saw, immediately after a violent hurricane, an annual Synantherea (*Erigeron ambiguus*) which is generally dispersed over the Mediterranean flora, suddenly germinate and take permanent possession of the soil in the most diversified stations," the amplification thus including some half-a-dozen statements not contained in the original memorandum, adding especially the *propter hoc* to the *post hoc*. *Erigeron ambiguus* is one of those plants of which a single individual will produce seed enough to cover a considerable tract of country in the next following season, if favoured by a suspension of those counteracting influences which annually destroy all but one out of thousands, either in the state of seed or of the infant plant; and in Berthelot's memorandum we find no evidence either that the plant was not in the islands before the storm, or that the seed was actually brought by the storm, or that if so brought its germination and early growth were so exceptionally rapid, as to show the plant in an observable stage "immediately" after the storm. The inquiry, however, into the causes of the disseverance of areas, whether due to the gradual extinction of old races, or to the colonisation of new ones, remains one of the most interesting problems for solution in Geographical Botany.

OUR BOOK SHELF

Consumption, and the Breath Rebreathed. By Henry MacCormac, M.D. (London: Longmans, 1872.)

THIS work is written *con amore* by an enthusiastic physician, who has satisfied himself of the truth of the theory he advances, and is now desirous of convincing the rest of the world. The theory broached by Dr. MacCormac is that phthisis or pulmonary consumption, as well as tubercle generally, is always and exclusively the result of the breathing of air that has already been vitiated by respiration. It is well known that air that has once passed through the lungs has undergone important changes. Its oxygen is reduced in quantity, a nearly corresponding amount of carbonic acid has been added, and it also contains certain organic compounds the nature of which has not been very satisfactorily determined, but which are undoubtedly of an effete nature, and analogous in their composition to the disintegrated organic compounds eliminated from the body by the other excretory organs. The extremely deleterious action of the re-introduction into the system of the materials discharged by the intestines is now very generally known, from the inquiries that have been instituted into the nature and origin of typhoid fever; and Dr. MacCormac is perfectly justified from analogy in attributing serious results to the re-introduction into the system by the lungs of the air which has once passed through it, and which is consequently charged with decomposing substances. The carbonic acid alone is bad enough, but even if this were removed as fast as formed and replaced by oxygen, while the animal still continues to breathe the air it has already expired, there can be little doubt that it would speedily feel the effects of the other impurities with which expired air is charged. Under ordinary circumstances the only means of avoiding these effects is to permit free access of air to all and every apartment in which man is confined either by day or night; and so far we cordially endorse the views and recommendations of the author of the work before us. But when Dr. MacCormac states that tubercle is exclusively the result of breathing expired air, we think he carries his theory too far. We cannot put aside in the facile manner he adopts the influence of hereditary predisposition, nor the effects of exposure to damp and cold, when combined with insufficient food. Imperfect ventilation is so common that it is almost always associated with the other probable causes of tubercle, and it is difficult to give instances where tubercular consumption has made its appearance whilst perfectly pure air is continually breathed. But, we think, various considerations render Dr. MacCormac's views untenable. We will not refer to Iceland or to the inhabitants of the elevated plains of the Andes, or of the Steppes of Asia—all of which are sad stumbling-blocks in his way—because, as he says, they are so far off, and our facts in regard to the frequency of tubercle in these regions are perhaps not quite satisfactorily ascertained. But we may call attention to the circumstance that the disease is more common in England than in almost any other country—than in France, for example; yet, surely, the hygienic relations in regard to ventilation are superior in England to those existing on the other side of the Channel.

If air that has been breathed is so certainly the cause of tubercle, the poor population of London and other large towns should not only be decimated, but should be swept off *en masse*, for they all breathe through the night, and through a great part of the day, air so contaminated. Once more, how is it that one member of a household belonging to the upper class is attacked and dies, though all the rest, notwithstanding their being exposed to the same conditions, are preserved? Looking at animals, again, any Indian medical officer will tell Dr. MacCormac that monkeys kept in confinement, though they have never had a roof over their heads and have consequently never breathed air a second time, will die with their lungs stuffed with tubercle. Lastly, the evidence is very strong in favour of Virchow's view, that tubercular matter is originally composed of cells resembling the white corpuscles of the blood, which are either modified white corpuscles, or, as Virchow himself maintains, proceeds from the prolification of connective tissue corpuscles. Whilst disagreeing, therefore, with Dr. MacCormac in regarding the breathing of air imperfectly freed from the products of previous respiration as the exclusive cause of tubercle, we may fully endorse his views upon the desirability of thorough and complete ventilation, especially in our sitting-rooms and sleeping apartments. The exigencies of modern civilisation seem to lead unavoidably to the close herding of mankind; but we confess it is with a sigh of regret that we see year by year long lines of close-packed houses, springing up on what were but recently green fields on every side of this great metropolis. To reach green fields and breathe fresh air is now a day's work.

H. Power

Theory of Friction. By John H. Jellett, B.D., P.R.I.A. (Dublin: Hodges and Co.; London: Macmillan)

This book is, to a certain extent, of the character of a supplement to ordinary treatises on mechanics. It deals with the question of friction by the use of analytical expressions very general in the possibility of their application, on which account perhaps some of the significance of their physical character may be apt to escape the general reader, and the book is thus, perhaps, rather more suitable for advanced than for junior students.

The author brings well into prominence the radical difference between problems in statical and dynamical friction, namely, that the latter are determinate, whereas the former are not necessarily so. He says:—

"When a system of material particles, each of which rests on a rough surface, is subject to the action of external forces, it will in general be found that, of these particles, some will be in a state of motion and others in a state of rest. Everything connected with the moving particles, namely, their positions, their velocities, and the forces, geometrical and frictional, which act upon them, is fully determined by means of the dynamical and geometrical equations. The geometrical and frictional forces which act upon the quiescent particles will also be determinate, unless it be possible to form by elimination one or more equations between the co-ordinates of the quiescent particles *only*. If this *be* possible, the geometrical force replacing every such equation will be indeterminate in intensity."

The character and cause of the analytical indeterminateness in the case of statical friction is enunciated in the following words, which obviously apply also to forces not frictional:—

"If any one or more of the forces acting upon the particles of a system be not determinate functions of the co-ordinates, the number of the unknown quantities will exceed the number of equations, and there will be in general an infinite number of positions satisfying the conditions of equilibrium, disposed in one or more groups, in each of which these positions succeed one another continuously."

There is an interesting chapter on the distinction between necessary and possible equilibrium, arising, so far as friction is concerned, from the fact that the coefficient of dynamical friction is less than that of statical friction, so that "if the system be disturbed from its position of equilibrium by the communication of infinitely small velocities to its several points, when the friction at each point will, of course, become dynamical, a finite force tending to augment the displacement may at once be developed at some or all of these points." The whole point of distinction between this and ordinary unstable equilibrium, when friction is not taken into account, consists in the fact of the infinitely small velocity calling into play a finite force, which it would not do in the case of ordinary unstable equilibrium, in the lapse of a finite time. Without questioning the analytical excellence and interest of the investigation, we may hesitate in adopting the change from statical to dynamical friction as a consequence of the assumption of an infinitely small velocity. We would point to the following problem (page 170) as a good example of the concrete application of the principles of the treatise:—"Two rods, AB, CD, firmly jointed together at B, rest so that A presses against a rough vertical surface, and CD lies on a rough peg in the same vertical; find the limiting positions and the nature of the equilibrium."

At the end of the book there are several problems worked out, namely, the well-known problem of a top spinning on a rough plane, the problem of "friction wheels," and one or two problems connected with the driving wheels of locomotives.

J. S.

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 Adamites

I should not have noticed the letter of "M. A. I.," which appeared in the last number of Nature, with reference to my paper on "The Adamites," were it not that my silence might be interpreted as an acknowledgment of the justice of the remarks of the anonymous writer. If I had been silent, however, I trust your readers would have had more sense than to accept the dictum of a writer, anonymous or otherwise, who thinks to negative the conclusions of a paper, written at least in a truly scientific spirit, by such nonsense as the reference to *Paddy* and *Taffy*. One looks for reasoning in the criticisms which appear in such a journal as Nature, and not for a misleading statement of an opponent's position, supported by reference to general conclusions and the use of weak satire. When "M. A. I." condescends to advance an argument, I shall be happy to consider it; and if it should be unanswerable, I shall not hesitate to admit it to be so. Doubtless I ought to feel thankful for the tenderness with which he has trodden on my toes, but I have scant regard for mere courtesy where questions of science are at stake; and in the interests of truth I would rather that the errors of my "unlucky paper" should be openly exposed, than that I should be "damned with faint praise."

Hull, April 8 C. STANILAND WAKE

The Aurora of February 4

THE Scottish Meteorological Society has just received the schedules of its observers in Iceland and Farö for February last. At Stykkisholm, on the north-west of Iceland, auroras were seen on each of the nights of the 3rd, 4th, and 5th, and at Thorshavn an aurora of a remarkably red colour was observed in the S.E and S. in the evening of the 4th. It was also observed at North Uist, Shetland, of a very red colour, and over all the S.E. of the sky; at Monach, the most western island of the Hebrides, and at nearly all the 150 stations which report to the Society, appearing at some places as early as 5 P.M., and continuing visible at others till half-past one on the morning of the 5th. Major Stuart, the Society's observer at Janina, Greece, also reports an aurora on the 4th from 6 30 P.M. to midnight.

On the evening of the 4th much thunder and lightning occurred in Monach, South Uist, Skye, and others of the Western Isles, and on the mainland of Scotland adjacent, even as far inland as Corrimony, fifteen miles west of Loch Ness.

The weather preceding and following this aurora was very remarkable. At Stykkisholm, 22° 43′ W. long., the mean height of the barometer from the 30th of January to the 5th of February was only 28·798 inches, and the wind N.E. throughout, except on one of the days, when it was E. At this same place a storm of wind, with snow showers, began at 1 A.M. of the 30th of January, and continued without intermission for 102 hours, or till 7 A.M. of the 3rd, on which day and on the 4th the weather was fine and seasonable and the wind light.

At Monach, 7°34′ W. long., a storm of wind began at 6 A.M. of January 30 and continued to blow from W.S.W., S.W., and S. till 2.30 A.M. of February 5, having thus lasted about 140 hours.

On the west of Scotland and the Western Isles, a heavy storm of wind from S. or S.W. was blowing during the evening of the 4th, the sky being generally clear, and the aurora, consequently, well seen. But at some places the sky presented a strange lurid appearance, as the aurora appeared through the opening clouds as they drifted past. Shortly after the disappearance of the aurora, the wind moderated and fine weather followed.

But in the east of Scotland the storm from the south, accompanied with drizzle and mist, did not break out till the morning of the 5 h, or some time after the aurora had disappeared. It was to have been expected that an aurora extending over so much of the earth's surface would be preceded, accompanied, and followed by very different weather in different regions; and we have seen it coming thirty-six hours after a protracted period of stormy weather in Iceland, closing an equally protracted period of stormy weather in West Hebrides, and preceding a storm of wind and rain in the east of Scotland.

ALEXANDER BUCHAN

Scottish Meteorological Society, Edinburgh, April 8

HAVING seen an account of the aurora borealis which was visible in England on the night of February 4, I think that you or some of your scientific friends might like to know that a very brilliant display of aurora was visible here and in other parts of the West Indies on the same night.

On the night of February 4, I was going from Porto Rico to Puerto Plata in, roughly speaking, lat. 19° N., long. 48° W. The aurora was first seen at 8.30 P.M., was most brilliant at 10 P.M., and gradually died away by midnight; the corresponding times at Greenwich would have been 1 A.M., 2.30 A.M., and 4.30 A.M., February 5.

I have several times seen auroras off the Western Islands, but only remember having seen one several years ago in the West Indies.

There were no pillars or points of light in this aurora, but a bright flush in the northern sky, which surged up and died away again every now and then, and was brightest about 10 P.M.

STEPHEN DIX

H.M.S. *Mersey*, St. Thomas, March 14

THE aurora of February 4 was visible at this point, but seems to have been unobserved, except by a very few. My position was on the deck of a steamboat on the river going from this point to one 23 miles miles higher up. The aurora was first noticed by me at about 7 P.M., hanging over the woods to the north-east, and was mistaken by the Captain for a large fire, a common occurrence in our pine forests. Soon after, the glow, which was a very deep red, extended to the zenith, shading off there, whilst a much fainter red light appeared in the north-west.

My last observation was made at 8.30 P.M., and the light was then still very strong in the north-east. Being then upon a train, and passing through an unbroken pine forest, I could not note the time of disappearance of the display. I saw no streamers.

There was no aurora whatever to the south at any time visible from at least sunset to 8.30 P.M. The facilities for observing the sky in that direction were peculiarly favourable from the position upon the river. F. G. BROMKERG

Mobile, Alabama, U.S.A, March 23

On the Colour of a Hydrogen Flame

A CORRESPONDENT to your last number has troubled himself to propound an elaborate theory, to account for the blue tinge which he states is always exhibited by the flame of hydrogen. There are also several text-books on chemistry which assert that hydrogen burns with a characteristic faint blue flame. It is easy to prove, however, that the flame of pure hydrogen has no blue tinge whatever. The blueness so frequently associated with the flame of hydrogen is really due to the presence of sulphur, as is shown in a little paper I published in the *Philosophical Magazine* for November 1865.* It is possible that the facts mentioned in that paper may be turned to a practical end by some of your readers, and therefore it may not be altogether useless if I put down—for such disposal as you deem proper—one or two interesting phenomena associated with the combustion of hydrogen.

There must I imagine be some people who write text-books on experimental science without having verified any of the facts they state. Otherwise one cannot account for some obvious errors which are propagated from one writer to another. The blueness of a hydrogen flame is one such error, and another still more glaring can be traced back through several high authorities. The fact is stated that a rod of iron, or a sewing needle, remains suspended in the centre of a helix of wire through which an electric current is passing. So long as the helix is animated by the current the iron is said to behave like Mahomet's coffin, and hang in the air without the least contact with any solid body. But this is *not* the case, however strong the current, or small the iron, or however the helix may be disposed.

More serious errors than these are to be met with in some of the little books on science for school use, that are now cropping up like mushrooms. Heads of schools cannot exercise too much caution in the introduction of text-books on science, for they know how a poor class book once in a school is a most difficult thing to eject. It is therefore impossible to over-estimate the value of books for boys written by men like Profs. Huxley, Roscoe, and Balfour Stewart. An extraordinary impulse to scientific teaching has been given by the manuals of these and other eminent authors, and of the gladness with which such books are received by elder boys I, like others, can testify.

And now, as a teacher, permit me, Sir, to tender to the same authors not only my own gratitude, but the genuine and hearty thanks of younger boys for their simply delightful Science Primers. W. F. BARRETT

International College, Spring Grove, W.

[We hope to give in our next number a summary of the experiments to which our correspondent alludes.—ED.]

Barometric Depressions

I HAVE only just seen Mr. Murphy's criticism on my paper, which appeared in your columns on the 21st ult. I intended that paper as a continuation of one which appeared last year. The former aimed at showing that the ordinary variations of the barometer could not be explained by aqueous vapour; the latter at proving that they were accounted for by the heating and cooling of dry air. Into this question of air *versus* vapour the earth's rotation did not enter, and I consequently took no account of it

* A year or two ago I was surprised and amused to read this investigation repeated in the pages of the *Comptes Rendus*. I forget the name of the French chemist who contributed it to the Academy, but he was doubtless unaware of anything I had written on the subject.

in my reasonings. The casual remark, however, which Mr. Murphy fastens on as involving "a serious mistake in the theory of the trade winds," was almost copied from Article 211 of "Tyndall on Heat;" and as to the matter of fact, I think it is Mr. Murphy, and not Prof. Tyndall or myself, who has fallen into error. Even if I saw any reason why east and west winds should exactly balance each other on the earth's surface, I could not accept Mr. Murphy's position, that if the earth were of any other shape the trade winds could not proceed from the medial line to the extremities. He assumes that the trade winds are east winds, *independently of the shape of the earth*, whereas it is just the shape of the earth that makes them east winds. If the earth were a cylinder revolving on its axis, the trade winds (if they could arise under the circumstances) would move directly north and south, and would not be east winds at all; and I can see no reason why they should not extend to the extremity of the cylinder. See "Tyndall," *loc. cit.*

Trinity College, Dublin, April 3 W. H. S. MONCK

Height of Cirrus Cloud

IT would be interesting if any of the readers of NATURE could give some information respecting the usual height of cirrus clouds. Mr. Clement Ley, in his work, "The Laws of the Winds," states—"The time occupied by these clouds in passing from the zenith to 45°, or the contrary, furnishes us with a standard of measurement which is both convenient for simultaneous observations, and also possesses this obvious advantage, that whenever the altitude of the cloud station is at all determinable, none but the simplest of calculations is required in deducing the actual from the apparent velocity." Granted; but it would have been advantageous had he shown by an example what he means. For, he goes on to say, "The ordinary range of the actual rapidity of this current is about twice as great as that of the rapidity of the surface winds, for while the latter, at stations most fully exposed to their violence, rarely attain, in Europe, a velocity of 60 or 70 miles an hour, the most elevated clouds not uncommonly traverse a distance of 120 miles an hour, and occasionally much more." Coupling this with the next statement—"I have only once or twice observed an actually motionless cirrus cloud, and it is on rare occasions that an hour is occupied in passing from the zenith to 45°," let me ask, what would be the vertical height of such a cloud? R. STRACHAN

Low Conductivity of Copper Wire

As one of very numerous instances which have come under his notice, Sir William Thomson desires to make known the following case of the employment of inferior copper wire in the construction of electrical apparatus. He received lately from a Glasgow bell-hanger a large quantity of cotton-covered copper wire, which was being largely used for the coils of electric bells, and upon having it tested very accurately by means of his new Multiple Arc Conductivity Box, its resistance per metre-gramme was found to be no less than 0·439 of a B. A. unit; that of ordinarily good copper wire for such purposes being about 0·16 of a B. A. unit. J. M.

A Pelagic Floating Fish Nest

AMONG other rarities which I have been fortunate enough to procure since my arrival in the Bermudas, is a pelagic fish nest, similar in most respects to that which Agassiz has so recently described, and which was obtained by the American Expedition in the Gulf Stream in December last, while on the voyage to the West Indies. As I am very busy at present preserving and packing specimens, and the mail steamer nearly due, I have only time to send you (by way of St. Thomas) a brief description of my nest, which has been preserved in diluted alcohol. It was taken from a mass of gulf weed (*Fucus natans*) blown ashore about a month ago. This weed, by-the-bye, has been especially abundant about the Bermudas during the present winter, thousands upon thousands of tons having been cast ashore by the waves during the stormy weather which has prevailed. The size of the whole mass is about eight inches by five as it hangs suspended, the former measurement being its depth. The weed is thicker at the top, and is woven together by a maze of fine elastic threads, affording a raft, from which depends the clustering mass of eggs, which I cannot illustrate better than by asking your readers to imagine two or three pounds of No. 7 shot grouped together in bunches of several grains, and held in position by the elastic thread-work previously mentioned. These threads are amazingly strong, especially at their terminal bases on the fucus sprays, where several are apparently twisted together like the fibres of rope, and are admirably adapted to hold the mass in a position where it must always be subject, more or less, to violence, from the continued agitation of the waves in these stormy latitudes. The sea-weed is not only on the summit, but sundry sprays are interwoven with the mass of eggs, thereby rendering the fabric still more solid and secure. It is truly a wonderful specimen of Nature's handiwork; a house built without hands, resting securely on the bosom of the rolling deep.

J. MATTHEW JONES

"An Odd Fish"

SOME short time ago I observed in one of the daily papers an account of "an odd fish" which had been captured, and described by Prof. Agassiz as a denizen of the Gulf weed, on which it is said to walk with legs, and not to swim as other fishes do.

From the above account I suppose that I must have caught the fish in question in July last, during the homeward voyage of H.M.S. *Charybdis*, in lat. somewhere about 15° N., and from the Gulf weed, as described by Prof. Agassiz. The preparation I shall be happy to present to the British Museum if it should turn out to be a species of which no specimen exists in that institution.

It will be observed that the pectoral fins are developed into arms, and the ventrals into legs, though less perfect in form than are the arms.

Sir Philip Egerton has seen it, and pronounces it to be a species of blenny, a shallow water fish; and Capt. Spratt has kindly informed me that it recalls to his mind a theory entertained by the late Prof. Forbes, that the Gulf weed is the product of a shallow water, such as existed before the subsidence of the Miocene formation; and that it may contain a shallow sea fauna, although found in latitudes where the ocean is deepest.

It is a curious fact if such be the case, and one which would appear to have its counterpart in the deepest holes from which Forbes dredged molluscs, which have continued to live therein, and to have survived their congeners of former geological epochs.

J. E. MERYON

The Law of Variation

IN Mr. A. W. Bennett's notice of the sixth edition of the "Origin of Species," he calls attention to the insufficiency of the theory of "Natural Selection" to explain original variations, and says, "If it is admitted that important modifications are due to 'spontaneous variability,'" &c. Now is there no cause for primary, or spontaneous variability?

Is it not presumed under the law of inheritance that, in order that the offspring may be the *exact type* of the parent form, all the conditions of generation and life, and all the forces that affect life, whether generating or external, must be *precisely the same?* Strictly speaking, under the varying circumstances of life, this is never the case; hence slight individual variations; for no individual force can operate as a cause without its effect. These caused variations may sometimes be wide, and may be helpful or hurtful; if helpful, "Natural Selection" would take them up and preserve them and improve them. A. J. WARNER

Marietta, Ohio, March 14

Actinic Power of the Electric Light

MR. MEEZE says in NATURE of the 4th, "May not the great actinic power of the electric light be due in a great measure to the secondary waves produced by the magnitude of the disturbing force?"

This may be true, but there is a cause for the fact which is known to exist, namely, that the electric light is bluer than solar light, that it is to say, it contains a greater proportion of the shorter and more refrangible waves, which have the greatest actinic power. This is due to the absorption of more of the shorter than of the longer waves—in other words, absorption rather at the blue than at the red end of the spectrum—which takes place in the sun's atmosphere. In the magnesium light also, great actinic power is associated with a blue tint.

JOSEPH JOHN MURPHY

Protective Mimicry

In Nature, No. 126, for 28th ult., at p. 436, M. G. Pouchet is recorded to have stated in a paper read before the Academy of Sciences, first, that prawns accommodate their colour to that of surrounding objects; second, that removing their eyes prevents this change of colour.

Of the truth of the first assertion I presume there is no doubt; but of the second I should much like to learn further, for when we speak of Protective Mimicry in all the lower forms of life, we do not assume that there is any ratiocinative mimicry. Yet if this power of protective mimicking in the prawn is dependent upon eye-sight, *i.e.*, upon the power of conveying impressions upon the optic nerve to the brain, does it not cease to be "mimicry" as generally understood, and pass into the order of mental volition? If so, how vast and interesting is the consideration!

I hope that Mr Darwin, Mr. Wallace, or some other of your scientific contributors will enlighten through your columns

March 31 Ignoramus

CRANIAL MEASUREMENTS

WHILE engaged in the investigation of another matter, I was induced to make a series of cranial measurements, and these I wish to record under the impression that they may be of use in the hands of some future worker, though by themselves they are not of much value.

The measurements were made at Wakefield, in Yorkshire, during 1868-9, and are those of the working-classes of the town and neighbourhood. Careful inquiry was made as to the birthplace and parentage of each subject, and no measurements are given save of those belonging to the basin of the rivers Calder and Aire. The type of the people is pre-eminently Saxon, and the results may therefore be taken as pretty accurately representing the configuration of the crania of modern Yorkshire.

Attached are also the average height and weight for each decade, and a calculation of the average cephalic index.

The measurements of the head were taken by large callipers, and are simply the greatest bi-parietal and occipito-frontal diameters, and the measurement of the face is from the tip of the chin to the root of the hair on the forehead.

The average cephalic indices of the whole would show men to be slightly more brachio-cephalic than women (by ·75), while the result of the whole is decidedly eurycephalic.

The cephalic indices of each decade of age would lead us to believe that dolicho-cephalic people have a better chance of life than the brachio-cephalic people, unless we believe that the form of the cranium alters between thirty and forty years of age.

The entire table leads me to believe that there is not much value to be placed in such cranial measurements for the purposes of racial distinction; certainly not in isolated skulls; for see the curious variations of measurement in couples of the same sex taken from the same decade, as shown in the table below:—

Age.	Length of head.	Breadth of head.	Cephalic Index.	Difference in Cephalic Index.
Years.	Inches.	Inches.		
3	7·25	5·25	72·41	15·59
3	6·25	5·50	88·00	
14	7·12	5·37	75·42	8·55
14	7·12	6·00	84·27	
25	7·37	5·75	78·02	8·84
28	6·62	5·75	86·86	
32	5·87	6·12	104·26	26·12
35	7·00	5·75	82·14	
38	8·00	6·75	84·37	19·89
43	8·00	6·00	75·00	35·71
47	7·00	7·75	110·71	
52	7·75	6·00	77·42	5·34
52	7·25	6·00	82·76	
65	7·00	5·50	78·57	9·21
69	7·12	6·25	87·78	

For the purpose of contrasting the results I have obtained in the measurements of height and weight, I add a translation of Quetelet's tables:—

Men.			Women.		
Age.	Size.	Weight.	Age.	Size.	Weight.
	Ft. In.	Lbs.		Ft. In.	Lbs.
Birth	1 7·527	7·052	Birth	1 7·008	6·413
1	2 3·401	22·040	1	2 3·136	20·497
2	2 7·377	26·448	2	2 6·758	25·125
3	2 9·858	29·115	3	2 9·456	27·440
4	3 0·692	33·214	4	2 11·020	31·253
5	3 2·776	36·807	5	3 2·340	34·162
6	3 4·964	39·760	6	3 4·620	36·905
7	3 7·779	44·433	7	3 7·152	40·644
8	3 10·068	49·061	8	3 8·832	43·683
9	4 0·307	53·094	9	3 11·264	49·458
10	4 2·473	57·569	10	4 1·128	53·425
11	4 4·484	61·381	11	4 2·196	57·855
12	4 5·504	68·324	12	4 4·236	67·320
13	4 7·234	77·846	13	4 6·364	75·079
14	4 10·543	89·262	14	4 8·964	83·973
15	5 1·378	102·288	15	4 10·068	91·026
16	5 3·386	117·672	16	4 11·032	97·946
17	5 5·748	126·510	17	5 1·176	108·175
18	5 6·684	135·018	18	5 1·488	117·003
19	5 7·164	139·558	19		(not given)
20	5 7·356	143·261	20	5 1·800	120·030
25	5 7·788	150·512	25	5 2·076	121·397
30	5 7·788	151·857	30	5 2·160	121·529
40	5 7·440	151·658	40	5 1·212	124·857
50	5 5·904	148·661	50	5 0·423	128·826
60	5 4·524	144·363	60	4 11·673	125·034
70	5 3·888	138·919	70	4 11·604	118·400
80	5 3·404	134·930	80	4 11·294	112·551

MALES.

Age.	No. of Individuals.	Height.	Weight.	Length of head.	Breadth of head.	Face.	Cephalic Index.
		Feet. In.	Lbs.	Inches.	Inches.	Inches.	
3 to 4 months . . .	8	1 11·00	12·00	5·12	4·12	3·94	80·47
6 to 12 ,, . . .	8	2 2·50	21·12	5·69	4·62	4·50	81·20
12 to 18 ,, . . .	10	2 4·50	22·50	5·50	5·00	4·75	90·91
18 months to 2 years .	20	2 5·25	23·25	6·33	5·12	5·00	80·88
2 to 3 years . . .	14	2 9·50	25·25	6·75	5·20	5·33	77·04
3 to 5 ,, . . .	17	3 5·00	36·17	6·62	5·37	5·37	81·12
5 to 7 ,, . . .	9	3 7·50	39·89	6·87	5·75	5·81	83·70
7 to 10 ,, . . .	17	4 3·25	56·75	7·09	5·75	6·12	81·10
10 to 15 ,, . . .	40	4 7·76	78·50	7·25	5·75	6·50	79·31
15 to 20 ,, . . .	22	5 5·17	120·33	7·50	6·00	7·12	80·00
20 to 25 ,, . . .	13	5 7·67	152·00	7·50	6·00	7·33	80·00
25 to 39 ,, . . .	18	5 7·25	149·50	7·50	6·00	7·67	80·00
30 to 40 ,, . . .	29	5 7·00	146·33	7·11	6·00	7·00	84·37
40 to 50 ,, . . .	37	5 2·50	148·00	7·83	6·00	6·00	76·63
50 to 60 ,, . . .	47	5 8·12	139·50	7·50	5·87	7·50	78·27
60 to 70 ,, . . .	20	5 8·12	126·00	7·40	5·75	7·33	77·70
83½ years	1	5 10·00	174·00	7·87	6·00	6·87	76·24
Total	330					Average. .	80·04

FEMALES.

Age.	No. of Individuals.	Height.	Weight.	Length of head.	Breadth of head.	Face.	Cephalic Index.
		Feet. In.	Lbs.	Inches.	Inches.	Inches	
10 weeks	1	1 6·00	10·00	4·87	3·87	3·62	79·44
6 to 12 months . . .	9	2 1·57	16·29	5·71	4·75	4·75	83·19
12 to 18 ,, . . .	11	2 4·56	19·50	6·25	4·87	4·98	77·92
18 months to 2 years .	8	2 5·87	22·94	6·39	5·05	5·08	79·03
2 to 3 years . . .	9	2 7·94	25·44	6·35	5·35	5·37	84·25
3 to 5 ,, ...	13	2 11·62	30·31	6·75	5·25	5·35	77·78
5 to 7 ,, ...	17	3 4·91	39·03	6·86	5·37	5·57	78·28
7 to 10 ,, ...	7	3 11·75	51·50	6·79	5·41	5·79	79·68
10 to 15 ,, ...	35	4 2·50	75·74	6·88	5·59	6·29	81·25
15 to 20 ,, ...	49	5 0·94	110·14	7·08	5·67	6·65	80·08
20 to 25 ,, ...	24	5 6·95	121·00	6·57	5·51	6·44	83·87
25 to 30 ,, ...	44	5 1·96	119·45	6·99	5·88	6·66	84·12
30 to 40 ,, ...	49	5 1·43	124·74	7·22	5·36	6·71	74·24
40 to 50 ,, ...	64	5 2·70	132·34	7·31	5·77	6·27	78·93
50 to 60 ,, ...	46	5 2·66	135·17	7·32	5·80	6·44	79·79
60 to 70 ,, ...	17	5 1·94	133·29	6·93	5·52	6·64	79·65
70 to 80 ,, ...	5	5 2·10	123·50	7·27	5·82	6·47	80·06
Total	408					Average .	79·30

LAWSON TAIT

ONE SOURCE OF SKIN DISEASES

OBSCURE affections of the skin of the face of men especially are well known to specialists to be widely spread. They are commonly classed as *ekzema*, and while causing great discomfort especially at night, show nothing, or almost nothing, to the eye, if the patient be otherwise in pretty good health. Skin specialists frequently ask patients whether they have been using any new sort of soap, but no one seems hitherto to have traced any distinct communication between soap and this troublesome disease.

As I have been able pretty distinctly to do so in reference to myself, probably a brief notice of the facts may not be out of place in NATURE, where it is likely to be of more popular benefit than if committed to the pages of a medical journal, in which the inferences of "mere laymen" are not greatly reputed. It is a fact but very little known to the multitude of both sexes who use the "Prime Old Brown Windsor Soap" of the perfumers' shops, that by far the largest proportion of it is manufactured from "bone-grease." Few more beautiful examples of chemical transformation are to be found in the whole range of chemical manufacture than this one. At one end of a long range of buildings we find a huge shed heaped up with bones, usually such as are of little value to the bone-turner or brush-maker, in all stages of putrefaction as to the adherent or inherent portions of softer animal matter attached to them, the odour of which is insupportable.

These are crushed and ground to a coarse powder, exposed to the action of boiling water under pressure, sometimes of steam, until the grease and marrow are extracted.

We need not here pursue the subsequent treatment of the rest of the material from which bone glue and "patent isinglass" are prepared, the latter of which we often eat in the soups and jellies of the pastrycooks, and finally to the "bone dust," or phosphate of lime, nearly free from animal matter, which is produced for the use of the assayer and the china manufacturer, &c., as well as for other purposes in the arts.

But let us follow up the bone-grease, which is of a dark tarry brown colour, and of an abominable odour.

By various processes it is more or less defæcated, bleached, and deodorised, and is separated into two or three different qualities, the most inferior of which goes to the formation of railway or other machinery greases, and the latter is saponified, and becomes, when well manufactured, a hard brown soap, still, however, retaining an unpleasant smell. It is now, after being remelted, strongly perfumed, so that, like the clothes and persons of the magnates of the Middle Ages, its own evil odour is hidden by the artificial perfume.

This is the "Fine Old Brown Windsor Soap" of most of our shops. The natural brown colour of the grease gives it the right tint in the cheapest way, without the colouring by caramel, which was the original method of manufacture.

Like all other things, there are cheap and dear Windsor soaps; and for the production of the former little is done beyond saponifying and casting into blocks or bars. Were we to rely upon the many experiments that have been made as to the degree of elevation of temperature at which putrescent or other contagious matter is deprived of its morbific power, we might conclude that boiling and saponifying had made this hitherto putrescent grease innocuous.

It seems, however, more than doubtful that such is the fact in this case, for the soap thus made seems to be capable of communicating skin diseases when rubbed on the face for use in shaving.

But another promoter of irritation is not unfrequently also found. Whether it be that it is more profitable to the soapmaker to have a liberal proportion of the finer particles of the ground bone made up with the soap, or that these are difficult to separate completely, the fact is that bars of this "Brown Windsor Soap" are to be bought containing a rich mixture of those small sharp angular fragments of bone which before boiling was putrid. When a piece of such soap is rubbed hard to a man's face, the skin is more or less cut and scored by these bony particles held in the soap like emery in a head "lap," and thus the skin is placed in the most favourable state to absorb whatever there may be of irritant, or contagious, or putrid in the soap itself. The existence of the bone fragments is easily verified by solution of the soap in water or alcohol, and examination of the undissolved particles with a lens; and I can readily, if need be, send you a piece of such soap for examination.

Now, without occupying too much of your space, I may just state that I have while using such shaving soap thrice suffered from ekzema of the face. On the first occasion I derived no benefit from treatment by the two most celebrated dermal surgeons in London, and at last the disease went away of itself after giving up shaving for a time. I had by me a quantity of this brown soap, and through inadvertence took to using it again, for a time without effect; but when dry and hot weather came, with it came a recurrence of the skin disease, which also again, after some months of discomfort, went away. Curious to make sure whether or not the soap was the real cause, I a third time employed the soap deliberately to see if the ekzema was due to it. I was in excellent health, and in about three weeks I found the disease reestablished, so that I think the soap must be viewed as found guilty. Good white unscented curd soap is now my resource, and with no ill-effects.

Ekzema is always a distressing complaint even when affecting those in the most robust health. With those of bad constitution or lowered health, however, it seems to degenerate into bad or intractable skin diseases, so that probably this notice may not be deemed useless or uncalled for. R. M.

THE SCHOOL OF MILITARY ENGINEERING

THERE are few educational establishments, in this country at any rate, that fulfil their object so aptly and so well, as the School of Military Engineering at Chatham. When we remember the many sciences and technical accomplishments with which the officers of the Royal Engineers are conversant, and the practical use that many of them are required to make of their acquirements, it is very obvious indeed that, to be successful, the system of education must be a most complete and substantial one. It is, in truth, necessary that a man entering either of the scientific corps of the army—the Royal Artillery or Royal Engineers—should not only be intuitively quick and clever, so as to grapple with the multifarious subjects of study, but it is moreover quite as indispensable that he should be at the outset sufficiently strong and healthy to withstand the wear and tear of so much hard work. To become a Mr. Toots would, we fear, be the fate of many young gentlemen, were they passed through the Woolwich Academy, and into these departments of the Army, without first undergoing a rigid medical examination; for the severe and lengthy curriculum is such as would certainly jeopardise the health of any but the strongest constitutions. Commissions in the Royal Artillery and Royal Engineers, be it remembered, have for many years past been obtainable only by open competition, the successful candidates being admitted into the Royal Military Academy, whence they are passed into the Army when found properly qualified. But to compete successfully for admission to the Academy in the first instance, involves already a knowledge of mathematics, of experimental and applied sciences, of languages, and other subjects too numerous to detail, such indeed as is scarcely possessed by other well-educated professional men; and this, bear in mind, is but the starting-point of the scientific soldier's education. At the Academy, where the course of special instruction sometimes continues for three years, he has to pass from a lower to an upper section, and when successfully through the examinations that beset him at every turn, he receives his commission in a provisional sort of way only. The successful Academicians highest on the list are sent to Chatham, to commence instruction in their duties as Royal Engineers, while the remainder complete their education at Woolwich and Shoeburyness, as lieutenants in the Royal Artillery. And if, after all this, there are yet dissatisfied spirits, who still exhibit a craving for more, then there is the staff college, the advanced class, instruction certificates, and other ends to be attained, enough in all conscience to satisfy the most ambitious.

It is to the School of Military Engineering that the young lieutenants of Engineers are sent for instruction in their various duties, and it is only after passing through a two years' course at this establishment that their commissions are actually secured to them. The professors, or instructors, as they are termed, are all officers of some years' standing in the corps, appointed by reason of their intimate acquaintance with the special subjects that they teach. These subjects are not only very various, but are, moreover, always increasing, as our system of warfare continually improves. Thus, besides the subjects of surveying, construction, estimating, fortifications, telegraphy, and other more ordinary, though not less practical, matters, there have been added of late years, chemistry, photography, army signalling, torpedo service, &c., all of which the Royal Engineer must know something about.

It is evident that mere theoretical instruction in matters like these would be of little use to men who occupy such practical appointments as are filled by most Engineer officers, and it is in this respect that the School of Military Engineering may claim superiority over kindred establishments. The studies, workshops, laboratories, and demonstrating schools are of the most complete description, while the outworks and broken ground upon the Chatham lines and around the Brompton Barracks afford ample scope for the practical prosecution of those studies which require a wide field of operations. It is this practical manner of going about one's duties that is calculated above all things to impart a thorough knowledge, and to inspire officers with true confidence in their abilities. Fortifications are designed, parallels drawn, mines prepared, bridges constructed, and other siege duties executed by the students themselves, to render them conversant with their duties practically as well as theoretically, while the appointment of temporary telegraph stations, the experimental application of explosive and torpedo charges, the actual exercise of signalling, both by day and night, impart experience which could not, of course, be gained by teaching or lectures in the schools.

But it is not only the officers who benefit by the Engineering School at Chatham. The non-commissioned officers, also, are required to attend instruction in field works, and can, indeed, if they desire it, pass through the entire system of study, a course imperative on all those desirous of promotion to "foremen of works," or to other similar positions. The sappers, too, are well acquainted with at least one trade, or calling, and as every company of Engineers is made up from a due proportion of all trades, it is obvious such a complete and intelligent body of men is ofttimes invaluable. Thus it is that, in the Colonies, in Australia, New Zealand, South Africa, and other stations where detachments of Royal Engineers have been sent, their services have been found so truly valuable, every available talent being at once at hand for the carrying out of the engineering and other kindred duties necessary to be fulfilled in the occupation of a rough untravelled country. As an instance of this, we need point merely to the recent Abyssinian Campaign, which may justly be called a triumph of engineering—a gigantic piece of road-making in fact—rather than a victory over half-naked Africans; for here we have in something like six months, a rough tract of country surveyed and mapped out, four hundred miles of road made, a line of railway laid down, telegraph communication established, wells sunk, and all this over and above the transport of a large body of men and war material.

The subject of torpedoes and submarine mines was so recently discussed in these columns, that we need not again refer at any length to this latest military science just now under special investigation at Chatham. But before concluding these few remarks, we may make mention of some experiments upon an extensive scale that were not long since made with these terribly destructive machines. The charges were fired from the shore by means of electricity, the signal for their discharge being given from the distance almost of a mile; and to show the control and certainty exercisable in the system employed, there was, in one instance, a steamer made to pass harmlessly over one of the charges, which immediately afterwards, at a given signal, blew into fragments a launch following in tow. H. B. P.

LYELL'S PRINCIPLES OF GEOLOGY

THE appearance of a new edition of the "Principles of Geology" would mark a fitting time to pass in review the state of Geologic Science, to count up what has been added to the treasury of truth, and inquire in what direction and by what methods the pioneers of Science encourage us to search for new facts. Within the limits of a short review, however, it is impossible to do more than call attention to a few of the more striking points which seem to illustrate the principles which we should apply to the examination of the phenomena of the crust of the earth.

We have before us the first edition of the "Principles of Geology," published in 1830, and that just issued in 1872. It is a remarkable fact that any work on a science which has made such rapid progress as Geology has within the last forty-two years, should, while maintaining the foremost place, have remained so little altered during that period. Almost all the passages which lay down the principles remain word for word as they were originally given to the world; the changes made from time to time being chiefly in the introduction of better illustrations or the consideration of new questions which the progress of research has raised; but to all we find the same methods applied, and from all the same conclusions drawn as to the operation of nature in the production of the visible crust of the earth.

What, then, are the principles laid down? Thoroughly to understand this, we ought to follow our author through the interesting outline he gives of the progress of geological inquiry, in order to realise fully the opinions which prevailed when he first entered the arena. But we will only refer to the views of Hutton, which most nearly approach those of Sir Charles Lyell, who points out that "the characteristic feature of the Huttonian theory was the exclusion of all causes not supposed to belong to the present order of nature. . . . But Hutton had made no step beyond Hooke, Moro, and Raspe, in pointing out in what manner the laws now governing subterranean movements might bring about geological changes if sufficient time be allowed. He therefore required alternate periods of general disturbance and repose; and such he believed had been and would for ever be the course of nature" (1st ed. p. 63, 11th ed. p. 76).

The views which Hutton and his eloquent illustrator

FIG. 1.—DWARF'S TOWER (ZWERGLI-THURM) NEAR VIESCH IN THE CANTON OF VALAIS.
(*From a Sketch by Lady Lyell, taken September 1857*)

Playfair taught were far from meeting with general reception, and Lyell had to combat the catastrophic views of their opponents, and also to carry Hutton's uniformitarian principle further than Hutton himself allowed, and show by an appeal to observations in regions which are and have recently been in a state of volcanic activity that local catastrophic action is not inconsistent with continuity of causation. "There can be no doubt," says Sir Charles, "that periods of disturbance and repose have followed each other in succession in every region of the globe, but it may be equally true that the energy of subterranean movements has been always uniform as regards the *whole earth*. The force of earthquakes may for a cycle of years have been invariably confined as it is now, to large but determinate spaces, and may then have gradually shifted its position, so that another region which had for ages been at rest became in its turn the great theatre of action" (1st ed. p. 64, 11th ed. p. 77).

Our author places before us a vast array of facts collected by himself and others all over the world, which show the ceaseless waste going on by rain, rivers, sea, frost, and ice.

> The hills are shadows, and they flow
> From form to form, and nothing stands.

He explains how all the land must in time be carried away and one vast ocean roll all round the world were there no compensating forces. But then he points out to us that nature does provide a compensating action in the accumulation of volcanic ash and lava thrown out during eruptions, in the upheaval of large tracts of land from below the sea, and still further, that it is part of nature's plan to shift the scene of action.

We will select a few examples from the facts adduced in proof of the gradual waste of the land.

Speaking of the effect produced by rain, our author says:—"It is not often that the effects of the denuding action of rain can be studied separately or as distinct from those of running water. There are, however, several cases in the Alps . . . where columns of indurated mud varying in height from 20ft. to 100ft., and usually capped by a single stone, have been separated by rain from the terrace of which they once formed a part, and

* "The Principles of Geology, or the Modern Changes of the Earth and its Inhabitants considered as illustrations of Geology." By Sir Charles Lyell, Bart. 11th and entirely revised edition. (London: J. Murray, 1872.)

now stand at various levels on the steep slopes bounding narrow valleys" (p. 329). "This mud, which is very hard and solid when dry, becomes traversed by vertical cracks after having been moistened by rain, and then dried by the sun. Those portions of the surface which are protected from the direct downward action of the rain by a stone or erratic block, become gradually detached and isolated." "The lower part of some of these ancient columns . . . has acquired new capping stones by the wearing out at the surface of blocks originally buried at great depths" (p. 332).

There they stand, a measure of the mass of drift that has been carried away by rain, as workmen sometimes leave a pillar of brickearth or clay here and there over a field to measure the depth of the earth they have removed.

They remind us also of the small pedestals of limestone which large boulders have sometimes preserved for themselves in the same way, and of the ice pillars where the thick stone cap had to keep off the sun instead of the rain.

By the courtesy of the publisher we are able to subjoin a sketch given by our author of an isolated stone-capped column seen by him near Viesch (Fig. 1).

In considering the action of rivers, Sir Charles notices how the clearing of forests increases the erosive power of the rain water. Speaking of a ravine in Georgia, he says, "before the land was cleared it had no existence, but when the trees of the forest were cut down, cracks three feet deep were caused by the sun's heat in the clay, and during the rains a sudden rush of water through the principal crack deepened it at its lower extremity, from whence the excavating power worked backwards till, in the course of twenty years, a chasm measuring no less than 55ft. in depth, 300 yards in length, and varying in width from 20ft. to 180ft., was the result" (p. 339).

In many parts of France the destruction of the woods has proved a source of very great injury, as they caught the rain and parted with it slowly, the roots all the while protecting the soil. But, now that the woods have been cut down, the water runs off at once, scouring away the earth from the slopes of the hills, and in the valleys causing sudden floods which sweep everything before them

In America it is especially interesting to watch the effect produced by man in this way upon climate and water supply.

We are shown the power of rivers, especially in flood, to tear away and transport to long distances the broken masses they find in their path. The glacier and ice-sheet, too, are for ever grinding and wearing the solid rocks away. But space will not allow us to give more than one other example, and we will select the formation of a pinnacle of solid rock by the action of the sea, which it will be interesting to compare with the column of indurated mud, of which we have given a sketch above.

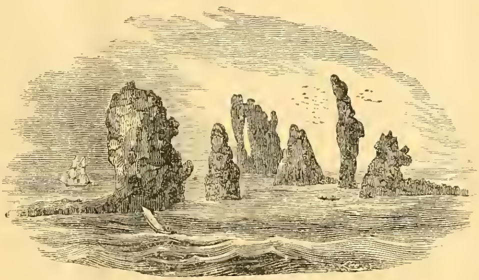


FIG. 2.—GRANITE ROCKS TO THE SOUTH OF HILLSWICK NESS, SHETLAND.

In considering the waste of sea cliffs, our author quotes Dr. Hibbert's account of a passage forced by the waves through rocks of hard porphyry, where the sea tears large masses of stone from the sides and forces them along, sometimes to a distance of no less than 180 ft., and adds:—"Such devastation cannot be incessantly committed for thousands of years without dividing islands, until they become at last mere clusters of rocks, the last shreds of masses once continuous. To this state many appear to have been reduced, and innumerable fantastic forms are assumed by rocks adjoining these islands, to which the name of Drongs is applied, as it is to those of similar shape in 'Feroe'" (p. 512). (Fig. 2.)

By such illustrations we are taught how ceaseless and how powerful are the destroying agencies of nature. But where is all this matter transported to? Sir Charles Lyell takes us out into mid-ocean, where he points out to us the icebergs carrying their load far and wide, and dropping it here and there over the sea bottom in warmer climes. On the shingle beach we see it travelling, and in the deep blue sea, says Dr. Tyndall, we see finely-divided matter still travelling on. With our author we examine the deltas of the great rivers, the Nile, the Ganges, and the Mississippi; and he shows us that some of the material is for a time arrested there. He tells us of the most recent discoveries in mid-Atlantic, where a chalky mud is being deposited over an area wider than that over which the ancient chalk sea has been traced; where swarms of little creatures live and die, and drop their tiny shells in such countless millions that the mud is in a great measure made up of them; where they

Sow
The dust of continents to be,

and give to us the explanation of the conditions under which that great deposit known as the Chalk was formed. Sir Charles Lyell refers to this in the following passage: "A fallacy which has helped to perpetuate the doctrine that the operations of water were on a different and grander scale in ancient times, is founded on the indefinite areas over which homogeneous deposits were supposed to extend. No modern sedimentary strata, it was said, equally identical in mineral character and fossil contents, can be traced continuously from one quarter of the globe to another. But the first propagators of these opinions were very slightly acquainted with the inconstancy in mineral composition of the ancient formations, and equally so of the wide spaces over which the same kind of sediment is now actually distributed by rivers and currents in the course of centuries. The persistency of character in the older series was exaggerated; its extreme variability in the newer was assumed without proof. In the chapter which treats of river deltas and the dispersion of sediment by currents, and in the description of reefs of coral now growing over areas many hundred miles in length, I shall have opportunities of convincing the reader of the danger of hasty generalisations on this head. I may also mention in this place that the vast distance to which the White Chalk can be traced east and west over Europe as well as north and south, from Denmark to the Crimea, seemed to some geologists a phenomenon to which the working of the causes now in action present no parallel. But the soundings made in the Atlantic for the submarine telegraph have taught us that white mud formed of organic bodies similar to those of the ancient Chalk, is in progress over spaces still more vast" (p. 109).

The teaching of Sir Charles Lyell is that all the rocks have been formed from pre-existing rocks as far back as we can trace them, in the same manner as they are being formed now, and that those which we see preserved are such as from their nature or surrounding circumstances were fittest to survive the various denuding forces to which they would from age to age be subjected.

Surely this is the true theory of evolution applied to geology. It does not, on the one hand, hold that the world has been going on always just as it is—that after a long period, during which all the varied forces of nature have been in full activity, the earth could be found in the same state as it was at the commencement. Nor, on the other hand, does it teach that the earth has been developed according to any original tendency or impulse, but that by the *uniform* action of forces such as we see now in operation it has been *evolved* out of previous states.

Nor is the objection valid that there is any "weakness or logical defect" in the teaching which would limit the inquiry to the period of which we have a record in the crust of the earth. If the true methods are employed, it is no objection to the methods themselves that their application is not more extended.

What were the possible or necessary first combinations out of a chaotic mass is a fair subject for investigation; but an author is no more to be censured for excluding it from a work treating of the visible crust of the earth, than a philosophic writer on the history of England is to be blamed for not including in his inquiry the conditions of that part of the earth now represented by our island previous to its last emergence from below the sea.

T. McK. HUGHES

(*To be continued.*)

NOTES

PROF. HUXLEY's friends will be rejoiced to hear that he has returned to this country, with his health and strength fully recruited by his absence from work; and that he has already resumed his lectures at the Government School of Mines.

THE *Examiner* prints the following extract of a letter from M. Elisée Reclus, dated Zurich, March 18:—"I am able at last to tell you that I am free. After having been kept for a long time in prisons, and sent from one prison to another, I left Paris for Pontarlier, escorted by two police agents, who left me on the free soil of Switzerland. While breathing and enjoying the pure air of liberty, I do not forget those to whom I am indebted for my freedom. Having been claimed by so many Englishmen as a student of science, I shall work on more than ever to show them my gratitude by my works and deeds."

THE Astronomer Royal will hold his first reception, as President of the Royal Society, on Saturday evening, the 27th inst.

IT will be seen from our report of the Proceedings of the Chemical Society that Prof. Cannissaro has been selected by the Council to deliver the Faraday lecture on Thursday, May 30.

THE Council of the Society of Arts has invited members of the Society to forward to the secretary, on or before April 29, the names of such men of high distinction as they may think worthy of receiving the Albert Medal, instituted to reward "distinguished merit in promoting arts, manufactures, or commerce." The recipients of the medal since its foundation, in 1864, have been Sir Rowland Hill, K.C.B., the Emperor of the French, Prof. Faraday, Sir W. Fothergill Cooke and Sir C. Wheatstone, Sir Joseph Whitworth, Baron von Liebig, M. de Lesseps, and Mr. Henry Cole, C.B.

MR. H. E. ARMSTRONG has been appointed Lecturer on Botany and Vegetable Physiology at the University of Durham College of Medicine, Newcastle-upon-Tyne.

A NUMBER of gentlemen connected with the Iron and Steel Institute, from the different parts of the Kingdom, and also from the Continent, assembled last week to the number of 200 or 300 at the Teeside Works, Middlesborough, belonging to Messrs. Hopkins, Gilkes, and Co., to witness the first public trial of the rotary puddling machine of Mr. Danks, to which we have recently referred. The machine has been in work for two or three weeks, and realises all that has been claimed for it by its inventor, and all that has been stated of its practicability by the Iron and Steel Institute Commission, which was sent to the United States to investigate the working of the machine. On Friday the gentlemen present saw the machine charged two or three times with molten metal, and generally the heat took about an hour, with all the different preparations, from the time of drawing the heat till the introduction of another. The quantity puddled at one time was between 5 and 6 cwt. generally, but as high as 1,000lbs. have been put into the furnace at one charge. The iron, after leaving the furnace, was hammered, and then re-heated and rolled into bars, the quality of which was stated to be very superior. They were all produced from No. 4, Cleveland pig iron. The "fettling" consisted of American ore and pottery mine. The important adjunct of a "seezer," which is part of Mr. Danks's invention, is not yet built, so that the operation was not complete. An unexpected occurrence happened later in the day, an opinion having been received from counsel that Mr. Danks's patent was not valid. A meeting was held between Mr. Danks and most of the gentlemen who had entered into the provisional arrangement to pay him by the 10th of April 50,000*l*. for the right of 200 of his furnaces, to which we have already alluded, and he was informed that the arrangement would not be ratified. The question remains open, and is entrusted to a committee of the gentlemen interested, who will report to a future meeting.

THE establishment is announced of a Meteorological Observatory at the top of the mountain of Puy-de-Dôme. The original cost of 1,000fr. will be borne one-half by the State, one-fourth by the town of Clermont, and one-fourth by the Council-General of Puy-de-Dôme. The annual cost of its maintenance will devolve on the town of Clermont.

CAPTAIN H.R.H. THE DUKE OF EDINBURGH, K.G., has signified his intention of becoming a vice-president of the Institution of Naval Architects.

UNDER the new management and direction of the Royal Polytechnic Institution, it has been determined to re-establish the scientific department of the Institution, and Mr. Edward V. Gardner has been appointed Professor of Chemistry. We understand that the Institution is about to arrange a well-organised laboratory, proper chemical accessories for lectures, classes, analyses, &c., of which due notice will be given in the papers when the arrangements are completed.

THE Council of the Literary and Philosophical Society of Leicester have received from Mr. John Bennett the sum of 20gs., for the purpose of offering prizes to students of Natural Science, and as an inducement to the useful occupation of that leisure which is afforded by shortened hours of labour. Mr. Bennett's prizes will be awarded immediately after Easter Week, 1873, according to the following plan :—1. Geology—Three prizes will be given of the value of 3gs., 2gs., and 1g. for the best collections of the Rocks of Leicestershire, named, and with the localities given from which they were obtained. 2. Botany—Three prizes will be given of the value of 3gs., 2gs., and 1g. for the best collections of dried specimens of the Flowering Plants of Leicestershire, properly mounted, with the name, locality, and date of gathering attached to each ; and classified according to the natural system. The scientific name must be given to each plant, and the popular name or names when it has any. 3. Freshwater life, animal and vegetable—Three prizes will be given of the value of 3gs., 2gs., and 1g. for the best aquaria, containing not more than two gallons of water, stocked with animal and vegetable life from the ponds, brooks, and rivers of Leicestershire, accompanied by a list of the specimens, with their scientific and popular names, and the locality and date of collection.

Mr. James Chamberlin, of Norwich, announces that, with the idea of improving the breeding of pheasants, he will award ten prizes varying in value from 1*l*. to 5*l*. for the best brace raised during the present year, on conditions which may be learned on application.

It is intended to form early in May a class for the study of Botany in the field belonging to the series of Church of the Saviour Science Classes. The object of the class is to enable Science students and others to obtain a practical knowledge of Systematic Botany, and to familiarise themselves with the form, structure, and habits of the principal flowering plants of the district. As the class will be limited in number, the names of intending students should be sent at once to the teacher—Mr. Joseph W. Oliver, 35, Cannon Street ; to Mr. W. T. Bulpitt, Albert Road, Aston ; or to the secretary, Mr. W. H. Hemming ; when arrangements will be made for a preliminary meeting.

Dr. Fraser will deliver two lectures on April 19 and 26 at 8 P.M., before the Fellows of the College of Physicians, on "The connection between the chemical properties and the physiological action of active substances ;" and on "The antagonism between the actions of active substances."

The third course of Cantor Lectures of the Society of Arts for the season will be by Prof. Barff, on "Silicates, Silicides, Glass, and Glass-painting," and will be delivered on Monday evenings, from April 8 to May 13.

We understand that at the request of the executors of the late Sir James Y. Simpson, his friend, Prof. Duns, has undertaken to write his biography.

The *Journal of Botany* mentions the appearance of a new botanical journal, under the title of *Journal de Botanique, pure et appliquée*, edited by M. G. Huberson, to appear fortnightly. It will contain, besides original communications, translations, extracts, and abstracts of botanical papers presented to the Académie des Sciences.

The Literary and Philosophical Society of Manchester has just published the tenth volume of its "Proceedings," containing an unusual number of papers of great value and interest.

We have received a copy of the lectures delivered at the Lecture-room of the Industrial and Technological Museum, Melbourne, for the autumn session of 1871. They deal with such subjects as Geology and Palæontology in their application to useful purposes, Respiration, Radiant Energy in relation to the spectrum, Forest culture in its relation to industrial pursuits, and various branches of manufacture. How long will it be before our Home Government undertakes such work ?

A very useful addition has been made to the series of publications issued by order of the Secretary of State for India in Council, in the form of "A Continuation of Maps of the British Provinces in India and other Parts of Asia, 1870."

Dr. Stoliczka, the palæontologist of the Geological Survey of India, has, during his stay on deputation in Kutch, made, according to the *Times of India*, an extremely valuable collection of zoological and fossil specimens. The doctor, it is said, anticipates that fully one-half of the latter are new to science.

The sale of Wombwell's Menagerie, to which we referred a few weeks since, took place at Edinburgh on Tuesday last. Among the prices realised were the following :—Tasmanian devil, 3*l*. 5*s*. ; Diana monkey, 7*l*. ; mandrill, 30*l*. ; ditto, 5*l*. ; Anubis baboon, 10*l*. 10*s*. ; ditto, 8*l*. 10*s*. ; condor, 15*l*. ; emeu, 7*l*. ; pelicans (two), 6*l*. 15*s*. each ; nylghau, 26*l*. ; ditto, 10*l*. 10*s*. ; lama, 15*l*. ; boomer kangaroo, 12*l*. ; ocelot, 6*l*. 10*s*. ; African porcupines (three), 5*l*. 10*s*. each ; wombat, 7*l*. ; Polar bear, 40*l*. ; brown bear, 7*l*. ; performing leopard, 20*l*. ; performing leopardess, 20*l*. ; ditto, ditto, 20*l*. ; performing hyæna, 3*l*. 5*s*. ; lion, "Wallace," 7½ years old, 85*l*. ; royal Bengal tigress, in cub, 3 years old, 155*l*. ; lion, "Duke of Edinburgh," 3 years old, 140*l*. ; lionesses, "Princess" and "Alexandra," about 3½ years old, 80*l*. each ; lioness, "Victoria," 4 years old, in cub, 105*l*. ; black-maned lion, "Hannibal," 6½ years old, 270*l*. ; lion, "Nero," 7½ years old, 140*l*. ; lion, "Prince Arthur," 18 months old, son of "Hannibal," 90*l*. ; lion, "Prince Alfred," 18 months old, son of "Hannibal," 90*l*. ; spotted hyæna, 15*l*. ; Burchell zebra, 50*l*. ; gnu, 85*l*. ; male tusked elephant, 7 feet 6 inches high, nearly 8 years old, 680*l*., bought for the Zoological Gardens, Manchester; female elephant, 5 feet 6 inches high, 145*l*. ; two boa constrictors, 6*l*. each; Malabar squirrel, 5*l*. ; male Bactrian camel, 7 feet high, 12 years old, 19*l*. ; female ditto, in calf, 6½ feet high, 10 years old, 30*l*. ; ditto, ditto, in calf, 6½ feet high, 5 years old, 23*l*. ; male ditto, 5 feet high, 11½ years old, 14*l* ; female ditto, in calf, 5 feet high, 1½ years old, 14*l*. ; male dromedary, 7½ feet high, 5 years old, 30*l*. ; female ditto, 6½ feet high, 14 years old, 21*l*. ; male camel calf, born February 6, 1872, 9*l*. 10*s*. The sale produced nearly 3,000*l*.

The severe frost of March 21 has done an incalculable amount of damage to the fruit crop. Apples, pears, and cherries appear to have suffered most severely. It is a remarkable circumstance that although the majority of the flowers have been killed in the bud, the central part being turned perfectly black, yet the flowers expand and present externally a perfectly uninjured appearance. The *Garden* estimates the damage done to the potato crop in Jersey by the spring frosts at many thousands of pounds.

Although the Brighton Aquarium has been formally opened to the public, it is still in a very unfinished condition, owing to a disagreement between the proprietors and the contractor, and the severe illness of the engineer. At the time of its inauguration by Prince Arthur, on Easter Monday, but one tank was supplied with fish. When completed, the collection will by no means be confined to marine animals, a portion of the building being devoted to fresh-water tanks.

The Senate of the University of Bombay has recently been engaged in investigating a scandal in connection with the Matriculation Examination, the passages set in Latin being taken entirely from books which one of the examiners had during the previous term made the special subject in his own class.

The *Engineer* states that the oxyhydric light has not proved a success in Paris, and that it has been discontinued in the public lamps on the Boulevard des Italiens.

Messrs. Waterlow and Sons, of 66, London Wall, announce that the invention of an entirely new method of producing a number of copies of the same manuscript without the use of ink, by a very simple process which they term printing by electricity, and to which we have already referred, may now be seen in operation on their premises.

We have received a circular from the Secretary of the Philadelphia Philosophical Association, containing a statement of its leading principles, and an outline of the method pursued in carrying them out. These principles are stated to be:—1, That force is persistent; 2, That all knowledge is relative; 3, That philosophy is the synthesis of the doctrines and methods of science; 4, The critical attitude of philosophy is not destructive, but constructive; not sceptical, but dogmatic; not negative, but positive. The Association appears to have been established in November 1871, and proposes to select a number of suitable papers, or parts of papers, for publication in a Quarterly Journal.

A correspondent at Brighton describes a solar phenomenon visible on the afternoon of April 8, at 5.35 P.M. The sun being just within the upper part of a mass of light clouds, through which it shone with a white glare, there appeared a distinct belt of colours, in order and apparent width exactly like those of an ordinary rainbow, but apparently flattened above. Half a minute afterwards a second belt appeared, equally bright, and with no interval between the two. At the same time a fainter belt appeared to the right, but not forming a part of the same circle as the others. The three were visible together, but did not last above a minute. After the unusual appearance was first noticed, the sky above was tolerably clear, with a few light upper clouds. After the prismatic lines had faded, there was that diffused white glare round the sun which is commonly said to betoken windy weather.

There is now every prospect that the getting of coal by machinery will be more generally adopted than hitherto. At present it has only been adopted at a few places, but a new machine, patented by Messrs. Gillott and Copley, has just been tested at the Wharncliffe Silkstone Colliery, near Barnsley, in the presence of a number of mining engineers from various parts of the kingdom, and with most satisfactory results. In 136 minutes a bank of coal, 58 yards long and four feet eight inches thick, was cut to a depth of three feet one inch. The quantity of coal so cut would be about 80 tons in the time stated. In connection with coal machinery a hydraulic coal breaker, patented by Mr. Clubb, of London, has just been very successfully tested at the Oaks Colliery, Barnsley.

An Indian paper prints the following interesting account of a fight between a hyæna and a man:—"About five days ago a party of six natives coming towards Deyra through the Mohun Pass, were attacked by a hyæna; it made straight at one of them, and flew at his throat. The poor devil stretched out his hands to keep off his assailant, on which the hyæna bit them severely; his companions, instead of coming to his aid, took refuge in some adjoining trees; the man, finding himself thus deserted and his hands in a mutilated state, pluckily turned on his enemy, and seized his nose with his teeth, roaring out in the best way he could for assistance. By this means he secured the animal, and his companions, taking courage, came down from their secure position, and belaboured the beast to death with sticks. I saw the unfortunate man at the dispensary, where he had gone to have his wounds dressed, and was shown the head of his enemy having his teeth marks on the nose. I believe this is almost an unprecedented instance in the annals of natural history, as a hyæna is well known as a most cowardly brute, never venturing to attack man, but preying chiefly on dogs, carrion, and young children."

ANNUAL ADDRESS TO THE GEOLOGICAL SOCIETY OF LONDON, FEB. 16, 1872

By J. Prestwich, F.R.S., President

(*Continued from page* 433.)

Our Coal-measures and our Coal-supply

While the presence of water has determined the early settlement of population, the existence of coal has given rise to exceptional local growths of that population, quite irrespective of the original cause of settlement. The existence of coal has created new wants, developed vast energies, enormous resources, and has established great industries dependent upon it for their maintenance and prosperity. Natural causes, unceasing and ever renewing in their action, maintain our supplies of water in a condition of constant and unfailing operation. They are physical and geological agents, equally in force in the past as in the future of the earth's history. Not so with coal, which is a store of the past, and of which we can look for no renewal. Our Coal Measures, great as they are, have defined limits, whereas our wants seem to have no bounds. With the increasing magnitude of the latter our fears of the extent of the former have increased, and have given rise to much speculation and much discussion. At first the estimates of the duration of our coal-fields were little more than guesses; but the subject has of late years been treated in a systematic manner, and in all its various bearings, in the able works of Hull, Jevons, and Warington Smyth. To obtain more precise data on these important questions, the Royal Commission of 1866 was appointed, with your President-elect, the Duke of Argyll, at its head. On the practical and economical questions different members of the Commission and separate committees have made valuable reports. I wish on this occasion merely to direct your attention to some of the more special geological bearings of the questions discussed in one of the committees, of which the lamented Sir Roderick Murchison was chairman, the object being "to inquire into the probability of finding coal under the Permian, New Red Sandstone, and other superincumbent strata."

On the evidence laid before this committee regarding England north of the Bristol coal-field, Prof. Ramsay was deputed to report, while the south of England was relegated to myself. The one district embraces all the unproved older secondary tracts between the different well-known coal-fields of the central and northern portions of England. The other district takes in that occupied by the later Secondary and the Tertiary strata, already the subject of a valuable paper in our Journal for 1856, by Mr. Godwin-Austen. The excellent mapping of our coal-districts by the Geological Survey, and their accurate sections through the several coal-fields, furnished Prof. Ramsay with data which have enabled him to prolong these sections across the intervening tracts with a degree of certainty which gives them very great value. He has presented us with 32 such sections, which, when published, will, with the text already before the public, show how great has been the task, and how successfully it has been accomplished.

The area of the exposed coal-measures of England may be estimated at about 2,840 square miles. To these Mr. Hull had added 932 square miles of coal-measures overspread by newer formations. The investigations of Prof. Ramsay lead him now to conclude that this latter total of unproved coal-measures may be increased to 2,988, to which may be added 153 miles of the Bristol coal-field, making a total of 3,141 square miles of Coal-measures under the Permian, New Red, and Triassic strata of central and northern England, or of 301 square miles more than the area of all our exposed coal-fields. This branch of the inquiry embraces curious questions of variations in the mass of the coal-measures, in the thickness of the strata, and in the number and persistence of the coal-seams. The extent and magnitude of the faults bounding so many of our coal-fields, is also a point of great difficulty, especially when it is complicated by denudations of pre-Permian and of pre-Triassic age; and in this intricate inquiry it must be borne in mind that it is only a question of superposition and faulting, but one also of removal and replacement, involving a number of important geological problems. Especially is it necessary to distinguish steep old-surface and submarine valley denudations from faults.

The other inquiry relating to the possible range of the coal-measures under the Jurassic, Cretaceous, and Tertiary strata of the south-east of England, involves questions of a much more hypothetical character, and can, in the absence of positive information, only be treated on purely abstract geological reasoning. Still it is one essentially within the range of inquiry, and the collateral geological data we possess are sufficient to guide and direct those inquiries. There are two primary points to be determined:—First, how much of the area under investigation remained dry land during the Carboniferous period, and was therefore never covered by Coal-strata. Secondly, supposing the Coal-strata to have spread over a portion of that area, how much of them escaped subsequent denudation? With regard to the first question it is comparatively easy, where the Palæozoic rocks now form the surface, to determine the antiquity of that surface, but where the old rocks are covered by great masses of other strata it becomes very difficult to determine the original conditions. Mr. Godwin-Austen has ingeniously sought to establish the position of the old coast-lines of the Carboniferous and other periods, the area of the old coal-growth, and the great features of the ancient physical geography of this period in Western Europe. I have given more especial attention to relations of the Secondary and Palæozoic formations to one another and to those points which depend upon physical conditions connected with the nature and age of old disturbances and denudations, the direction and position of the great anticlinal and synclinal lines, to the correlation of certain strata, and the dimensions of the overlying strata.

The great lines of disturbance traversing Central and North-eastern England are subsequent to the Carboniferous period, and the many detached coal-basins separated by the Penine chain and the Derbyshire hills, together with the Mountain Limestone forming those ranges, are held to be portions of one great Carboniferous formation, which, in its entirety, spread from the south of Scotland to central England, and, as we shall observe presently, probably still farther south. This great Carboniferous deposit was originally bounded on the north either by the uplands of the Scottish-border counties, or, possibly, by the Grampians; on the west by the high lands of Cumberland and Wales; while on the south we find no old exposed land-surfaces of older Palæozoic age until we reach Brittany and Central France. With respect to the deposits going on during the Carboniferous period in this area, Professor Phillips was the first to show that the lower Carboniferous series puts on, as it trends north from Derbyshire, more sedimentary conditions—that the Mountain Limestone there begins to show traces of the proximity to land, which increase rapidly in proceeding northwards,—beds of shale and sandstone and subordinate beds of coal gradually setting in in the limestone series, and increasing in importance as they approach the older border land. In the same way the approach to an old barrier-land on the south and west is supposed by Professor Ramsay to be indicated in the overlying Coal Measures by the increase in number and thickness of the beds of sandstone in the south of the Staffordshire and Shropshire coal-field, and Mr. Hull connects that old land with the Cambrian and Silurian rocks of Leicestershire.

If such were the case, the question arises, did this form a barrier which cut off the Carboniferous deposits from extending over the south of England, or was it only a partial barrier which in no way prevented the extension southward of the Carboniferous rocks?

It has been supposed that during the Carboniferous period a spur from the Silurian district of Wales extended eastward from Herefordshire into central England, dividing the coal-fields of Shropshire and Staffordshire from those of Gloucestershire; and that against this old Silurian tract the Coal Measures of South Staffordshire die out. If carried farther eastward it would limit the southern prolongation of the Coal Measures of Leicestershire, and then pass under the Oolites of Northamptonshire and the Cretaceous series of Norfolk; and so great an expansion has been given it southward, that it would equally exclude the Coal Measures from the area of the south east of England. We have, however, no sufficient evidence of the continuous extension of these old rocks eastward of Staffordshire. Palæozoic rocks show, it is true, in Leicestershire; but there the Coal Measures wrap round them, and the older rocks seem merely to be an island in their midst. At those spots in the southern counties where they have been proved underground, I imagine they were raised by disturbances of a later date than the Coal Measures, and did not form part of the land surface of the Carboniferous period. As just mentioned, the older Carboniferous rocks show deeper-sea conditions as they trend from north to south, and the same deep-sea conditions existing in Derbyshire are found to prevail in the Mountain Limestone of Belgium, while, at the same time, similar slight indications of distant land, in the presence of intercalated shales and imperfect coal, reappear and increase westward in their range into the district of the Boulonnais, in France. There is nothing to show but that the spur of old land stretching eastward from Herefordshire was merely a promontory ending in Warwickshire, and round which the Carboniferous sea passed and extended southward uninterruptedly to Belgium and the north of France, and westward to Somersetshire and South of Wales, spreading over all this wide area first the Mountain Limestone and then, in due order, the Coal Measures. Of the existence of these formations over the south-western and south-eastern portions of this area we have proof in Wales, Somersetshire, and Belgium. The intermediate area is covered by Jurassic, Cretaceous, and Tertiary formations, which hide from us the older rocks whose position it is our object to determine.

Just as with the disturbance which at a later period caused the Mountain Limestone of the Penine chain to break through the great expanse of Coal Measures originally spread over the central and northern counties of England, and brought up to the surface the disturbed and disjointed coal-strata, of which, after subsequent denudation, we have the isolated portions remaining in the existing coal-fields, so was the area of Southern England traversed by the earlier axis of Palæozoic rocks of the Ardennes and Mendips, bringing up the Coal Measures in like manner along their northern flanks in separate basins and troughs, some of which are uncovered by newer strata, while other basins not exposed on the surface may still possibly exist beneath the newer strata of the south-east of England. They have in fact been proved to exist under considerable portions of those newer strata of north-western France and of Belgium, and under some of the older Secondary strata in the south-west of England.

The probable continuation of this great range of Palæozoic rocks from the Rhine to South Wales, passing underground in the south of England, was shadowed out by Buckland and Conybeare in 1826, commented on by Dufresnoy and Elie de Beaumont in 1841, by M. Meugy in 1851, and more fully investigated and discussed by Mr. Godwin-Austen in 1855. These views having been controverted, the subject was fully discussed by the Commission, and again in the separate report drawn up by myself.

All geologists are agreed upon the age of this great east-and-west axis of disturbance. It took place after deposition of the Coal Measures, and before the deposition of the Permian strata. Its effects, all through its range, are singularly alike. It was not so much a great mountain-elevation, as a crumpling up and contortion of the strata for a breadth of many miles, and along a length of above eight hundred miles. The Silurian and Devonian rocks are thrown up by it into a number of narrow anticlinals, and the flanking coal-strata are tilted, turned back on themselves, squeezed and contorted in the most remarkable manner,—the same type of disturbance being apparent whether in Westphalia, Belgium, France, Somerset, or Pembroke. These great flexures have also resulted in throwing the Coal Measures into deep narrow troughs, having a length of many miles and a width of but very few.

In France, these disturbed old strata are covered transgressively by Jurassic, Cretaceous, and Tertiary strata, and in Somerset by Permian, Liassic, and Jurassic strata; they sink beneath the Oolites at Frome, and reappear in Belgium from beneath the Cretaceous strata. What becomes of them in the intermediate area? It is not to be supposed that a line of disturbance of such great magnitude could have been intermittent. The coal-trough has, in fact, been followed from near Charleroi, where it passes under the Cretaceous and Tertiary strata, to Mons, Valenciennes, and Bethune, a distance of eighty-six miles. Along the whole of this line, the Chalk and overlying beds extend, with a thickness varying from 500 to 900 feet around Mons, decreasing to from 250 to 300 near Valenciennes, and increasing again towards Bethune. At Guines the Chalk was found to be 670 feet thick, and at Calais 762 feet. On the other side, the coal-trough of Somerset passes eastward under the older Secondary rocks, which in their turn pass under the Cretaceous and Tertiary strata of Wiltshire; but no attempt has been made to follow Coal Measures beyond a distance of six miles from their outcrop, where the overlying strata have been found to attain a thickness of about 450 feet.

The original supposition that the Secondary strata maintained, in the main, their regular sequence, and, to a certain extent, their thickness over large areas has long been proved to be erroneous;

but we were hardly prepared until lately to learn how rapid the variation in their thickness is. Mr. Hull has now shown that the Great and Inferior Oolites thin out from a thickness of 792 feet in Gloucestershire to 205 feet in Oxfordshire, and the Lias and Trias from 1090 feet to 400 (?) feet; while in like manner the Trias decreases from 5600 feet in Lancashire and Cheshire, to 2000 in Staffordshire, and 600 feet in Warwickshire. We also know that on the northern flank of the Mendips, the Trias, Lias, and Oolites tail off, although their dimensions in Gloucestershire are so considerable. It would appear that all the Secondary rocks, except those of the Cretaceous series, show a distinct thinning-out in their range southward, which is doubtless due to the existence of an old pre-Triassic land on the south—such as would have been formed by the prolongation of the Palæozoic rocks of the Ardennes and Mendips through the south of England. It has been urged, on the other hand, that this thinning-out is a proof of the existence of a still older land in that area; but as the argument is based on the evidence of rocks of post-Carboniferous age, it is clear that, whether the land were of Cambrian and Silurian, or of Devonian and Carboniferous age, the result, as affecting the Secondary rocks, would be the same.

This thinning-out of the Secondary strata has now been proved not to be merely hypothetical. At three points, on or near the presumed line of the old underground range, the Tertiary and Cretaceous strata have been traversed in well-sections, and Palæozoic rocks found to underlie them at once, without the intervention of any Triassic, Liassic, or Oolitic strata. Thus at London the presence of red and grey Sandstones, apparently of Palæozoic age, has been proved under the Chalk at a depth of 1,114 feet. Again, at Harwich and at Calais, strata of early Carboniferous age have been found also immediately under the Chalk, at depths respectively of 1026 and 1032 feet. There is therefore reason to believe that the underground ridge of the Mendips and the Ardennes passes in a line from Frome through North Wiltshire, Berkshire, Middlesex, North-east Kent, and between Calais and Boulogne, at a depth beneath the Secondary strata of not more than from 1000 to 1500 feet, while the coal-troughs, which may flank this range on the north would, judging from the analogy of the structure and relations of the same rocks at Mons and Valenciennes, be met with at depths very little, if at all, greater.

To the north of this area it is probable that the thickness of the overlying rocks is greater; but we have no means of knowing exactly. In Northamptonshire the Great and Inferior Oolites and the Lias have been found not to exceed together 880 feet, at which depth the New Red Sandstone was reached; but its thickness was not proved beyond 87 feet; while at Rugby, the Lias was found to be about 905 feet thick, below which 136 feet of beds of New Red Sandstone were passed through. Looking at the proved thinning out from north to south of the New Red and Permian strata, there is no reason to suppose that they would be found of any very great thickness in the southern counties. Even immediately to the south of the known coal-fields of the Midland counties, the trials for coal have not yet proved any very great thickness of these rocks. It would seem, in fact, that the extensive tracts of Chalk, Oolites, and Trias, forming the substrata of our Midland and Southern counties, constitute but a comparatively shallow crust filling up the plains and valleys of Palæozoic rocks, the great framework of which stretches apparently at but a moderate depth under our feet, and of which the highest ridges only, such as those of the Ardennes and Mendips, now rise above ground.

It is clear, therefore, that in any search for coal, the relation of the Secondary and the Palæozoic groups of rocks to one another being perfectly independent, the latter must be considered entirely on their own internal evidence, and apart from the bearing of the newer rocks covering them and forming the present surface, except possibly in a few cases where old lines of disturbance have proved points of least resistance, and yielded again, as suggested by Mr. Godwin-Austen, to later movements, which have equally affected the overlying formations.

It may be asked if any correlation can be established between the coal-measures of Bristol and South Wales and those of France and Belgium. So far as the identity of any particular bed of coal or of rock, it is impossible, and we should not expect it; for the variation in all the beds of any coal-basin is well known to be so great and rapid, that in the different parts of the same basin it is often difficult, and sometimes impossible, to establish any correlation; while in adjacent basins, such as those of Wales and Bristol, or of Hainaut and Liége, such attempts have, with few exceptions, hitherto utterly failed. There are, however, more general features which serve to show, at all events, some relationship. The great dividing mass of from 2,000 to 3,000 feet of rock called Pennant exists in both the Welsh and Bristol coal-field; and the total mass of coal-measures is not very different, it being 10,000 to 11,000 feet in the one, and from 8,000 to 9,000 in the other, and there being in Wales 76, and in Somerset 55 workable seams of coal. In the Hainaut (or Mons and Charleroi) basin, the Measures are 9,400 feet thick, with 110 seams of coal; in the Liége basin 7,600 feet, with 85 seams; and in Westphalia 7,200 feet, with 117 seams. On the other hand, none of our central or northern coal-basins, with the exception of the Lancashire field, exceed half this thickness, and more generally are nearer one fourth. Further, the marked difference which exists between the northern coals and those of Wales and Somerset, the preponderance of caking-coals in the north, and of anthracite, steam, and smiths' coal in the south, equally exists between our northern coals and those of Belgium, which latter show, on the other hand, close affinities with those of Wales and Bristol. I am informed by two experienced Belgian coal-mining engineers and good geologists, who have twice visited our coal-districts, that the only coals they found like those of Belgium were the coals of South Wales and Radstock—there was the same form of cleavage, the same character of measures, and the same fitness for like economical purposes. Organic remains help us but little, but too little is yet known of their relative distribution. The plants are, as usual, the same; so also are shells of the genus *Anthracosia*, and a number of small *Entomostraca*; while there is a scarcity of many of the marine forms which are more common in some of our central and northern fields. That, therefore, which best indicates the relation between the coal-fields of the south-west of England and those of the north of France and Belgium, is the similarity of mass and structure, uniformity of subjection to like physical causes, and identity of relation to the underlying older and to the overlying newer formations.

It was in the north that the conditions fitted for the formation of coal first set in. The common *Stigmaria ficoides* and various Coal Measure plants appear at the base of the Carboniferous or in the Tuedian series of Northumberland, which there overlies conformably the Upper Old Red Sandstone; and productive beds of coal exist low down in the Mountain-Limestone series. These disappear in proceeding southward, and the great productive coal-series becomes confined to beds overlying the Millstone Grit. If the coal-growth set in earlier in the north, it seems to have been prolonged farther south, under more favourable conditions, to a later period. What those conditions were—whether the proximity of a greater land-surface, of a longer and greater subsidence, with more numerous rests—we cannot yet pretend to say.

Of the prolongation of the axis of the Ardennes under the south of England there can be little doubt; nor can there be much doubt that the same great contortions of the strata, which in Belgium placed the crown of the anticlinal arch at a height of four or five miles above the level of the base of the accompanying synclinal trough, to the bottom of which the Coal Measures descend, and was the cause of similar folds in the Coal Measures of Somerset and Wales, were continued along the whole line of disturbance, and that the preservation of detached portions of the same great supplementary trough is to be looked for underground in the immediate area, just as it exists above ground in the proved area; for the minor subordinate barriers dividing the coal-basins can, I conceive, in no way permanently affect the great master disturbance, by which the presence of the Coal Measures is ruled. Whether, however, admitting that the Coal Measures were originally present, they have been removed by subsequent denudation is another question.

(To be continued.)

SCIENTIFIC SERIALS

Annalen der Chemie und Pharmacie, December 1871. A considerable part of this number is occupied by a valuable paper "On valeric acids from different sources," by Erlenmeyer and Hell. They prepared isobutyl iodic acid, and from this the corresponding iodide, which they treated with alcoholic potash to convert it into potassic valerate: the valeric acid from these reactions had no action on polarised light. They prepared valeric acid from valerian root, and this also had no rotating action on a polarised ray. A series of experiments was made on the valeric acids obtained from active and inactive amylic alcohols, and also on the acid obtained from leucin; this latter is found to rotate a ray of polarised light to the right, but not to so great an extent as the acid which is obtained from the left-handed amylic alcohol. The acids from isobutyl cyanide, from valerian root, and from inactive amylic alcohol, show very great similarity; whilst the acids from the active alcohol and from leucin agree in most of their properties. The valeric acid made from inactive amylic alcohol is almost certainly isopropacetic acid, and that from the active alcohol is probably methethacetic acid, although the authors consider that the latter acid might possibly be a molecular compound of two isomeric acids, such as isobutylformic and methethacetic acids. Besides this communication, there are several important physiologico-chemical papers, together with translations of two others from foreign periodicals.

THE *American Naturalist* for February commences with an exhaustive account of the Mountains of Colorado by Dr. J. W. Foster, read before the Chicago Academy of Sciences. Mr. E. L. Greene, in a short paper on the Irrigation and the Flora of the Plains, shows how a gradual alteration is going on in the character of the flora of those parts where a system of irrigation has been established, *Typha* and other marsh and water plants supplanting the original inhabitants of the drier plains. Mr. John G. Henderson, on the former range of the buffalo, brings forward evidence to show that the buffalo was at a not very remote period extremely abundant over almost the whole of the Northern United States, while he thinks that it is doomed in a short time to become extinct like the great Irish elk, the mastodon, and the dodo. The remainder of the number is occupied with reviews and short notes.

A CONSIDERABLE portion of the *Canadian Naturalist*, vol. vi., No. 2, is occupied with a report of the Edinburgh meeting of the British Association. Prof. Dawson continues his note on the Post-pliocene Geology of Canada. Prof. H. A. Nicholson (late of Edinburgh) contributes an article on the "Colonies" of M. Barrande, in which the best account we have yet seen is given of the celebrated theory of the French palæontologist. Dr. J. W. Anderson has a short article on the Whale of the St. Lawrence; Mr. S. W. Ford some notes on the Primordial Rocks in the vicinity of Troy, N.Y.; and Mr. E. S. Billings a paper on some new species of Palæozoic Fossils belonging to the classes Pteropoda and Brachiopoda.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 21.—"New Researches on the Phosphorus bases," by Dr. A. W. Hofmann, F.R.S. Ten years since the author presented to the Royal Society a series of papers on the remarkable group of phosphorus compounds first discovered by Thenard in 1847. These researches were devoted to the investigation of the tertiary and quartary derivatives of phosphoretted hydrogen, exclusively accessible by the methods then known. Since then numerous attempts have been made to prepare the primary and secondary phosphines, but with no result until the present time. The author wishing to obtain pure phosphoretted hydrogen for lecture experiments, was led to prepare it by the action of water or soda on the beautiful compound of phosphoretted hydrogen and hydriodic acid. The ease with which this body decomposed led the author to think that it might be made available for the production of the missing compounds. For this purpose it was necessary to liberate phosphoretted hydrogen in the presence of an alcohol iodide under pressure. This could be done by heating together the phosphonium iodide and alcohol iodide in presence of some substance capable of slowly decomposing the former body, such as zinc oxide. This process yields the alcoholic phosphines, easily giving rise to the formation exclusively of primary and secondary phosphines. A further simplification of the process was tried, namely, by utilising the hydriodic acid from the phosphonium iodide in the formation of the alcohol iodide to be acted on by phosphoretted hydrogen. This was accomplished by digesting the phosphonium iodide with the alcohol; by this method it was found that only the tertiary phosphines and the quartary phosphonium compounds already known were produced, but which were more easily and plentifully obtained by the new than by the old method. The reactions by which the various groups of phosphines are produced from phosphonium iodide are as follows, the reaction being assumed to take place in the methyl series:—

Primary Phosphines

$$2\,CH_3I + 2\,(H_3P.HI) + Zn\,O = 2\left[(CH_3)\,H_2P.HI\right] + Zn\,I_2 + H_2O$$

Secondary Phosphines

$$2\,CH_3I + H_3P.HI + Zn\,O = (CH_3)_2\,HP + Zn\,I_2 + H_2O$$

Tertiary Phosphines

$$3\,(CH_3.HO) + H_3P.HI = (CH_3)_3\,P.HI + 3\,H_2O$$

Quartary Phosphonium compounds

$$4\,(CH_3.HO) + H_3P.HI = (CH_3)_4\,PI + 4\,H_2O$$

The primary and secondary methylic derivatives of phosphoretted hydrogen are prepared by placing together in a sealed tube 2 molecules of methylic iodide, 2 molecules of phosphonium iodide, and 1 of zinc oxide. The mixture is heated to 100° for six or eight hours, when the reaction is complete; on cooling the tube contains a white crystalline solid, and also a considerable amount of compressed gas. The crude product of the reaction is first treated with water, which decomposes the salts of monomethylphosphine, liberating it as a gas, which is collected in concentrated hydriodic acid; and secondly with potash, which decomposes the salts of dimethylphosphine, and liberates the dimethylated phosphine as a liquid. The whole process must be conducted in an atmosphere of hydrogen, as the two bodies are powerfully acted on by the oxygen of the air.

Methylphosphine $CH_3\,H_2\,.\,P$, is a colourless and transparent gas of a most overwhelming odour, which, by cooling and by pressure, can be condensed to a colourless liquid floating on water. It boils at $-14°$ under a pressure of 0·7585 metre. At 0° it began to liquefy at $1\frac{3}{4}$ atmospheres pressure, and at $2\frac{1}{2}$ atmospheres it was entirely liquefied. At 10° liquefaction commenced at $2\frac{1}{2}$ atmospheres and was completed at 4 atmospheres pressure, and at 20° under a pressure of 4 and $4\frac{1}{2}$ atmospheres. The volume weight of the gas was determined by decomposing a known weight of the iodhydrate over mercury. Experiment gave the number 24·35, the theoretical value being 24. Methylphosphine is nearly insoluble in water free from air; if it contain air the gas disappears, owing to oxidation; it is rather soluble in alcohol, more especially at low temperatures; ether dissolves but little at ordinary temperatures, but at 0° one volume of ether dissolves in less than 70 volumes of methylphosphine. When gently heated in contact with air it takes fire, as it does also in pressure of chlorine or bromine. By its union with acids it forms a remarkable series of salts, distinguished by the remarkable property of being decomposed by water.

The chlorhydrate is obtained by mixing methylphosphine with gaseous hydrochloric acid, the gases at once condense to beautiful four-sided plates; the iodhydrate $CH_6\,PI$ is obtained by passing the gas into a concentrated solution of hydriodic acid; it can be crystallised in plates, which may be easily sublimed.

Dimethylphosphine $(CH_3)_2\,HP$, obtained as above, is a transparent colourless liquid which is lighter than water and insoluble in it; readily soluble in alcohol and ether. Its boiling point is 25°. In contact with the air it instantly takes fire, and burns with a powerfully luminous phosphorus flame. It unites easily with acids, all its salts being exceedingly soluble. The chlorhydrate furnishes with platinum perchloride a fine crystalline salt.

Methylphosphine passed into fuming nitric acid is absorbed and oxidised, with the formation of a new acid, small quantities of phosphoric acid being also produced. The excess of nitric acid is removed by evaporation in a water bath, and the phosphoric acid by boiling with lead oxide, which forms the lead salt of a new acid which is soluble in acetic acid, and lead phosphate which is insoluble. The lead salt is decomposed by sulphuretted hydrogen, and the acetic acid removed by evaporation, which leaves the new acid as a crystalline mass resembling spermaceti, melting at 105°. Its composition is found to be $CH_3\,H_2\,PO_3$, and may be called methylphosphinic acid. It forms two series of salts, in which H_1 and H_2 are replaced by metals. The primary silver salt crystallises in beautiful white needles which, in contact with water, are converted into the secondary salt. The lead and barium salts of this acid have also been obtained.

Methylphosphinic acid has the same composition as methylphosphorous acid, but they are two absolutely different bodies. Methylphosphorous acid is an uncrystallisable ephemeral compound, decomposing at a gentle heat into phosphorous acid and methyl alcohol, whilst methylphosphinic acid may be distilled without decomposition.

Dimethylphosphinic acid, obtained in a nearly similar manner to the above, is a white crystalline solid, melting at 76°, and may be distiiled without change. Its composition is $(CH_3)_2$ H PO_2; the silver, lead, and barium salts have been obtained, but do not crystallise so well as the salts of the last-named acid.

Phosphoretted hydrogen, on treatment with nitric acid, fixes four atoms of oxygen yielding tribasic orthophosphoric acid, whilst trimethylphosphine fixes only one atom of oxygen yielding trimethylphosphine oxide, a body which is no longer capable of forming saline compounds. We have thus a series of three bodies which may be looked on as derived from orthophosphoric acid by the replacement of hydroxyl by methyl:—$(HO)_3$ PO, orthophosphoric acid; (CH_3) $(HO)_2$. PO, methylphosphinic acid; (CH_3) HO_2 . PO, dimethylphosphinic acid; $(CH_3)_3$ PO, trimethylphosphine oxide. An analogous series of bodies is known in the arsenic group.

The primary and secondary ethylic derivatives of phosphoretted hydrogen are prepared in a precisely similar manner to the methyl compounds, except that the tubes containing ethyl iodide, phosphonium iodide, and zinc oxide, must be heated to 140°—150° for six hours.

Ethylphosphine $(C_2 H_5)$ H_2 . P, is a transparent mobile liquid, powerfully refractive, lighter than water, and insoluble in it. It boils at 25°, and has an overwhelming odour. Its vapour bleaches like chlorine; caoutchouc placed in it becomes transparent, and loses its elasticity. It is inflamed by chlorine, bromine, and nitric acid. It is isomeric with dimethylphosphine previously described. With acids it forms salts which are crystalline and are decomposed by water.

Diethylphosphine is a colourless transparent liquid, insoluble in water and lighter than it. Its odour is very penetrating and persistent. It boils at 85°, and forms corresponding salts to dimethylphosphine which are not decomposed by water.

The primary and secondary ethyl phosphines, on oxidation by nitric acid, yield precisely corresponding products to the methylphosphines already described.

By the action of benzyl chloride, phosphonium iodide, and zinc oxide at 160°, the author has succeeded in obtaining the benzyl phosphine in a similar manner as before described.

Benzyl phosphine, $(C_7 H_7)$ H_2 P, is a liquid boiling at 180°, attracting oxygen with great avidity; it forms a beautifully crystalline iodhydrate, and also other salts corresponding to those obtained from methylphosphine.

Dibenzylphosphine, $(C_7 H_7)_2$ H P, is a crystalline body melting at 205°, which does not oxidise in the air, nor does it form salts with acids like the corresponding dimethyl and diethylphosphines.

The author has likewise obtained the phosphorus compounds in the propyl, butyl, and amyl series, the details of which will be shortly communicated.

Geological Society, March 20—"On the Wealden as a Fluvio-lacustrine Formation, and on the relation of the so-called 'Punfield Formation' to the Wealden and Neocomian." By C. J. A. Meyer. In this paper the author questioned the correctness of assigning the Wealden beds of the south-east of England to the delta of a single river; he considered it more probable that they are a fluvio-lacustrine rather than a fluvio-marine deposit, and attributed their accumulation to the combined action of several rivers flowing into a wide but shallow lake or inland sea. The evidence adduced in favour of these views was mainly as follows:—The quiet deposition of most of the sedimentary strata, the almost total absence of shingle, the prevalence of such species of mollusca as delight in nearly quiet waters, the comparative absence of broken shells such as usually abound in tidal rivers, and the total absence of drift-wood perforated by mollusca in either the Purbeck or Wealden strata. This Wealden lacustrine area the author supposed to have originated in the slow and comparatively local subsidence of a portion of a land-surface just previously elevated. He considered that during the Purbeck and later portion of the Wealden era the waters of such lacustrine area had no direct communication with the ocean. The changes from freshwater to purely marine conditions, which are twice apparent in the Purbeck beds, and the final change from Wealden to Necomian conditions at the close of the Wealden, were attributed to the sudden intrusion of oceanic waters into an area below sea-level. The author then pointed to the traces o terrestrial vegetation in the Lower Greensand as evidence of the continuance of river-action after the close of the Wealden period. In the concluding portion of his paper the author referred to the relation of the Punfield beds of Mr. Judd to the Neocomian and Wealden strata of the south-east of England. From the sequence of the strata, no less than on palæontological evidence, he considered the whole of the so-called "Punfield formation" of the Isle of Purbeck to be referable to the Lower Greensand of the Atherfield section. Mr. Godwin-Austen did not agree with Mr. Judd in calling the bed at Punfield the Punfield "formation;" it was merely a bed intercalated between beds of a different character below and above. Prof. Ramsay thought that the Purbeck strata were connected with lagoons in contiguity with a large river rather than with inland lakes. These, from time to time, owing to the oscillations of level, were covered with marine deposits. He did not think that the absence of gravelly deposits offered any serious difficulty in regarding the Wealden strata as marine. It seemed to him more probable, however, that the sands and clays of the Wealden were due to some ancient rivers on a large scale, and deposited at their mouths, though in some spots the beds were subject to the action of fresh and salt water alternately. He regarded the Neocomian as, to some extent, a marine representative of the Wealden, though of later date. Mr. Etheridge recalled the fact that Mr. Judd had correlated the Punfield fossils with those of the north of Spain, twenty-two species found in each being absolutely identical. He argued from this that the extent of the beds may have been far larger than might be supposed. Prof. T. Rupert Jones remarked that the Purbeck-Wealden lake theory had not only been intimated by several previous writers, but had been illustrated by maps by Messrs. Godwin-Austen and Searles Wood, Jun. The Chairman, alluding to the pseudomorphs of salt mentioned by the author, stated that they had been somewhat compressed, and thus modified in form. They had also been found in other beds in the Wealden. He commented on the extension of the Wealden strata even to the south of Moscow. In the Oxford and Buckinghamshire area there was evidence of great denudation of the Purbeck and Wealden beds prior to the deposit of the Neocomian, so that great changes would seem to have taken place, giving rise to a great amount of denudation towards the close of the Wealden period. Mr. Meyer agreed with Mr. Godwin-Austen and other speakers as to there having been a certain amount of denudation of the Upper Wealden beds prior to the deposit of others upon them, but this he regarded as merely local. It was the absence of shingle rather than of gravel to which he had alluded in his paper. He thought that there was a distinction to be traced between the Neocomian of the north of England and that of the south, and that the middle beds of one were equivalent to the lower beds of the other.

Zoological Society, March 19.—John Gould, F.R.S., vice-president, in the chair. The secretary read a report on the additions that had been made to the Society's collection during the month of February 1872, amongst which were specimens of the Sumatran rhinoceros, two-wattled cassowary, and other rare animals.—Mr. R. B. Sharpe exhibited some specimens of blue rock thrushes from Europe and Eastern Asia. After tracing the different plumages through which *Petrocossyphus cyanus* passed, he came to the conclusion that the Eastern blue rock thrush, *P. solitarius*, eventually becomes entirely blue like the European species, and that the birds usually called *P. manillensis* and *P. affinis* are merely stages of plumage of *P. solitarius*.—Major Godwin-Austen exhibited a skin of *Ceriornis blythii*, which had been obtained by Mr. Roberts, of the Indian Topographical Survey, in the Naga Hills.—Mr. Sclater exhibited and made remarks upon a specimen of the American yellow-billed cuckoo (*Coccyzus americanus*) which had been obtained near Buenos Ayres.—A communication was read from Prof. A. Macalister, of the University of Dublin, containing notes on a specimen of the broad-headed wombat (*Phascolomys latifrons*).—A communication was read from Mr. W. E. Brooks, of Etawah, India, containing remarks on the Imperial eagles of India, *Aquila crassipes* and *A. bifasciata*.—A paper by Dr. J. E. Gray, F.R.S., was read, containing observations on the genus *Chelymys*, and its allies, from Australia.—Sir Victor Brooke, Bart., read a paper on *Hydropotes inermis* and its cranial characters, as compared with those of *Moschus moschiferus* and other Cervine forms.—Major Godwin-Austen read descriptions of new land and freshwater shells which he had recently met with in the Khási, North Cachar and Nágá Hills of N.E. Bengal.—Mr. Howard Saunders read some notes on the introduction of *Anser albatus* of Cassin into the European avifauna, and exhibited two examples of that species lately shot near Wexford in Ireland.

Chemical Society, March 21.—Dr. Odling, F.R.S., vice-president, in the chair.—The chairman announced that the

Faraday lecture would be delivered by Prof. Cannissaro on Thursday, May 30.—A communication from M. Maumerie, of Paris, was then read by the secretary, in which he denied the existence of the hyponitrous acid recently discovered by Dr. Divers (Proceedings of the Royal Society, xix. 425), on purely theoretical grounds, unsupported by any experiments or analyses. Dr. Divers, who was present, explained M. Maumerie's theory. —An interesting discussion took place on theoretical points connected with some remarks made by Dr. Debus, in which he stated that no organic compound existed, in which the number of atoms of hydroxyl, HO, was greater than the number of carbon atoms.

March 30.—The President delivered the annual address, in the course of which he commented upon the comparatively small number of papers communicated to the Society. The apathy and lethargy from which chemical science in this country is at present suffering, he believed to be due to a great extent to our system of university education. After the officers and council for the ensuing year had been elected, and the usual votes of thanks proposed, the meeting was adjourned.

April 4.—Dr. Frankland, F.R.S., president, in the chair.—Dr. Schorlemmer, F.R.S., delivered a very interesting lecture "On the Chemistry of the Hydro-carbons," defining organic chemistry as the chemistry of hydro-carbons and their derivatives. The characteristic properties of the paraffin, olefine, and acetyline series, and their relations one to another, were discussed, also those of the great aromatic group, the speaker pointing out the great assistance derived from the atomic theory in determining both the constitution of isomeric compounds, and also the relations existing between the various members of the aromatic series.

Entomological Society, March 18, 1872.—Mr. F. Smith, vice-president, in the chair. R. Meldola was elected a member. —Mr. Higgins exhibited beautiful species of *Cetoniidæ* from Java, including some apparently new.—Mr. Bond exhibited a dimorphic example of *Acronycta leporina*, one side of which was coloured and marked as in typical examples, the other side as in the variety *bradyporina*, the two forms having at one time been considered distinct species.—Mr. Smith said that the remarks on Siberian insects at the last meeting had induced him to make a minute examination of specimens of the hornet (*Vespa crabro*) from Europe, Siberia, and North America, and he found that individuals from these districts presented no appreciable variation. The Asiatic *V. orientalis* was, however, quite distinct.—Mr. Müller read notes on *Serropalpus striatus*, which beetle he considered to be a wood-feeder, and especially attached to fir-wood; hence its occurrence in a hose-warehouse at Leicester could only be considered as accidental.—The Secretary read a long account of the ravages of locusts in South Australia in December 1871, as related in the *South Australian Register* for January 2, 1872. The insects were described as coming in swarms that darkened the air, eating every morsel of vegetation. It was found that those individuals that had partaken of leaves of the castor-oil plant were immediately killed thereby, and larkspur seemed also inimical to them.—Mr. Horne related his experiences of locusts in India. The castor-oil plant had certainly no injurious effects upon Indian species, though they were affected by the leaves of the tamarind-tree.

April 1.—Professor Westwood, president, in the chair.—Dr. A. S. Packard, Jun., of Salem, United States, was present as a visitor.—Professor Westwood exhibited a large spongy oak-gall found on the ground under an oak, which Mr. Müller considered to be the work of *Cynips radicis*. He further alluded to the differences existing in the genital apparatus of various species of the genus *Cynips*, and exhibited drawings illustrating his remarks. Also, he alluded to the different structure existing in the antennæ of various species of fleas, and maintained that these insects formed a distinct order, *Aphaniptera*. Finally he produced drawings, sent to him by a correspondent, of a minute Hymenopterous insect of the genus *Coccophagus*, parasitic upon the common *Coccus* of the orange; and he remarked that now is the best time for finding the males of *Coccus*, and especially of that infesting espalier pear trees.—Mr. Müller read notes on the larvæ of *Anaspis maculata*, which he had obtained from the excrescences or outgrowths on a trunk of birch. —Mr. Butler read additional remarks on the *Pericopides*, especially referring to species recently described by Dr. Boisduval.—Mr. McLachlan read a paper on the external sexual apparatus of the males of the genus *Acentropus*, and exhibited drawings of this apparatus made from microscopic examination of individuals from various parts of England and the Continent. Although there were minute differences, he could find nothing to indicate, on these characters alone, that more than one species existed.

Geologists' Association, March 1.—Prof. Morris, vice-president, in the chair. "On the Geology of Hampstead, Middlesex," by Mr. Caleb Evans. The author described the deposits which had been exposed from time to time during the last few years in and near Hampstead. The principal excavations noticed were the several drainage works near Child's Hill, on Hampstead Heath, and in Frognal Lane, and the tunnel on the Midland Railway under Haverstock Hill. It appeared from these sections that the Lower Bagshot Sand which caps the hill passes downwards into a dark sandy clay about 50 feet thick abounding with fossils, especially *Voluta nodosa* and *Pectunculus decussatus*. The *Pectunculus* bed passes down into the London Clay of ordinary character, which forms the lower part of Hampstead Hill. The author noticed the great changes in physical geography which must have taken place during the time that intervened between the deposition of the Woolwich series and that of the Lower Bagshot Sand. He considered that remains of the glacial deposits probably exist on the north side of the hill. The position of these deposits on an eroded surface of the London Clay showed the large amount of denudation that had taken place prior to the Glacial epoch. The author, in conclusion, directed attention to the existing valleys around and to the north of Hampstead, which he considered had been formed by means of the springs issuing from the water-bearing Eocene sand and the glacial gravels. Mr. A. Bell thought the leaf-beds of the Middle Eocene indicated fresh-water conditions. Mr. H. Woodward considered the presence of *Zanthopsis* in these beds evidence of Marine or Estuarine origin. He pointed out the great value of the maps and sections exhibited by Mr. Evans. Prof. Morris spoke of the foreign equivalents of the London Eocenes, during the deposition of which great changes of level took place. Though there are no traces of the Woolwich beds in the Belgian area, these deposits are represented near Epernay in France, while the London Clay forms a considerable area in Belgium. The patches of London clay on Salisbury Plain indicate the extension of the Lower Eocene sea over that area, and Bracklesham species are found at Chertsey. With respect to the Glacial deposits the Professor considered their importance in Middlesex very considerable, and thought it not improbable that the towns of Barnet, Hendon, and Finchley owed their origin to the presence of these deposits. The physical features of the country north of Hampstead are different from those south of that place, and this difference is due to the glacial deposits. Though the valleys of the district have been formed as we now see them by the rivers, their formation commenced during the rise of the land from the sea.—"On a recently exposed section at Battersea," by Mr. John A. Coombs. This was a brief description of a section exposed at the works of the London Gas Company now in progress near Battersea. The Thames Valley gravels are cut through and several feet of the London Clay is exposed. The gravels, which show much false bedding, yield mammalian remains, but the *Cyrena fluminalis* has not been found. Several species of Mollusca have been found in the clay, but the most abundant fossil is a species of Echinodermata, the *Pentacrinus sub-basaltiformis*. Mr. Hudleston noticed that at the Law Courts site in the Strand the gravels were much more ferruginous than those at Battersea, and the clay immediately underlying the gravels was altered in colour and character to a much greater depth at the former than at the latter locality.—Mr. A. Bell thought the *Cyrena fluminalis* would never be found in these beds at Battersea, as it belongs he considered to beds of a different age.

Victoria Institute, February 4.—Mr. C. Brooke, F.R.S., in the chair. "Prehistoric Monotheism, considered in relation to Man as an Aboriginal Savage," being a reply to certain statements made by Sir John Lubbock in his work on Primitive Man. The paper combated the statements made by that writer, that man in his original state was a savage and without religious knowledge, from the results of investigations into the present condition of savages, from the earliest authentic records to be found in various countries, and from the writings of Aristotle, Herodotus, and others. Mr. Prichard stated that so far as his inquiries had extended, they confirmed the view taken in the paper, and the Rev. G. Percy Badger, who gave similar testimony, in alluding to an apology made by the author of the paper, for not quoting Scripture as an authority, stated that it was perhaps judicious, as it enabled him to refute Sir John Lubbock's statement on his own ground, though it seemed strange that the latter should prefer the authority of such as Herodotus, whose writings betrayed ignorance on several points, for instance, where he refuses to believe in snow existing in a land so hot that the inhabitants were black,—to the writings of Moses, which, as writings even, were of a much higher order.

Paris

Academy of Sciences, March 25.—M. Serret presented a note by M. A. Mannheim, containing geometrical investigations upon the contact of the third order of two surfaces.—General Morin read a memoir on the simultaneous employment of electrical induction apparatus and apparatus of deformation of solids, for the study of the laws of the movement of projectiles, and of the variation of pressures in the bore of guns.—A memoir was also read by M. V. Albenque, relating to the theory of rifled artillery, and treating of the effects of the resistance of the air upon a solid of revolution animated by a simultaneous movement of rotation.—M. Phillips presented a note by M. Bresse on the determination of brachistochrones.—A note from Father Secchi was read, giving an account of injury done at Alatri by lightning striking a lightning-conductor, and passing from it to large water pipes.—A note by M. G. Volpicelli, on the use of the proof-plane in the investigation of electrical conditions, was read.—M. Wurtz communicated a note by M. G. Salet, on the absorption spectrum of the vapour of sulphur, in which the author claimed to be the first describer of this spectrum, which was noticed by M. Gernez at the meeting of the Academy on March 18. He stated that the most perceptible dark lines coincide with the luminous bands in the spectrum of sulphur in the flame of hydrogen.—A letter from M. Donati to M. Delaunay, on auroras and their cosmical origin, was read. The author considers these phenomena to depend on an exchange of electricity between the sun and the planets.—M. Delaunay announced the discovery at Bilk by M. Luther on the night of March 15-16 of a new planet of the eleventh magnitude. The discoverer proposes to name it *Peitho*.—The miserable dispute as to the priority of the invention of the preservation of wines by heat was continued by MM. Vergnette-Lamotte, Pasteur, and Thenard.—M. Wurtz presented a note on a new class of compounds of dulcite with the hydracids by M. Bouchardat. These compounds are crystallisable, but rather unstable.—M. Fremy presented a note by M. Prinvault on the action of bromine upon protochloride of phosphorus, by which he has obtained some curious and unexpected compounds.—A note by M. E. Jannetaz on a new type of idiocyclophanous crystals was presented by M. Delafosse.—M. C. Robin communicated a note by M. V. Feltz on the properties of the bones, in which the author stated that matters injected into the spongy tissues of the bones in the living subject are absorbed as rapidly as if they were introduced directly into the veins, from which he inferred that this spongy tissue is in direct connection with the veins, and must be regarded as forming a system of sinuses.—M. Champouillon, in a note presented by M. Larrey, stated that putrefaction is much more rapid in the dead bodies of alcoholised subjects than in those of comparatively sober individuals.—M. C. Robin presented a note by MM. Legros and Onimus containing an account of some experiments on spontaneous generation, in which the authors describe the production of fermentation within an egg penetrated with sugar by endosmotic action, and afterwards immersed in a fermenting solution of sugar.—A note by M. A. Gris containing general considerations upon the structure of the bark in the Ericineæ was communicated by M. Brongniart.—M. A. Baudrimont read a paper on the existence of mineral matter in plants, which contains some interesting results as to the amount of solid matter in fleshy plants.—M. Roulin presented a note by M. Triana on the *Gonolobus cundurango*, a South American plant, reputed to furnish a remedy for cancer.—A paper by M. L. Vaillant on the fossil Crocodiles of Saint-Gérand-le-Puy was communicated by M. Milne-Edwards. The author described three species, two belonging to the subgenus *Diplocynodon* (*D. Ratelli* Pomel, and *D. gracilis* n. sp.), and a true Crocodile allied to the African species (*Croc. æduinus* n. sp.)

BOOKS RECEIVED

English.—On Bone Setting: W. P. Hood (Macmillan and Co.)—The Natural History of the Year: B. B. Woodward (S. W. Partridge).—The Journal of Mental Science, No. 4 (Churchill).

DIARY

THURSDAY, April 11.

Royal Society, at 8.30.—Researches on Solar Physics—Part III.: W. De La Rue, F.R.S., B. Stewart, F.R.S., and B. Loewy.—The Action of Oxygen on Copper Nitrate in a State of Tension: Dr. Gladstone, F.R.S., and A. Tribe.

Society of Antiquaries, at 8.30.—On some of the Stone Remains of Brittany: Sir H. E. L. Dryden, Bart.

Mathematical Society, at 8.—On the Mechanical Description of certain Sextic Curves: Prof. Cayley, F.R.S.

Royal Institution, at 3.—Heat and Light: Dr. Tyndall.

London Institution, at 7.30.—On the Distribution of Coal in the British Islands, and its probable duration: R. Etheridge, F.R.S.

FRIDAY, April 12.

Astronomical Society, at 8.

Royal Institution, at 9.—Rousseau's Influence on European Thought: J. Morley.

Quekett Microscopical Club, at 8.

SATURDAY, April 13.

Royal Institution, at 3.—The Star-Depths: R. A. Proctor.

Government School of Mines, at 8.—On Geology: Dr. Cobbold.

SUNDAY, April 14.

Sunday Lecture Society, at 4.—On Æther: the Evidence for its Existence, and the Phenomena it explains: Prof. W. K. Clifford.

MONDAY, April 15.

Victoria Institute, at 8.—On the Rationality of the Lower Animals: Rev. J. G. Wood.

TUESDAY, April 16.

Royal Institution, at 3.—On Statistics, Social Science, and Political Economy: Dr. Guy.

Zoological Society, at 9.—On the Mechanism of the Gizzard of Birds: A. H. Garrod.—On a supposed New Monkey from the Sunderbunds to the East of Calcutta: Dr. John Anderson.

Statistical Society, at 7.45.

WEDNESDAY, April 17.

Society of Arts, at 8.—On the Great Central Gas Company's Works: A. Angus Croll.

Royal Society of Literature, at 8.30.—On the Trade of Phœnicia with Ophir, Tarshish, and Britain: W. S. W. Vaux.

Meteorological Society, at 7.

THURSDAY, April 18.

Royal Society, at 8.30.

Royal Institution, at 3.—On Heat and Light: Prof. Tyndall, F.R.S.

Society of Antiquaries, at 8.30.

Linnean Society, at 8.—On *Begoniella*, a new genus of Begoniaceæ: Prof. Oliver.—On three new genera of Malayan plants: Prof. Oliver.—On *Camellia scottiana* and *Ternstrœmia coriacea*: Prof. Dyer.

Chemical Society, at 8.—Notes from the Laboratory of the Andersonian University; On a Compound of Sodium and Glycerine; and On Benzylisocyanate and Isocyanurate: E. A. Letts.

CONTENTS

NOTICE

We beg leave to state that we decline to return rejected communications, and to this rule we can make no exception. Communications respecting Subscriptions or Advertisements must be addressed to the Publishers, NOT *to the Editor.*

THURSDAY, APRIL 18, 1872

THE SECOND REPORT OF THE ROYAL COMMISSION ON SCIENTIFIC INSTRUCTION AND THE ADVANCEMENT OF SCIENCE

THE Commission has just issued its Second Report, dealing with the scientific side of the instruction given in Training Colleges and National Schools, and in the Science Classes at present conducted by the Science and Art Department. The report is so long that it is impossible to give it *in extenso*. It can, however, be easily obtained, and it should be read by all interested in one of the most important questions for England just now. Both with reference to elementary education and the Science Classes the present condition of things is fully stated, and this condition is criticised where, in the opinion of the Commissioners, criticism is necessary. The provisions of the new code we may refer to as a case in point.

The Report concludes with the following recommendations :—

SCIENTIFIC INSTRUCTION IN TRAINING COLLEGES AND ELEMENTARY DAY SCHOOLS

I. We recommend, as regards the elder children in the elementary schools, that the teaching of such rudiments of physical science as we have previously indicated should receive more substantial encouragement than is given in the regulations of the new code.

II. We recommend, as regards the younger children that Her Majesty's Inspectors should be directed to satisfy themselves that such elementary lessons are given as would prepare these children for the more advanced instruction which will follow.

III. We recommend that the mode of instruction of pupil teachers, the conditions of admission to training colleges, the duration of the course of study in them, and the syllabus of subjects taught, should be so modified as to provide for the instruction of students in the elements of physical science.

SCIENTIFIC INSTRUCTION IN SCIENCE CLASSES UNDER THE SCIENCE AND ART DEPARTMENT

IV. We recommend that the instruction in Elementary Science Classes under the Science and Art Department, be so arranged as to work in complete harmony with the general system of public elementary education, but, at the same time, we consider it important that the Education Department and the Department charged with Instruction in Science shall continue to be co-ordinate.

V. We recommend that a more efficient inspection of Elementary Science Classes be organised, and that the inspectors should advise the local committees and report on :—

(*a*) The apparatus of instruction.
(*b*) The state of the discipline and methods.
(*c*) The general efficiency of the arrangements.

VI. We recommend that teachers who have already qualified by passing the May examination in either of the advanced classes shall continue to be recognised as qualified to conduct Elementary Science Classes, with the title of Elementary Science Teacher, and to earn the grants awarded by the Department of Science and Art on the results of the examination of their scholars ; but that this qualification and title shall in future only be attainable by passing in the first of the advanced classes.

VII. We recommend that should such arrangements as are hereinafter set forth for conducting the practical instruction of teachers, and for providing for them practical examination at several centres, be adopted, all elementary science teachers shall, after such practical instruction, be admissible to a further examination, which, in all suitable subjects, shall be practical. We recommend that success in this examination shall entitle a teacher to a certificate of Second Grade Science Master.

VIII. We recommend that, as an inducement to teachers to prepare for and pass this further examination, payment for results in the case of a Second Grade Science Master be made at a somewhat higher rate than in that of the Elementary Science Teacher.

IX. We recommend that an examination, both by papers and by practical tests, in any group of allied subjects defined by the Department which the candidate may select, shall be open to all those teachers who have passed in the advanced classes, or who have been otherwise admitted as Science Teachers; and that success in this examination shall entitle the candidate to receive a certificate of First Grade Science Master in that group.

X. We recommend that a greater capitation grant be payable in respect of the scholars of a First Grade Science Master teaching in any group of allied subjects with or without assistance, than in respect of the scholars of a Second Grade Science Master, provided that the Inspector report that the apparatus is sufficient, and that practical instruction has been given in each suitable subject.

XI. We recommend that, with a view of maintaining uniformity of standard in these examinations, they shall be conducted at the several local centres by the staff of Examiners acting under the Science and Art Department.

XII. We recommend that the more systematic training of the teachers of science referred to, be provided for—

(*a*) By the adoption of special arrangements for this purpose in the Science School which has been referred to in our First Report ; and by the recognition by the Department of similar arrangements for the instruction of this class of students in any University or College, and in Science Schools as hereinafter described.

(*b*) By giving to the students of Training Colleges the opportunity of remaining a third year, during which scientific instruction may either form a principal part of the curriculum of such Colleges, or be accessible in some adjacent College or School of Science approved as efficient for that purpose.

XIII. We recommend that the Science and Art Department be at liberty to dispense with the preceding examinations, and to accord the privilege of First and Second Grade Science Masters in consideration of University Examinations in Science, or of a satisfactory course of study in colleges in which science is taught, as well as in other cases of obvious scientific qualification.

XIV. We recommend that in schools recognised as Science Schools, as hereinafter set forth, facilities for the employment of assistant teachers be afforded as an experiment on a limited scale, some addition being made to the emoluments of the teacher in consideration of the instruction afforded; provided the Department be satisfied, on the report of an inspector, that such assistant teacher has received practical instruction in subjects in which it is prescribed, and that he has been actively engaged in teaching.

To encourage the more advanced scholars to become assistant teachers under first grade masters in such schools, a small stipend, rising in successive years, might be granted on condition that a like sum was raised locally, subject to such conditions as the Department might deem expedient. The proportion of assistant teachers should not exceed one for every fifteen successful scholars in any science school, and no scholar should be recognised as an assistant teacher until he has passed in the first division of the elementary class in the May examination.

XV. We recommend that, with a view of training First Grade Science Teachers, exhibitions of sufficient value and in sufficient numbers be offered to elementary science teachers and to assistant teachers who have served three years, and passed in the first division of the advanced class in the May examinations; and that such exhibitions should be tenable in any University, College, or Science School recognised in Recommendation XII.

XVI. We recommend that the grants made by the Science and Art Department for buildings be extended, under sufficient guarantees, so as to embrace institutions for scientific instruction, although they may not be built under the Public Libraries Act, or be in connection with a School of Art.

XVII. We recommend that grants similar to those now made for apparatus be given for laboratory and museum fittings under proper guarantees.

XVIII. We recommend that whenever the arrangements for scientific teaching in any institution shall have attained a considerable degree of completeness and efficiency, such institution be recognised as a Science School, and be so organised as to become the centre of a group of Elementary Science Classes; and to provide the assistance of First Grade Science Masters, the loan of apparatus and specimens, and the means of instruction in the laboratories and museums to the more advanced students of the group.

XIX. We recommend that assistance be given for the formation and maintenance of such Science Schools by special grants, the conditions of which shall be determined by regulations to be framed by the Science and Art Department.

XX. We recommend that when laboratories are attached to second grade grammar schools in the schemes issued by the Endowed Schools Commissioners, the trustees of such schools be encouraged and enabled to invite the formation of elementary science classes to be taught therein.

AMERICAN WAR-OFFICE REPORTS

Report on Barracks and Hospitals, with Descriptions of Military Posts. War Department, Surgeon-General's Office, Washington, December 5, 1870; pp. 525.

Approved Plans and Specifications for Post Hospitals. Surgeon-General's Office, Washington, July 27, 1871; pp. 14.

THESE two documents are intended to fulfil for the United States army the same purpose as the Reports of the Royal Commissions of 1857 and 1863 on the sanitary state of the British and Indian armies, and the Report of the Barrack and Hospital Improvement Commission were intended to fulfil for Her Majesty's troops serving at home and abroad.

The first document contains an excellent general report by Assistant-Surgeon Billings, followed by a digest of reports from the posts of the United States army scattered all over their territory. These reports, besides dealing with the general sanitary condition and diseases of troops, are full of interesting general information regarding local topography, surface geology, hydrography, meteorology, and natural history, having reference to 151 points and districts of the country extending from the lakes to the mouths of the Mississippi, and from the east of Maine to the far west of Oregon and California. The reports are illustrated by topographical plans, showing the outlines of the more important localities, and also by plans and details of barrack and hospital arrangements.

The most common diseases to which troops are liable are malarial fevers, catarrhal affections, diarrhœa, and dysentery. Malaria appears to exist more or less in all the military "departments," while in Arizona it produces results of more importance to efficiency than this pest does in India.

The purely medical details are of more interest to professional readers, but it is evident that most of the officers who have supplied the local information have been fully alive to the importance of scientific questions generally, and hence these reports may be advantageously consulted by persons interested in the physical geography of this division of the American continent. In Mr. Billing's report the general results of these district inquiries are given, and the principles of local improvements are discussed. Those referring to post hospitals are embodied in the "approved plans and specifications," which show simple, efficient, and economical, methods of erecting hospitals of the denomination required. The plans are generally the same as those proposed by the Army Sanitary Committee in this country, but they contain one or two of those ingenious adaptations of principles for which our transatlantic cousins are famous. One of the great difficulties in American climates is to keep apartments sufficiently heated and yet to preserve the air from contamination.

In improved barracks and hospitals at home this has been effected by a peculiar form of fire grate, contrived by Captain Galton, which, while retaining the advantages of the open radiating fire, supplies the room with a large body of fresh air warmed to about 60° F., the chimney draught being used as a means of removing foul air from the room. A modification of this contrivance for burning wood is figured in the report on the Sanitary Improvement of Indian Stations, drawn up by the Army Sanitary Committee.

The American contrivance produces the same result in duplicate by one fire-place intended to be fixed in the centre of the ward. There are two open fires, one facing each way. The fresh air to be warmed is passed under the floor to the space between the backs of the two fires, and is thence admitted in the room. The arrangement is simple, and ought to be effective.

It is evident from the reports generally, that much improvement is required in existing barracks and hospitals in the United States, and that overcrowding, defective ventilation, and other disease causes, still exist there as they used to do with us. It is a great step towards improvement to have an honest statement of defects. We must congratulate the Surgeon-General's department on the production of these reports, and express our hope that the executive authorities may make as good a use of them as the reporters have done of their opportunities of acquiring information regarding the stations.

OUR BOOK SHELF

Scottish Meteorology, from 1856 to 1871. Being a continued monthly and annual representation of the more important mean results for the whole country, deduced at the Royal Observatory, Edinburgh, from the schedules of observation by the Observers of the Scottish Meteorological Society, for the purposes of the Registrar-General of Births, Deaths, and Marriages in Scotland. (Edinburgh Astronomical Observations, vol. xiii.)

IN the Introduction to this work, the Astronomer Royal for Scotland tells us that it was undertaken at the request of Government, the application being to deduce from the observations taken under the auspices of the Scottish Meteorological Society, "certain monthly and general results for each and all of the stations, results supposed to be important for medical climatology and its influence on population and national welfare." The ways of statisticians are mysterious; it is difficult to understand what advantage either to medical climatology, to agriculture, or, broadly, to national welfare, is to be derived from the means here printed, means not only of barometric pressure, but of temperature, rain, and hours of sunshine, including as they do the observations at some 55 stations scattered over all Scotland, from the Shetland Islands to Dumfries, from Aberdeen to Islay—places with peculiarities of climate as distinct as could anywhere be found within anything like equal distances. We suppose, however, that there is a use for them; and, that being the case, they could not be put before the reader with more beautiful simplicity and clearness than we here find; but as we reflect on the enormous amount of skilled labour which the reductions must have cost, we cannot help regretting that meteorology can derive no advantage from it. With this report for "the purposes of the Registrar-General" is sewn up one of a very different and highly interesting character, the detailed observations of the storm which passed over the North of Scotland on October 3, 1860. These observations describe very fully a storm of extraordinary intensity, bursting almost with the suddenness of a meteor on the northern coasts; with such suddenness, indeed, that at several of the stations where the barometer was registered only at intervals of twelve hours, the whole fall, amounting, it would seem, to about 1·8 in., and the subsequent rise, passed quite unnoticed. One point which has been often, though not very closely, observed in tropical cyclones, comes out most distinctly—the remarkable rise of the barometer beyond the limits of the storm, before and after it, in Scotland, in England, and France, about the time of its meridian passage. The lowest barometric reading anywhere observed was 28·5; this leads us to remark that, in tabulating the conclusions, the force of the wind has been unintentionally much exaggerated, owing, it appears to us, to a confusion common to all non-nautical minds between the land scale, which numbers from 0 to 6, and the Beaufort, or sea scale, which numbers from 0 to 12; for the one is not to be converted into the other by simply doubling; and the shore 6, far from being the equivalent of the Beaufort 12, is more nearly represented by 9 to 10, or at the outside by 10, which may be considered as corresponding to a velocity of about 80 miles an hour. In the discussion of the observations of this storm, many points of great interest arise: amongst others, the relationship between wind and pressure, the howling of the wind, and the ascensional motion of the air near the centre. The curt, able, cautious, and suggestive treatment of these is such as we might expect from the high standing of Prof. Smyth, and leaves little to be wished for except time for meditation. J. K. L.

The Deviation of the Compass in Iron Ships considered practically for Sea Use, and for the Board of Trade Examinations. By W. H. Rosser. (London: Longmans.)

IN this small treatise the Deviation of the Compass in iron ships is professedly dealt with as a matter of observation, and distinct generally from magnetic science and the mathematical investigations based thereon. Mr. Rosser's long experience both as a "teacher" of officers in the mercantile marine, and an adjuster of compasses for the ships of that service, has enabled him to produce a work calculated to give those with whom he has been so long associated good practical information. The articles on the compass equipment of ships and the heeling error are judiciously given, and rightly occupy a prominent place. Whilst, however, thus commending the work, it must be regarded as meeting only a present and passing want; for from the absence of many theoretical, but not necessarily abstruse, details, the subject even as presented from a practical point of view cannot be considered as grasped with that entirety which certainly belongs to it. Those theoretical deductions which have been practically confirmed are further requisite in the advanced examinations instituted by the Board of Trade, and are, moreover, to be found in the several manuals compiled under the Admiralty and Board of Trade auspices.

LETTERS TO THE EDITOR

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

Error in Humboldt's Cosmos

I BEG to call the attention of geometers to what appears to me to be an inaccuracy in a work, which is, perhaps, the last which one would suspect to be capable of error—the "Cosmos" of Humboldt.

In vol. i. p. 293, he says, "I have found by a laborious investigation, which, from its nature, can only give a maximum limit, that the centre of gravity of the land at present above the level of the ocean is, in Europe, 630; in N. America, 702; in Asia, 1,062; and in S. America, 1,080 French feet (or 671, 748, 1,132, and 1,151 English feet) above the level of the sea." Sir John Herschel in his "Physical Geography" (Encylop. Britt.) quotes these numbers of Humboldt as giving the height of the centre of gravity of these continents; and adds, "whence it follows, that the mean elevation of their *surfaces* (the doubles of these) are respectively 1,342, 1,496, 2,264, and 2,302." Herschel's conclusion is, of course, just, if Humboldt meant what he seems to say. But at the risk of being thought most presumptuous, I submit that Humboldt meant the height of the centre of gravity of the *surface* of the land; in other words, the mean height of the land; and by thus misleading Sir John Herschel he has by a *coup de plume* doubled all our continents.

1. In the first volume of his "Asie Centrale," p. 165, writing on "la hauteur moyenne des continents," Humboldt says, "en cherchant à évaluer l'élévation moyenne de la hauteur des divers continents, c'est à dire la position du centre de gravité du volume des terres élevées audessus du niveau actuel des eaux. . . ." It thus appears that Humboldt used the words "hauteur moyenne," and "hauteur du centre de gravité du volume," as equivalent expressions, which I submit they are not. Had he said "centre de gravité de la surface," he would have been right, for that height is the mean height.

2. But though inaccurate in expression, Humboldt could never be other than right in principle. Fortunately in the "Asie Centrale" he describes with much detail the process by which he arrives at his so-called "centre de gravité du volume"; and the process legitimately leads to the mean height. He divides the continent into great areas, which I shall call a_1, a_2, a_3 . . . finds the mean height of each b_1, b_2, b_3, . . . by taking the mean of several; and then the mean height is

$$\frac{a_1 b_1 + a_2 b_2 + a_3 b_3 \ . \ . \ .}{a_1 + a_2 + a_3 \ . \ . \ .}$$

A range of mountains he regards as a triangular prism; and to find its mass he multiplies the area of the base by half the mean height, and then computes how much this would raise the whole country if spread over it; and the former number thus increased is, as is plain, the mean height.

3. Arago, in his "Astronomie Populaire," cites the labours of Humboldt with approbation, goes over all the details, adds a vast number more, and deduces numbers approximately the same for the mean height of land. Arago, it is to be observed, invariably uses the phrase "hauteur moyenne." Like Humboldt, he considers that the mean of all the continents lies between 900 and 1,000 feet.

4. Humboldt (Note 360, "Cosmos") apologises for differing from La Place, who, he says, made the mean height of continents *more than three times* too great. Now La Place's estimate was 3,078 feet.

I conclude, therefore, with the greatest deference, that Humboldt's "centre de gravité du volume" is an inaccurate expression, and that he meant "centre de gravité de la surface," or mean height. If this be so, Sir John Herschel has been led into the error of doubling our continents, which he estimates at a mean elevation of 1,800 feet.

It is a matter of some importance; for Sir Charles Lyell computes that the continent of N. America will be utterly washed away into the ocean by the ordinary processes of degradation in four and a half millions of years. If, indeed, this period is to be doubled, we can take a more cheerful view of the future of that continent. But I greatly fear with Sir Charles that it is limited to four and a half millions of years, unless some upheaval of the land shall protect its short span of existence.

JOHN CARRICK MOORE

113, Eaton Square, March 28

Conscious Mimicry

THE idea of mimicry in animals being induced through the sense of sight appears to me to deserve more than a passing notice of M. G. Pouchet's statement that changes of colour in prawns, to accommodate them to the colour of surrounding objects, are prevented by removing the eyes of the prawns.

In 1869 I expressed my belief that such was the case, and endeavoured to embrace a large class of phenomena, as well as mimicry, within the same instrumentality. I allude to the asserted cases of the human or other fœtus being affected through the sense of sight of the mother. But on ascertaining the views of many able medical men, as well as of scientific naturalists, I found opinions so divided on the matter that I did not think it desirable to pursue further inquiries, nor publish my memoranda made at the time. I could not bring myself to see that natural selection alone could produce mimicry. If it were of rare occurrence it would be called a remarkable coincidence, and might reasonably be due to selection, but what is really very general becomes a law, and must be traced to some far more "regular" influence than natural selection.

In basing the idea of mimicry in general upon the supposed act of the fœtus being susceptible through the mother's sense of sight, one is aware of the critical nature of the ground adopted, and that possibly nine-tenths of the cases recorded must be put aside as worthless; but I have strong reasons for believing the one-tenth at least to have been true.

On the other hand, the experiments of Mr. Leslie on the caterpillars of *Pontia Rapæ*, which when enclosed, some in black and others in white boxes, produced chrysalises respectively modified to suit the colour of the box (*Sc. Gossip*, 1867, p. 261), appear to support my view, as also do those of Mr. Robert Holland (*Ib.* p. 279), in which the cocoons of the Emperor moth spun in white paper were white, while those on soil or in dead grass were brown.

G. HENSLOW

The Adamites

MR. C. STANILAND WAKE objects to my remarks on his paper on the "Adamites," which paper he protests is "written at least in a truly scientific spirit." This, I venture to say, is just Mr. Wake's error. He does not seem to be aware that comparative philology has a scientific method, and that words have to be compared by sound and structure according to fixed and even strict principles. Mr. Wake comes upon a Sanscrit word *pita*, father, and finds in it a primitive root *ta*, which he compares with another syllable *ta* got by cutting in two in the same way an Arabic verb, *'ata*. Had he looked into the structure of Sanscrit, he would have found that *pita* is the nominative case, and precisely the one that does not show the real crude-form of the word, which is *pitar*, the *tar* being a suffix. If it is lawful to compare languages by cutting words up anyhow and finding resemblances among the bits, of course connections may be found between any languages whatsoever. In the same easy way Mr. Wake finds a relation in Polynesian mythology between a divine being called *Taata* (by the way, he should have taken the name in one of its fuller forms, such as *Tamata* or *Tangata*), and another divine being called *Tiki*. But these are two different gods with different attributes, why should their names be altered to make them into one?

Mr. Wake thinks it nonsense for me to have set up an imaginary derivation for *Paddy* and *Taffy*, as commemorating the same ancestor *Ad* or *Ta*, from whom he traces *Akkad* and *Taata*. But of all ways of testing methods, one of the most useful is to try whether they can be made to prove transparent nonsense. If they can, it is evident that the method wants correction. As for my communication to you being anonymous, it was so for much the same reason that Mr. Wake's name was not mentioned in it, viz., that it is best to keep the personal element in the background in such matters, and the paper itself is the thing to be judged by.

M. A. I.

IF your correspondent, "M. A. I.," instead of endeavouring to negative the conclusions of Mr. Wake's paper "by such nonsense as the reference to Paddy and Taffy," as the author of the paper justly observes, had brought forward the word *Adam* itself, and shown that, by dividing it into *Ad* and *am*, and prefixing its consonant in each case, we obtain *Dad* and *Mam*, *father* and *mother*, he might have been held to have been critical, as well as satirical.

I believe, however, that Mr. Wake is wholly wrong in his conclusions, simply because his premisses are wholly wrong.

The word *Adam* has nothing of the meaning of *father* in it. The *Ad*, which Mr. Wake has so ingeniously made so much of, should for his argument be the Hebrew *Ab*, Arabic *Aba*, *a father*. To suppose that the word *Adam* has anything of the meaning of *father* in it shows a complete disregard of its root-meaning. In Hebrew the verb *adam* means *he was red* or *brown*, and the substantive *Adam* means *a red* or *a brown man*. The word *Edom* is from the same root, and means the *Red land*, probably because Red Sandstone constitutes its principal geological formation, and even *adamah*, the *ground*, is so called because of its *reddish* or *dark brown* colour. The Scripture narrative of the origin of man is that the Creator formed "the *Adam* (or man) of the dust of the *adamah* (or ground)."

If Mr. Wake's object had been to show that the *Adamites* were derived from the earth or *earth*-born, he would have found little difficulty both by internal and external evidence; he might have instanced the autochthones of the Greeks, the homines (*humus*, the ground), of the Latins, the yellow-earth men of the Chinese, and the red-clay men of the North American Indians.

April 15

B. G. JENKINS

On the Colour of a Hydrogen Flame

Accepting, for the time being, the experiments of Mr. Barrett as sufficient proof that a pure hydrogen flame does not exhibit a blue colour, my "elaborate theory" must, I suppose, seek refuge under the actinic power of the electric light.

Mr. Murphy refers this actinism to the fact "that the electric light is *bluer* than solar light," the blue rays of the sun's light having been abstracted by absorption. This is a bare fact, and deals solely with the relative proportions of the different coloured rays which reach us from the two sources—it conveys no clue to the reason *why* the blue rays have an entity in the first instance.

I would not have it understood that I consider *all* the high refrangible rays to be due to secondary waves; but I think it possible that some, at least, of those emitted from sources of a very high temperature may owe their existence to this cause. Considering for the moment the electric light, we have a centre of the most intense commotion sending off waves in all directions—a condition necessary, and at the same time eminently favourable, for the production of secondary waves.

With respect to Mr. Barrett's experiments, I intend to repeat them as soon as I can command the time. The absence of the higher refrangible rays in a hydrogen flame does not, however, affect the mechanical possibility of the existence of secondary waves; although it would be reasonable to expect their presence in a pure oxy-hydrogen flame, the amplitude of the disturbed particles being necessarily very great. A. G. Meeze

Hartley Institution, Southampton, April 15.

Another Aurora

A magnificent aurora, scarcely inferior to that of February 4, was observed here on the evening of the 10th inst., between $8^h 30^m$ and $9^h 30^m$.

The display was at its greatest beauty about $9^h 0^m$, when the creamy-white streamers attained an altitude of at least 60° above the N. horizon, and formed a fine contrast with a pale rose-pink background. The streamers appeared to proceed from behind a dense mass of stratus cloud which, although a moderate breeze was blowing from the S.W., remained almost stationary and unaltered during the display. The N. horizon was lighted up with a glow as intense as the early twilight on an evening in June.

With a small direct-vision spectroscope by Browning, I could see the line in the green near F, but no others. It was remarkably bright and sharply defined.

Bedford, April 12 Thos. Gwyn E. Elger

Brilliant Meteor

Yesterday afternoon, whilst standing on the lawn of the Observatory with my back to the sun, which was brightly shining, I saw a splendid meteor fall in the south-east. The sky at the time was of an intense blue and cloudless, with the exception of a few cirri in the north and north-west, and the meteor as seen against it presented the appearance of polished silver. The flight of the meteor was almost vertical at an altitude of about 30°, its extent was about 10°, and the trail which seemed to hang in the air and fade away like the trail of a rocket, was at the instant of explosion probably 3° in length. There was no report accompanying its disruption, or it would certainly have been heard, the neighbourhood being very still at the time.

Immediately on its disappearance I looked at my watch, it was $4^h 36^s$ P.M. G.M.T.

Had the fall occurred after dark I have no doubt but that the meteor would have exhibited a magnificent spectacle, for its brilliancy far exceeded that of the moon as seen by daylight.

During the aurora on the evening of the 10th I observed at 9.16 P.M. a peculiar well-defined patch or short band of bright red light, the position of which, as seen from here, was N.N.E. altitude 40° to 45°. Perhaps other observers may have noticed it, and their observations will give data which may serve to assist in determining the true height of the auroral discharge.

The magnetic disturbance on the 10th commenced abruptly at 2 P.M., and was greatest during the hours of daylight, so it is extremely probable, the sky being but partially clouded, that if the aurora was visible before night, some observers may have seen it. I cannot say I have ever seen it myself in the daytime, although I have repeatedly seen cirrus clouds assuming a form very similar to auroral streamers. However, on looking at the magnets and finding them undisturbed at the time, I have concluded that no aurora was taking place.

Kew Observatory, April 13 G. Mathus Whipple

Tide Gauges

The subject of the tides is now one in which much interest is taken by the committee of the British Association, and it would be a great boon to many who are in a position to give attention to it, if some of your readers would supply a description of a self-registering gauge for recording the heights, which should do its work effectively and not very expensively. Many plans are suggested; the difficulty is to know which is the best.

Vicarage, Fleetwood, April 11 James Pearson

NOTES ON THE RAINFALL OF 1871

The following are a few particulars of the rainfall of the past year, deduced from daily observations with Glaisher's (Hall's improved) rain gauge* at Fulwell,† near Twickenham, Middlesex, the place of observation being in lat. 51° 26′ 0″ N. long. 0° 20′ 53″ W.

The orifice, or receiving surface of the gauge, which is placed horizontally, is 8·00 inches in diameter (50·26 in area), the height of the same above the ground being one foot, and, as determined by spirit levelling from Ordnance B.M., 47 feet above mean sea-level.

The results of the observations have been calculated in the imperial system, and metric equivalents are placed in brackets, the use of which (brackets), for the sake of distinction, has been avoided in all other formulæ; they have, in each instance, been calculated to two or three places of decimals, but are here given, so far as is practicable, in whole numbers; the nearest integer, in each instance, having been taken; *they have further been calculated upon the hypothesis that the rainfall was equally distributed.*

In the following table:—

α = depth of rainfall in inches } Total fall
β = depth in centimetres } per month.
γ = number of gallons } Equivalents
δ = number of hectolitres } per acre.

	α	β	γ	δ
January	2·03	5·156	45,675	2,074
February	1·00	2·540	22,500	1,022
March	1·08	2·743	24,300	1,103
April	3·52	8·941	79,200	3,596
May	0·62	1·575	13,950	633
June	3·21	8·153	72,225	3,279
July	3·00	7·620	67,500	3,065
August	0·93	2·362	20,925	950
September	4·20	10·668	94,500	4,291
October	1·10	2·794	24,750	1,124
November	0·54	1·372	12,150	552
December	1·19	3·023	26,775	1,216

The total depth during the year was 22·42 in., or 56·947 centimetres.

The rainfall on a square mile during the year was $22,500 \times 640 \times 22·42 = 322,848,000$ gallons $(\div 22·024 = 14,658,918$ hectolitres), or $640 \times 4840 \times 9 \times 22·42 \div 12 = 52,086,144$ cubic feet $(\div 35·31658 = 1,474,835$ cubic metres).

A cubic inch of distilled water at a temperature of 62° Fahr. (16·66 C.) is a standard of weight; this quantity has been determined to weigh 252·458 grains, of which 437·5 make one ounce Av.;‡ therefore, a cubic foot weighs

* Vide *Scientific Opinion*, Vol iii., pp. 429, 440 (May 18, 1870).
† Although the observations refer especially to this locality, they will probably be scarcely the less interesting.
‡ *Practical Meteorology*, by John Drew, Ph.D., sec. 127, p. 190.

$\frac{252\cdot458 \times 1728}{437\cdot5} = 997\cdot137$ oz. Av.; hence we may assume that the entire weight of water which fell on one square mile was $\frac{52,086,144 \times 997\cdot137}{35,840} = 1,449,136$ tons, ($\div \cdot984 = 1,472,699$ milliers). Some idea of this enormous quantity will be afforded by the following illustrations.

The Thames at London Bridge is, at low water, nearly 700 feet wide,* and from 12 to 13 (say 12·5) feet deep. We will, for the sake of argument, assume the sectional area throughout to be $700 \times 12\cdot5 = 8,750$ square feet. The amount of rainfall on a square mile was equivalent to a volume of water corresponding in sectional area to the Thames at London Bridge, and extending $\frac{52,086,144}{8750 \times 5280} =$ 1·127 miles in length; in other words, it would extend from London Bridge, past Cannon Street (Railway), Southwark and Blackfriars (Railway and Road) Bridges, to about Somerset House, or nearly to Waterloo Bridge.

The same quantity of water would equal the contents of a river or canal having an uniform width of 20 feet, and depth of 5 feet—the sectional area being 100 feet—extending nearly 99 miles, or 159 kilometres in length.

The cubic contents of a sphere are $\frac{2}{3}$ of that of a cylinder of the same diameter and altitude. But the altitude being equal to the diameter, and $\frac{2}{3}$ of ·7854 being ·5236, the contents may be expressed as I have arranged it in the following formula. Calling $\triangle$ the diameter, and x the cubic contents required, we have

$$\triangle^3 \times \cdot5236 = x,$$

or the reverse, calling C the cubic contents and x the diameter required.

$$\sqrt[3]{\frac{C}{\cdot5236}} = x.$$

By these formulæ I have determined that the rainfall on a square mile—under the conditions mentioned in paragraph 3—was equivalent to a globe of water 463 ft. in diameter (approximate), a height exceeding that of the top of the cross surmounting the dome above the pavement of the churchyard of St. Paul's Cathedral (370 ft.†) by 93 ft.

The same quantity of water was equivalent to the following:—

A circular column of water 144 ft. in diameter (corresponding to that of the dome of St. Paul's Cathedral—interior surface‡), rising to a height of 3,198 ft.; in other words, it would be upwards of 8½ times the height of the cross before-mentioned.

Or, with regard to specific gravity:—

A circular column of lead (cast)§ of the same diameter (144 ft.—a cubic foot being taken as 710 lbs., or 11,360 oz.) containing 4,571,921 cubic ft., and rising to a height of 278 ft.

A circular column of granite (Aberdeen) of the same diameter, a cubic foot being taken as 2,690 oz.,‖ containing 19,307,443 cubic feet, and rising to a height of 1,184 ft.

But perhaps the most remarkable illustration will be afforded by comparing the weight of this quantity of water to a corresponding weight contained in, say, a number of railway coal waggons. Railway coal waggons are constructed to carry, on an average, from eight to ten tons. Let us assume it as the former of the two, and the average length of a number of waggons as 16 feet each from buffer to buffer. It would require no less than 181,142 such waggons to carry a corresponding weight of coal (or 3,623 heavy trains of fifty waggons each) which would, when close coupled, *i.e.*, buffer to buffer, extend over a distance of nearly 549 miles (883 kilometres) represented very nearly by the distance from London (Euston Station) to Aberdeen *viâ* Rugby, Stafford, Crewe, Carlisle, Glasgow, and Perth (London and North Western and Caledonian Railways). An express train, travelling at an uniform speed of sixty miles per hour, would take upwards of nine hours to run this distance, in other words, to pass this number of waggons; or, if I may indulge in another illustration, this number of waggons would, if travelling at an uniform rate of twenty-five miles per hour—which is about the average rate of goods trains—be nearly twenty-two hours in passing any given point, such, for instance, as a station. (Aberdeen is upwards of 130 miles N.N.E. of Edinburgh by the Caledonian Railway—Eastern route from London.)

Such a means of illustration as the one I have here set forth may not be considered in all respects strictly scientific; it has nevertheless this advantage, it enables us to comprehend something of the truth and magnitude of the subject—although dealing with hypotheses—where mere abstract figures would fail to produce anything like a similar result.

JOHN JAMES HALL

* I quote this from a paper "On the Rainfall of Devonshire," by W. Pengelly, Esq., F.R.S., *Scientific Opinion*, Vol. i. p. 137. (From the Transactions of the Devonshire Association for the Advancement of Science, 1868.) The depth is confirmed in the *Encyclopædia Brittanica*, Vol. xxi p. 163.

† *Encyclopædia Brittanica*, vol. xiii. p. 670.

‡ From the Cathedral authorities.

§ "Sprague's Pocket Tables (Architects and Surveyors)," p. 9.

‖ Ibid.

ON CERTAIN PHENOMENA ASSOCIATED WITH A HYDROGEN FLAME

PHENOMENA of much interest and possibly of future usefulness are associated with the combustion of ordinary hydrogen.

I. To study these phenomena free from disturbing causes three things should be attended to, although the effects to be described can be obtained without any special precaution.

(*a*) The gas must be stored and purified in the ordinary way, namely, by passing into a gas-holder through a solution of potash, and then through a solution of perchloride of mercury or nitrate of silver.

(*b*) From the holder the gas must be led through red or black india-rubber tubing to a platinum, or better, a steatite jet.

(*c*) And then the gas should be burnt in a perfectly dark room, and amid calm and dustless air.

II. In this way the flame gives a faint reddish brown colour, invisible in bright daylight. Issuing from a narrow jet in a dark room, a stream of luminosity, more than six times the length of the flame, is seen to stretch upward from the burning hydrogen. This weird appearance is probably caused by the swifter flow of the particles of gas in the centre of the tube. The central particles as they shoot upward are protected awhile by their neighbours; metaphorically, they are hindered from entering the fiery ordeal which dooms them finally to a watery grave. Dr. Tyndall has shown that the radiation from burning hydogen is hugely ultra-red, and moreover, that it has not the quality of the radiation from an elementary body like hydrogen, but practically is found to be the radiation from molecules of incandescent steam. So that, except at its base, a hydrogen flame is a hollow stream of glowing water raised to a prodigious heat.

III. Bringing the flame into contact with solid bodies, in many cases *phosphorescent* effects are produced. Thus allowing the flame to play for a moment on sand paper and then promptly extinguishing the gas, a vivid green, phosphorescence remains for some seconds. The appearance is a beautiful one, as a luminous and perfect section of the hollow flame is depicted. Similar phosphorescence is produced by the flame on white writing paper, or on marble, or chalk, or granite, or gypsum, &c. But no such effect is produced by coal gas, or olefiant or marsh gas. It is evidently a question of temperature, as oxygen given through coal gas shows the phosphorescence well.

IV. Far exceeding in generality the effect just noticed is a really magnificent *blue image of the flame* that starts up on almost every substance with which the flame is brought into contact. I have already drawn attention to this effect in the Phil. Mag. for November 1865, and in my letter of last week pointed out how the same effect has more recently formed the subject of a memoir, presented through M. Wurtz to the Paris Academy of Sciences, the author of that paper evidently being unaware that the subject had already been investigated by myself.

The appearance is as follows: When the hydrogen flame is brought either vertically or sideways, say, upon a white plate or a block of marble, there instantly appears a deep blue and glowing impression of the exact size and shape of the hollow flame. The moment the gas is extinguished, or the flame removed to the slightest distance from the solid, the effect as instantly ceases. If the flame be brought successively to the same spot on the solid, the effect grows fainter and finally vanishes, but instantly reappears upon an adjoining portion.

Other combustible gases, such as carbonic oxide, or marsh gas, or olefiant, or coal gas, do not yield this effect, nor does any lamp flame, luminous or otherwise; nor is it obtained in the oxidising flame of an ordinary blowpipe; but it is imperfectly produced in the reducing flame when coal gas is used; it is not seen when oxygen is driven through coal gas, unless the latter be in excess, and it is poorer and vanishes more quickly with the oxyhydrogen flame than with hydrogen alone. This blue luminosity is, therefore, not a question of heat, but some property depending either on (a) the chemical nature of hydrogen, or on (β) the physical effect of its radiation. At first I thought it was the latter, and that it was a new form of fluorescence, so closely did it resemble those phenomena. But after a week's incessant experimenting, the true cause was hunted down, and found to be dependent on the former effect (a), and in every case ultimately due to the presence of *sulphur*. A chemically clean body, or a freshly broken surface, did not show the blue coloration; but after exposure for a short time to the air of London, the substance invariably yielded the blueness; this, however, was not the case when the clean surface was covered by a shade, or exposed to the air of the open country. The combustion of coal gas and coal fires yields sulphate of ammonia, a body often deposited in acicular crystals in the glass tubes in a laboratory. Sulphate of ammonia is decomposed by a hydrogen flame, and when that salt is brought into contact with burning hydrogen, it permanently yields the blue colorescence. Hence this body is probably the main source of the blueness seen whenever a hydrogen flame comes into contact with glass tubes or a dirty surface. This effect must repeatedly have been seen by every one who has experimented on singing flames.

When the blueness, as is so often the case, is seen tinging the flame itself, without contact with any body, the sulphur is derived either from the vulcanised tubing, the dust of which is taken up by the passing gas; or if the hydrogen be burnt from the bottle generating it, the blueness is due to the decomposition of the sulphuric acid spray, as will be shown further on.

As a chemical re-agent for detecting sulphur, the delicacy of a hydrogen flame is extraordinary. This fact was estimated as follows:—Pure precipitated silica yields no blueness with the flame; 500 grains of silica were intimately mingled with one grain of milk of sulphur. Less than a $\frac{1}{100}$th of a grain of this mixture was thrown on the surface of pure water or placed upon chemically clean platinum foil. The water is best, but in either case the blue colour (absent before) now shot forth on bringing the hydrogen flame down. Tried again and again with fresh portions, the effect was very evident, but quickly vanished. The sulphur in a similar portion of the mixture could not be detected chemically by nitro-prusside of sodium. The wonderful sensitiveness of the flame may be still better seen in another way. Immediately after washing, the fingers show no colour when brought for a moment into the flame, but if a white india-rubber tube be touched ever so lightly, the fingers not only show a vivid blueness, but for some time any clean object touched by them, such as platinum foil, shows traces of sulphur by the appearance of the blue coloration with the flame. A block of melting ice continually weeps itself free from dust, and thus presents an excellent surface upon which to try the foregoing experiment. Or a plate of platinum, after heating to redness, may be written over with a stick of sulphur. If kept covered, the invisible letters may long after be traced out by sweeping the hydrogen flame over the surface of the platinum.

Examined through a prism, the blueness derived from any source shows blue and green bands, similar to the spectrum of sulphur, but I have noticed also a red band. This mode of obtaining a sulphur spectrum suggests further inquiry. White marble smeared over with a bit of sulphur, or with vulcanised rubber tubing, is a convenient source for obtaining the effect at pleasure.

Some sulphates and sulphides show the blueness with the flame, and are evidently decomposed by the hydrogen. Thus sulphate of soda gives no blue appearance, whilst sulphate of ammonia, or alum, does.

V. Various liquids were tried in contact with the flame. Sulphuric acid was very notable. Here a magnificent blue effect was observed. For persistence and brilliancy of the colour, this experiment leaves nothing to be desired; the spectrum is very fine. If the liquid is in a glass dish when the flame is brought vertically down, the blueness lights up the glass in a lovely manner.*

VI. But the presence of sulphur is by no means the only body that a hydrogen flame reveals. The least trace of *phosphorus* is detected by the production of a vivid green light. It is striking to notice the wonderful subdivision of matter in these experiments, and how an immeasurable trace of an element can evoke pronounced and apparently disproportionate effects.

Might not this ready detection of minute quantities of sulphur and phosphorus be of use in the manufacture of iron; and might not hydrogen introduced into the molten metal be employed for the removal of these great enemies of the iron worker? I speak ignorantly.

VII. Among the range of substances I have tried, *tin* was found to yield the most conspicuous effect, after the bodies named. A fine scarlet colour is almost instantly produced when the hydrogen flame is brought into contact with tin or any alloy of tin. Tin is somewhat volatile, and its spectrum is rich in red rays. The tin must be clean; or the sulphur blue, which is much brighter, will mask the effect. A charming experiment may be made by partially scraping a soiled surface of tin; the blue and the scarlet colours mingle, and a lovely purple is the result. When a trace of phosphorus is present there may be obtained a green belt encircling a rich blue, then a purple zone, and finally a glowing scarlet at the root of the flame. These colours, it must be remembered, are not imparted to the flame, but reside on the surface of the body which the flame touches. And where the combustion of the hydrogen is complete, as in the upper part of the flame, or in the luminous stream referred to (II.), these effects are not produced, they are best developed at the root of the flame.

VIII. Passing from liquids and solids, I next tried *gases* in contact with the flame of hydrogen. Many gases imparted a colour to the flame, but here the effect was

* With all liquids, but best with mercury, a fine musical note can be obtained by causing the jet to dip just below the surface of the liquid.

different to that previously noticed. The whole flame was tinged with the colour imparted to it. A mere trace of hydrochloric acid gas imparts a reddish brown to the flame; ammonia gas gives a yellow, and burns freely. It is striking to note the combustion of ammonia gas rising from an unstopped bottle that contains the usual solution and which is placed below the flame.

But carbonic acid gas yields the most striking result in contact with the flame. A pale lilac tinge is instantly produced by a stream of this gas. This, I imagine, is due to the decomposition of the carbonic acid by the hydrogen, and the production and combustion of carbonic oxide. For it is at the lower part of the flame that the effect is most marked. One per cent. of pure carbonic acid admitted to a jar of air, can be detected on holding the jar over the flame. The breath, of course, shows the effect most strikingly.

IX. Here then is an eminently practical method of noting the presence of vitiated air in rooms or public buildings. A continuous hydrogen apparatus might be employed with a wash bulb attached. The flame might be burnt from a brass burner or lava jet, placed within a blackened tin cylinder. Opposite the flame a hole might be pierced in the cylinder, and closed by a lens for better viewing the flame within. As soon as the atmosphere in a room becomes unpleasantly vitiated the flame would indicate the fact by its changed colour. A similar apparatus might likewise be employed by miners: in metal mines as a warning against impure air, and in coal mines as a detector of fire damp. In this latter case the ends of the cylinder could be covered with wire gauze.

To this practical aspect of the question I am now giving such little leisure as I possess.

The results thus briefly described demonstrate—

1. That the combustion of hydrogen exhibits some physical peculiarities, and produces phosphorescence on many substances with which it comes in contact.

2. That the blueness so often seen in a hydrogen flame is due to the presence of sulphur, derived either from the vulcanised rubber tubing, or from atmospheric dust, or from the decomposition of the sulphuric acid spray from the generator.

3. That a flame of hydrogen forms an exceeedingly delicate re-agent for the detection of sulphur or phosphorus, and possibly also of tin.

4. That many sulphates, and also carbonic acid, are apparently decomposed by a hydrogen flame.

5. That a hydrogen flame is further a test for the presence of some gases, notably carbonic acid.

6. That these results are capable of practical application.

W. F. Barrett

International College, Spring Grove, W.

THE INHABITANTS OF THE MAMMOTH CAVE OF KENTUCKY

Crustaceans and Insects

(*Concluded from page* 448)

NEXT to the blind fish, the blind crawfish attracts the attention of visitors to the cave. This is the *Cambarus pellucidus* (Fig. 10, p. 486, from Hagen's monograph of the North American Astacidæ) first described by Dr. Tellkampf. He remarks that "the eyes are rudimentary in the adults, but are larger in the young." We might add that this is an evidence that the embryo develops like those of the other species; and that the inheritance of the blind condition is probably due to causes first acting on the adults and transmitted to their young, until the production of offspring that become blind becomes a habit. This is a partial proof at least that the characters separating the genera and species of animals are those inherited from adults, modified by their physical surroundings and adaptations to changing conditions of life, inducing certain alterations in parts which have been transmitted with more or less rapidity, and become finally fixed and habitual. Prof. Hagen has seen a female of *Cambarus Bartonii* from Mammoth Cave, "with the eyes well developed," and a specimen was also found by Mr. Cooke. Prof. Hagen remarks that "*C. pellucidus* is the most aberrant species of the genus. The eyes are atrophied, smaller at the base, conical, instead of cylindrical and elongated, as in the other species. The cornea exists, but is small, circular, and not faceted; the optic fibres and the dark-coloured pigments surrounding them in all other species are not developed." It seems difficult for one to imagine that our blind crawfish was created suddenly, without the intervention of secondary laws, for there are the eyes *more perfect in the young than the adult*, thus pointing back to ancestors unlike the species now existing. We can now understand, why embryologists are anxiously studying the embryology of animals to see what organs or characteristics are inherited, and what originate *de novo*, thus building up genealogies, and forming almost a new department of science,—comparative embryology in its truest and widest sense.

Of all the animals found in caves, either in this country or Europe, perhaps the most strange and unexpected is the little creature of which we now speak. It is an Isopod crustacean, of which the pill bugs or sow bugs are examples. A true species of pill bug (*Titanethes albus* Schiödte) inhabits the caves of Carniolia, and it is easy to believe that one of the numerous species of this group may have become isolated in these caves and modified into its present form. So also with the blind *Niphargus stygius* of Europe, allied to the fresh water Gammarus so abundant in pools of fresh water. We can also imagine how a species of Asellus, a fresh water Isopod, could represent the Idoteidæ in our caves, and one may yet be found; but how the present form became a cave dweller is difficult of explanation, as its nearest allies are certain species of Idotea which are all marine, with the exception of two species: *I. entomon*, living in the sea and also in the depths of the Swedish lakes, as discovered by Loven, the distinguished Swedish naturalist, while a species representing this has been detected by Dr. Stimpson at the bottom of Lake Michigan. Our cave dweller is nearly allied to Idotea, but differs in being blind, and in other particulars, and may be called *Cæcidotea stygia.** (Fig. 11 side view, enlarged; Fig. 12 dorsal view; *b*, inner antenna; *c*, 1st leg.) It was found creeping over the fine sandy bottom, in company with the Campodea, in a shallow pool of water four or five miles from the mouth of the cave.

This closes our list of known articulates from this and other caves in this country, the result of slight explorations by a few individuals. The number will doubtless be increased by future research. It is to be hoped that our western naturalists will thoroughly explore all the sinks and holes in the cave country of the western and middle states. The subject is one of the highest interest in a zoological point of view, and from the light it throws on

* Generic characters. Head large, much thicker than the body, and as long as broad; subcylindrical, rounded in front. No eyes. First antennæ slender, 8-jointed (2nd antennæ broken off). Abdominal segments consolidated into one piece. Differs chiefly from Idotea, to which it is otherwise closely allied, by the 8-jointed (instead of 4-jointed 1st (inner) antennæ, the very large head, and by the absence of any traces of the three basal segments of the abdomen usually present in Idotea. Specific characters. Body smooth, pure white; tegument thin, the viscera appearing through. Head as wide as succeeding segment, and a little more than twiee as long. Inner antennæ minute, slender, the four basal joints of nearly equal length, though the fourth is a little smaller than the basal three, remaining four joints much smaller than others, being one-half as thick and two-thirds as long as either of the four basal joints; ends of last four joints a little swollen, giving rise to two or three hairs; terminal joint ending in a more distinct knob, and bearing five hairs. Segment of thorax very distinct, sutures deeply incised: edges of segments pilose; abdomen flat above, rounded behind, with a very slight median projection; the entire pair of gills do not reach to the end of the abdomen, and the inner edges diverge posteriorly. Legs long and slender, 1st pair shorter, but no smaller than the second. Length ·25 inch.

the doctrine of evolution. Prof. Schiödte, the eminent Danish zoologist, has given us the most extended account of the cave fauna of Europe, which has been translated from the Danish into the Transactions of the Entomological Society of London (new series, vol. i., 1851).

A pertinent question arises as to the time of the formation of these caves and when they became inhabitable. As previously stated, the caves of the western and middle States are in lower Carboniferous limestone rocks, though the Port Kennedy cave explored by Wheatley and Cope * is in the Potsdam limestone. They could not have been formed under water, but when the land was drained by large rivers. This could not have occurred previous to the Triassic period. Prof. Dana in his "Manual of Geology" shows that the Triassic continent spread westward from the Atlantic coast "to Kansas, and southward to Alabama; for through this great area there are no rocks more recent than the Palæozoic." "Through the Mesozoic period (comprising the Triassic, Jurassic, and Cretaceous periods) North America was in general dry land, and on the east it stood a large part of the time above its present level." Though at the close of these periods there was a general extinction of life, yet this was not probably a sudden (one of months and even years), but rather a secular extinction, and there may be plants and animals now living on dry land, which are the lineal descendants of Mesozoic and more remotely of Carboniferous forms of life. So our cave animals may possibly be the survivors of Mesozoic forms of life, just as we find now living at great depths in the sea remnants of Cretaceous life. But from the recent explorations in the caves of Europe and this country, especially the Port Kennedy cave, with its remarkable assemblage of vertebrates and insects, we are led to believe from the array of facts presented by Prof. Cope that our true subterranean fauna probably does not date farther back than the beginning of the Quaternary, or post-Pliocene, period. We quote his "general observations" in his article on the Port Kennedy fauna:—

"The origin of the caves which so abound in the limestones of the Alleghany and Mississippi valley regions, is a subject of much interest. Their galleries measure many thousands of miles, and their number is legion. The writer has examined twenty-five, in more or less detail, in Virginia and Tennessee, and can add his testimony to the belief that they have been formed by currents of running water. They generally extend in a direction parallel to the strike of the strata, and have their greatest diameter in the direction of the dip. Their depth is determined in some measure by the softness of the stratum whose removal has given them existence, but in thinly stratified or soft material, the roofs or large masses of rocks fall in, which interrupt the passage below. Caves, however, exist when the strata are horizontal. Their course is changed by joints or faults, into which the excavating waters have found their way.

"That these caves were formed prior to the post-Pliocene fauna is evident from the fact that they contain its remains. That they were not in existence prior to the drift is probable, from the fact that they contain no remains of life of any earlier period so far as known, though in only two cases, in Virginia and Pennsylvania, have they been examined to the bottom. No agency is at hand to account for their excavation, comparable in potency and efficiency to the floods supposed to have marked the close of the glacial epoch, and which Prof. Dana ascribes to the Champlain epoch. An extraordinary number of rapidly flowing waters must have operated over a great part of the Southern States, some of them at an elevation of fifteen hundred feet and over (perhaps two thousand) above the present level of the sea. A cave in the Gap Mountain, on the Kanawha river, which I explored for three miles, has at least that elevation.

"That a territory experiencing such conditions was suitable for the occupation of such a fauna as the deposits contained in these caves reveal, is not probable. The material in which the bones occur in the south is an impure limestone, being mixed with and coloured by the red soil which covers the surface of the ground. It is rather soft but hardens on exposure to the air.

"The question then remains so far unanswered as to whether a submergence occurred subsequent to the development of the post-pliocene mammalian fauna. That some important change took place is rendered probable by the fact that nearly all the neotropical types of the animals have been banished from our territory, and the greater part of the species of all types have become extinct. Two facts have come under my observation which indicate a subsequent submergence. A series of caves or portions of a single cave once existing on the south-east side of a range of low hills among the Alleghany mountains in Wythe Co., Virginia, was found to have been removed by denudation, fragments of the bottom deposit only remaining in fissures and concavities, separated by various intervals from each other. These fragments yielded the remains of twenty species of post-Pliocene mammalia.* This denudation can be ascribed to local causes, following a subsidence of uncertain extent. In a cave examined in Tennessee the ossiferous deposit was in part attached to the *roof* of the chamber. Identical fossils were taken from the floor. This might, however, be accounted for on local grounds. The islands of the eastern part of the West Indies appear to have been separated by submergence of larger areas, at the close of the period during which they were inhabited by post-Pliocene mammalia and shells. The caves of Anguilla include remains of twelve vertebrates,† of which seven are mammalia of extinct species, and several of them are of large size. These are associated with two recent species of molluscs, *Turbo pica* and a *Tudora* near *pupæformis*. ‡ As these large animals no doubt required a more extended territory for their support than that represented by the small island Anguilla, there is every probability that the separation of these islands took place at a late period of time and probably subsequent to the spread of the post-Pliocene fauna over North America."

I think the reader will conclude from the facts Prof. Cope so clearly presents, that the subterranean fauna of this country does not date back beyond the Quaternary period. These species must have been created and taken up their abode in these caves (Mammoth Cave and those of Montgomery County, Virginia) after the breccia flooring their bottoms and containing the bones of Quaternary animals had been deposited; or else migrated from Tertiary caves farther south, which is not probable, as it has been previously shown that those blind animals inhabiting wells immediately die on being exposed to the light. (British Sessile-eyed Crustacea, i. p. 313. Though this is true of the Gammandæ, Mr. Putnam tells me that a blind crawfish lived several days in a bottle of water exposed to the light, and is thus as hardy as any crustacean.)

* A notice of the animals found in this cave will be found in the Proc. Amer. Phil. Soc., April 1871. The insects there enumerated would probably not come under the head of cave insects.

* See Proceed. Amer. Phil. Soc., 1869, 171.

† Loc. cit. 1869, 183; 1870, 608. A fourth species of gigantic Chinchillid has been found by Dr. Rijgersma, which may be called *Loxomylus quadrans* Cope. It is represented by portions of jaws and teeth of three individuals. It is one of the largest species, equalling the *L. latidens*, and has several marked characters. Thus the roots of the molars are very short, and the triturating surface oblique to the shaft. The roots of the second and fourth are longer than those of the first and third. The last molar has four dental columns instead of three as in the other *Loxomyli*, and is triangular or quadrant-shaped in section; the third is quadrangular in section, and has three columns. The second is the smallest, being only ·6 the length of the subtriangular, first. Length of dental series m. ·063 or 2·5 inches. Palate narrow and deeply concave. There is but little or no lateral constriction in the outlines of the teeth; the shanks are entirely straight. In its additional dentinal column, this species approaches the genus *Amblyrhiza*. The large Chinchillas of Angudla are as follows, *Loxomylus longidens*, *L latidens*, *L. quadrans*, and *Amblyrhiza inundata*.

‡ See Bland, Proceed. Amer. Phil. Soc., 1871, 58.

Assuming, on the principles of evolution, that the cave animals were derived from other species changed by migration from the outer world to the new and strange regions of total darkness, it seems evident that geologically speaking, the species were suddenly formed, though the changes may not have been wrought until after several thousand generations. According to the doctrine of natural selection, by which animal species pass from one into another by a great number of minute variations, this time was not sufficient for the production of even a species, to say nothing of a genus. But the comparatively sudden creation of these cave animals affords, it seems to us, a very strong argument for the theory of Cope and Hyatt, of

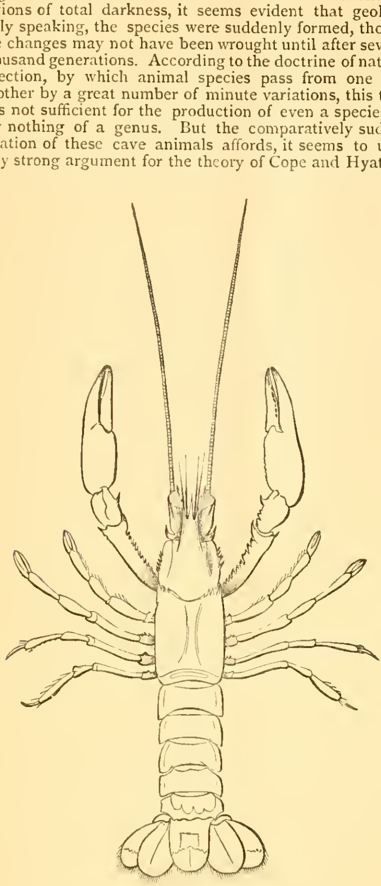


FIG. 10.—*Cambarus pellucidus.*

creation by acceleration and retardation. The strongly marked characters which separate these animals from their allies in the sunlight, are just those fitting them for their cave life, and those which we would imagine would be the first to be acquired by them on being removed from their normal habitat.

On introducing the wingless locust, *Ceuthophilus maculatus* into a cave, where it must live, not under stones, but by clinging to the walls, its legs would tend to grow longer, its antennæ and palpi would elongate and become more delicate organs of hearing as well as touch,* and the body would bleach partially out, as we find to be the case in *Hadenœcus subterranetus* and *stygius*. The Carabid beetle, Anopthalmus, extending farther into the cave, would lose its wings (all cave insects except the Diptera have no wings, elytra excepted) and eyes, but as nearly all the family are retiring in their habits, the species hiding under stones, its form would not undergo further striking modification. So with the blind Campodea, which does not differ from its blind congeners which live more or less in the twilight, except in its antennæ becoming longer. The blind Adelops, but with rudiments of eyes, does not greatly depart in habits from Catops, while on the other hand the remarkable Stagobius of the Illyrian caves, which according to Schiödte spends its life in crawling ten to twenty feet above the floors over the columns formed by the stalactites

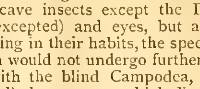


FIG. 11.—*Cæcidotea stygia* (side view).

to which unique mode of life it is throughout perfectly adapted, is remarkably different from other Silphids. Its legs are very long and inserted far apart (the prothorax being remarkably long), with surprisingly long claws, while the antennæ, again, are of great length and densely clothed with hairs, making them most delicate sense organs.* So also are the limbs of the false scorpion, and the spider and pill bug (Titanethes) of remarkable length.

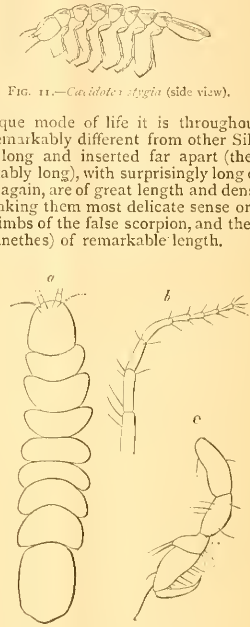


FIG. 12.—*Cæcidotea stygia* (dorsal view).

But the modifications in the body of the Spirostrephon are such that many might deem its aberrant characters as of generic importance. It loses its eyes, which its nearest allies in other, but smaller, caves possess, and instead gains in the delicate hairs on its back, which evidently form tactile organs of great delicacy; the feet are remark-

* After writing this article, and without the knowledge of his views, we turned to Darwin's "Origin of Species" to learn what he had to say on the origin of cave animals. He attributes their loss of sight to disuse, and remarks:—"By the time an animal has reached, after numberless generations, the deepest recesses, disuse will on this view have more or less perfectly obliterated its eyes, and natural selection will often have effected other changes, such as an increase in the length of the antennæ or palpi, as a compensation for blindness." (5th Amer. Edit., p. 143.) We are glad to find our views as to the increase in the length of the antennæ and palpi compensating for the loss of eyesight, confirmed by Mr. Darwin.

* Schiödte remarks that "it is difficult to understand the mode of life of *Stagobius troglodytes*, or how this slow and defenceless animal can escape being devoured by the rapid, piratical Arachnidans, or find adequate support on columns, for inhabiting which it is so manifestly constructed. We are led in this respect to consider the antennæ. Whatever significance we attach to those enigmatical organs, we must admit that they are organs of sense, in which view an animal having them so much developed as Stagobius, must possess a great advantage over its enemies, if these be only Arachnidans. Its cautious and slow progress, and its timid reconnoitring demeanour, fully indicate that it is conscious of life being in perpetual danger, and that it endeavours to the utmost to avoid that danger. Darkness, which always favours the pursued more than the pursuer, comes to its aid, especially on the uneven excavated surface of the columns."

ably long, as also the antennæ. These are not new formations, but simply modifications apparently, by use or disuse of organs present in the other species. The aberrant myriopod and Stagobius are paralleled by the blind fish, an animal so difficult to classify, and so evidently adapted for its abode in endless darkness. And as an additional proof of the view here taken that these cave animals are modified from more or less allied species existing outside of the caves, we have the case of the crawfish, whose eyes (like those of the mole), are larger in the young than in the adult, indicating its descent from a species endowed with the faculty of sight, while in the adult the appendages are modified as tactile organs so as to make up for its loss of eyesight, in order that it may still take its prey.

We thus see that these cave animals are modified in various ways, some being blind, others very hairy, others with long appendages. All are not modified in the same way in homologous organs; another argument in proof of their descent from ancestors whose habits varied as those of their out-of-door allies do at present. Had they been specially created for subterranean life, we should have expected a much greater uniformity in the organs adapting them to a cave life than we actually find to be the case.

Another fact of interest in this connection is the circumstance that these cave species breed slowly, being remarkably poor in individuals; they are nearly all, except the wingless grasshoppers, extremely rare. Did they breed as numerously as their allies in the outer world, the whole race would probably starve, as the supply of food even for those which do live is wonderfully limited.

It is now known that animals inhabiting the abysses of the sea are often highly coloured: light must penetrate there, for we know that were the darkness total they would be colourless like the cave insects.

In view of the many important questions which arise in relation to cave animals, and which have been too imperfectly discussed here, we trust naturalists the world over will be led to explore caves with new zeal, and record their discoveries with minuteness, and the greatest possible regard to exactness. The caves of the West Indian Islands should first of all be carefully explored. Also those of Brazil, those of the East Indies, and of Africa, while fresh and more extended explorations of our own Mammoth Cave should be made, perhaps by a commission acting under Government or State authority, in order that the most ample facilities may be afforded by the parties owning the cave. A. S. PACKARD

PROPOSED GRAND AQUARIUM FOR MANCHESTER

THE *Manchester Examiner and Times* of April 2 gives a long account of a Grand Marine Aquarium which it is proposed to build at Manchester, and which shows the interest which is felt in scientific studies in the northern capital. From this article we have made the following extracts, as showing the complete scale upon which everything is proposed to be carried out.

The funds are to be raised by a company started under the superintendence of a number of gentlemen resident in the city who are interested in marine zoology, and desire to promote scientific education in all its branches. The building will contain all the recent improvements shown to be necessary at the Crystal Palace and Brighton Aquaria, and will be rectangular in shape, 120 ft. long and 70 ft. wide. This space will be divided into two side galleries, each 120 ft. long and 15 ft. wide, separated from the central saloon by a light screen. Running along one side of each of these galleries will be a series of tanks, about eighty in number, forty in each gallery, varying in capacity from 300 to 3,000 gallons, and the roofs will be so arranged that the light will pass through the water at an angle of about forty-five degrees to the spectators, thus rendering distinctly visible the living inhabitants and plants contained in the grotto-like tanks. The grand saloon will be also 120 ft. long by 40 ft. wide, supporting on light iron columns an open panelled roof. All the windows will be so arranged as to admit only the exact quantity of light required, as it is found that an excess of light acts upon the higher marine plants and animals in a manner directly contrary to its action upon terrestrial life. It blanches them in a similar manner as ordinary plants are blanched by being earthed up. The most brilliant coloured marine plants are those which live in comparative darkness. The grand saloon will contain two tanks—the largest that have yet been constructed—one at each end of the room, 30 ft. long, 10 ft. wide, and 8 ft. deep, capable of containing each 15,000 gallons of water, and in which the largest specimens of fish found in the British seas will find ample room to display themselves. These tanks will have also a polished plate-glass frontage of great strength, through which the animals can be well seen.

In order to accommodate the inhabitants of what is called the littoral zone round our coasts, a series of shallow tanks, varying in capacity from 20 to 200 gallons, will be erected, in which the animals can be seen either from the surface of the water or through the transparent fronts, and by an ingenious contrivance the supply of water will be so regulated as to afford in every respect tidal currents. Besides these there will be other tanks at the back of the exhibition tanks for reserve stocks, and in the basement cisterns to hold a reserve supply of 60,000 gallons of sea water.

Such are the contemplated arrangements for marine, animal, and vegetable life; but in addition to these the inhabitants of our brooks, ponds, &c., will not be forgotten, and a series of table aquaria will be provided: while the larger inhabitants of our rivers and lakes will swim in an ever-flowing river and pond supplied by fountains, and placed in the centre of the grand saloon. Such is a brief description of the proposed Manchester Grand Aquarium, which, it is hoped, will both be a success in a scientific, as well as a pecuniary point of view. Mr. B. Hooper, a well-known naturalist, has been engaged as curator of the Aquarium. A site for the Aquarium has been obtained in the vicinity of the Alexandra Park, and it is proposed to open it on Saturdays and Mondays at an admission fee of 1*d.*; on Tuesdays, Wednesdays, and Thursdays, at 6*d.*; and on Friday, which will be a students' day, at 1*s.*

NOTES

THE following lectures in Natural Sciences will be delivered in Trinity, St. John's, and Sidney Sussex Colleges, Cambridge, during Easter Term, 1872:—"On Light and Heat" (for the natural sciences tripos), by Mr. Trotter, Trinity College; Mondays, Wednesdays, and Fridays, at 10, commencing Wednesday, April 17. "On Heat" (for the special examination for the ordinary degree), by Mr. Trotter, Trinity College; Tuesdays, Thursdays, Saturdays, at 11, commencing Tuesday, April 16. "On Chemistry," by Mr. Main, St. John's College; Mondays, Wednesdays, Fridays, at 12, in St. John's College Laboratory, commencing Friday, April 19. Instruction in Practical Chemistry will also be given. Attendance on these lectures is recognised by the University for the certificate required by medical students previous to admission for the examination for the degree of M.B. "On Palæontology" (the Mollusca), by Mr. Bonney, St. John's College; Wednesdays and Fridays, at 9, commencing Friday, April 19. "On Geology" (for the natural sciences tripos. Stratigraphical Geology), by Mr. Bonney, St. John's College; Tuesdays, Thursdays, and Saturdays, at 10, commencing Thursday, April 18. There will be excursions every Saturday. "Elementary Geology" (for the special examination); Wednesdays and Fridays, at 11, commencing Friday, April 19. "On Botany," by Mr. Hicks, Sidney College; Mondays, Wednesdays at 1 P.M., and Fridays, at 12, beginning Monday, April 15. The lectures this term will be chiefly on Cryptogamic Botany, the movements of plants, and the principles of classification. "On Embryology," the Trinity Prælector in Physiology (Dr. M. Foster) will deliver a short course at the new museums, beginning Monday, April 22, at 11 o'clock. The Physiological Laboratory is open for practical instruction in Physiology to all those who have gone through the elementary course.

WE have to record this week the death of *facile princeps* the most eminent of vegetable physiologists, Prof. Hugo von Mohl, which took place on April 1 at Tübingen. Von Mohl was born at Stuttgart in 1805, and in 1835 was appointed Professor of Botany and director of the Botanic Garden at Tübingen, a position he has held ever since. Conjointly with Schlechtendal, and since his death with Prof. de Bary, formerly one of his pupils, he has been editor of the weekly "Botanische Zeitung" since its commencement in 1843. He was one of the foreign members of the Linnean Society, having been elected as long ago as 1837. Von Mohl has been a copious and most accurate writer on subjects connected with vegetable anatomy and physiology, of which he may be said to have laid the secure foundation in his early investigations of the true relations of cell-membrane and contents. Among his original observations we may especially mention his essay on the Structure of Endogens, published by von Martius in his "Historia Palmarum," and on the Stem-structure of Cycads in the "Vegetable Cell," which appeared in Rudolph Wagner's "Handwörterbuch;" on the Origin and Structure of Stomates; on Cuticle; on the Structure of Cell-membrane; on the Structure and Anatomical relations of Chlorophyll; on the Multiplication of Plant-cells by division, and numerous other essays collected in his "Vermischte Schriften."

ASTRONOMY has sustained a heavy loss in the death of M. P. A. E. Laugier, which took place at Paris on the 5th inst., in the 50th year of his age. M. Laugier was a member of the French Academy and of the "Bureau des Longitudes," and examiner to the naval school at Brest. He was a pupil of Arago, and the following account of his various researches is furnished to the *Révue Scientifique* by M. G. Rayet:—In 1841 he presented a memoir to the Academy on Solar Spots, and was the first to determine their proper motion. The discovery and calculation of a telescopic comet in 1842 won for the young astronomer the Lalande gold medal. At the request of Humboldt he was engaged for some years in the improvement of the construction of astronomical clocks. In 1853 he made an exact determination of the latitude of the Paris Observatory, estimating it at 48° 50′ 11″·19, differing considerably from the earlier determination of Arago and Mathieu. In 1857 he published a catalogue of the declination of 140 stars, having previously issued one of 53 nebulæ. M. Laugier was associated with Arago in a number of his researches on the physics of the globe, and in his magnetic and photometric labours; and has for long made the observations on the declination and inclination of the magnet for the "Bureau des Longitudes." M. Rayet speaks of his death as a source of great grief to the Academy, which had formerly elected him president, and to his colleagues, by whom he was beloved for the moderation of his character, and his affable manners.

AT the meeting of the French Academy of Sciences on the 1st inst. the Abbé David and M. Ledieu were elected correspondents of the Section of Geography and Navigation, in the room of M. d'Abbadie, who has been elected a member of the Academy, and of the late Prince Demidoff.

THE Museums and Lecture Room Syndicate have presented their sixth Annual Report to the Senate of the University of Cambridge. It includes separate reports from Mr. J. W. Clark, superintendent of the Museum of Zoology and Comparative Anatomy, and from Profs. Humphry, Newton, Babington, Miller, Challis, Liveing, and Sedgwick. In response to an appeal from the venerable Prof. Sedgwick, the Woodwardian Museum has acquired during the past year (the purchase money having been raised by subscription) the very valuable collection of fossils made by Mr. Leckenby of Scarborough. Prof. Sedgwick considers the present geological collection of the University to be one of the noblest collections, as far as regards British geology, that exists in England, and for study and practical use, to be inferior to none existing in the island. In order to supply facilities for the practical study of Comparative Anatomy, and to supplement the lectures of Prof. Newton, Mr. J. W. Clark has commenced a class for practical work, for which, however, no sufficient accommodation is at present provided by the University. Mr. G. R. Crotch has been engaged for nearly the last twelvemonth in determining and arranging the extensive collections of insects, both British and exotic, contained in the Museum. The collection includes long series of those insects which were peculiar to certain localities in Cambridgeshire and the adjoining counties, and which, from increase of drainage in the fens and other causes, are either extinct or likely to become so in a few years.

THE discovery of two new planets is recorded. The elements of the first, No. 119, discovered by M. Paul Henry at Paris, are:—

April 9, 11^h, Paris M.T. R.A. = $13^h\ 18^m\ 59^s$. D. = $-8°\ 40'\ 23''$

The first position is approximate only. The horary movement is $-1^s{\cdot}75$ R.A., $+25''$ declination. It is of the 11th magnitude. The second was discovered by M. Borelly, and has the following elements:—

April 10, $12^h\ 16^m\ 32^s$, Marseilles M.T. R.A. $12^h\ 0^m\ 55^s{\cdot}38$. Polar distance, $95°\ 2'\ 44''{\cdot}9$.

April 10, $13^h\ 14^m\ 36^s$, Marseilles M.T. R.A. $12^h\ 0^m\ 53^s{\cdot}63$. Polar distance, $95°\ 2'\ 41''{\cdot}4$.

It is between the 11th and 12th magnitude.

THE next lecture to the Crystal Palace School of Science will be delivered this evening by Dr. W. B. Carpenter on "Researches in the Deep Sea."

DR. LIEBREICH will deliver his lecture on "Turner and Mulready" at the London Institution, Finsbury Circus, on Thursday evening next, the 25th inst., at 7.30 P.M.

THE following paragraph, copied *verbatim et literatim* from an evening contemporary, is a striking comment on our remarks last week on "Newspaper Science:"—"M. Agassiz has been finding out some more curious creatures in the deep-sea dredgings near Rio. It would really seem that if we only go deep enough we shall eventually reach the beginning of all things. Dr. Carpenter found living at the bottom of the Atlantic crustaceans of the same kind as those whose bodies now lie in our chalk hills, only seeming slightly degenerated, as if the family had once 'seen better days.' And now Prof. Agassiz tells his friend Prof. Peircei, of Harvard, in a long letter published in the American papers, how at 500 fathoms down he has fished pectens, and also other creatures, who are henceforth to bear the fearful but doubtless honourable appellation of Tomocaris Peircei, which resemble nothing living, only fossils of some of the earliest formations. The Tomocaris, in particular, is strongly suspected of being—we blush to name it—no better than a Trilobite! We shall not disturb our readers by quoting all the array of terrible words—maxillipeds, pygidiums, phyllopods, and the like—with which Prof. Agassiz's letter bristles; nor his interesting controversy with Prof. Milne Edwards concerning the Simulus, which animal's 'cephalo thorax' is so remarkable that 'the function of chewing is devolved upon the legs.' We only advise our friends who may be intensely anxious about these points to consult his letter *in extenso.*" Is the author of "What is a Joule?" the special scientific correspondent of all the daily papers?

WE learn from the *Journal of the Society of Arts* that King Victor Emmanuel has presented to the Geological Museum of the University of Rome a collection of Peruvian antiquities—silver vases, curious musical instruments, a coloured garment made from the bark of trees, and arrows and lances. The articles were discovered in a guano bed, and are antiques. The lances are notched, ornamented with feathers, and have wooden heads, showing that they were made before iron was used.

A REPORT of the meeting of delegates of the French departmental learned societies, held on the 4th inst., under the presidency of M. Jules Simon, is given in *Les Mondes*. The following medals were awarded:—Gold: to Grenier, of Besançon, botanical researches; Grandidier, scientific travels in Madagascar; Houzeau, of Rouen, researches in ozone. Silver: to Boussinesq, of Gap, mathematical mechanics; Tourdes, of Strasbourg, legal medicine; Faivre, of Lyons, vegetable physiology; Fromontel, of Gray, palæontology; Reboul, of Besançon, chemistry; Cailletet, of Châtillon-sur-Seine, agricultural and industrial chemistry; Mazure, of Bar-le-Duc, agriculture; Chautard, of Nancy, meteorology; Coquelin, of Beauficel, meteorology; Crova, of Montpellier, physics; Raoult, of Grenoble, physics; Mussy, of Montluçon, a geological map of Ariége.

PROF. HAYDEN has applied to the Congress of the United States for a grant of 75,000 dols. for the purpose of continuing for another year his most important geological survey of the territories of the United States. He proposes making a thorough series of astronomical, topographical, meteorological, geological, and chemical observations, which cannot but be of the utmost value in developing the material resources of the country. The application has the cordial support of the Secretary of the Interior.

*SCIENTIFIC INTELLIGENCE FROM AMERICA**

MAJOR POWELL has returned from the cañons of the Colorado, having left his party in the field in charge of Prof. Thompson. Since the party started in April last, it has passed through the cañons of Green River and the cañons of the Colorado to the mouth of the Paria, at the head of Marble Cañon. Here the Major left his boats for the winter, and he expects to return as soon as there is a favourable stage of water, and embark for the second trip through the Grand Cañon. On the way down the party explored the region to the west of the Green and Colorado, tracing the courses of the larger streams emptying into the two great rivers to their sources in the Wasatch Mountains and Sevier Plateau, and examined the geology of the great mesas and cliffs. Early in the winter a base-line 47,000 feet in length was measured on a meridian running south from Kanab, and the party is now engaged in extending a system of triangles along the cliffs and peaks among lateral cañons of the Colorado. During the past season the party has discovered many more ruins of the communal houses once occupied by the prehistoric people of that land. Many of these houses stood upon the cliffs overhanging the cañons, and many more are found in the valleys among the mountains to the west. Stone implements, pottery, basket-ware, and other articles were found buried in some of the ruins. The Major found a tribe of Utes on the Kaibab Plateau who still make stone arrow-heads and other stone implements, and he had an opportunity to observe the process of manufacturing such tools.—Mr. Joseph Sullivant, of Columbus, Ohio, a well-known naturalist, publishes an account in the *Ohio State Journal* of the capture of the *Bassaris astuta*, or ring-tailed cat of the Rio Grande region. It was taken in Fairfield County, Ohio, and was said to be accompanied by a second specimen. The occurrence of this animal so far north is very remarkable, and it may be a question whether it had not been brought from Mexico or California, and escaped from confinement. It is an animal very much sought after as a pet, being clean in its habits, and readily becoming very tame and affectionate; indeed, it would seem to be quite a desirable animal to domesticate and keep about the house as a protection against rats and mice. Some years ago a specimen of this same animal was brought into the Smithsonian Institution, having been captured in a hen-coop near the city. It was in capital condition and in full fur; but it had evidently escaped from captivity, as shown by the marks of the rubbing of a collar round the neck.—The greatest depth between the west end of Cuba and the coast of Yucatan found by the Coast Survey steamer *Bibb* is 1,164 fathoms, as reported to Prof. Peirce by Captain Robert Platt, commanding the surveying vessel. The lowest temperature observed is 39·5° F. at the bottom; surface, 81°; strongest current, two knots; direction, north. Dr. Stimpson reports the bottom from Cape San Antonio to Yucatan very barren of animal life. A few rare shells were found.—In a paper by Prof. Cope upon the *Pythonomorpha*, or Python-like fossil saurians of the cretaceous formation of Kansas, presented to the Academy of the American Philosophical Society of Philadelphia, he shows that America is the home of this group, four species only having been described from Europe. Forty-two North American species are already known, of which fifteen belong to the Greensand formation of New Jersey, seven to the Limestone region of Alabama, seventeen to the Chalk of Kansas, and three to other localities. Of the Kansas species six are described as new by Prof. Cope in the paper referred to.—A new fossil reptile, from the cretaceous strata of Kansas, has just been described by Prof Cope under the name of *Cynocercus incisus*. The peculiarity of this reptile consists in having the articular faces of the vertebræ deeply excavated above and below, so as to give them a transverse character. A new crocodilian from the same region was also described, under the name of *Hyposaurus webbii*.—Prof. Cope has shown, in a paper read to the American Philosophical Society, that the greater number of the fossil fishes of the cretaceous strata of Kansas belong to three families, namely, the *Saurodontidæ*, the *Pachyrhizodontidæ*, and the *Stratodontidæ*. Of the first family four genera and ten species are described in his paper, some of them (as those of the genus *Portheus*) being among the most formidable of marine fishes. Of the second family one genus and four species are introduced, and three genera and seven species of the third. The *Stratodus*, a genus of the *Stratodontidæ*, is provided with multitudes of minute shovel-headed teeth. He finds a great resemblance between this Kansas fauna and that of the English Chalk, no less than six of the eight Kansas genera having been found in the latter.—Some of our readers may remember the letter written by Prof. Agassiz to Prof. Peirce in December 1871, just before starting upon the Hassler expedition, in which he announced beforehand the general nature of the discoveries that he expected to make. His ability to make these predictions with any degree of certainty was much questioned by those who were not familiar with the method of research in natural history, and of the almost mathematical nature of the inferences to be derived from certain given premises. We now have a second letter addressed to Prof. Peirce, written at Pernambuco on Jan. 16, giving an account of experiences up to that date, which go far toward showing that the Professor really knew of what he was speaking in the first instance. Owing to various adverse influences, among them the necessity of hastening with all possible despatch to reach the Straits of Magellan at the earliest possible date, only four hauls of the dredge were made in water of any great depth, those being at depths of from 75 to 120 fathoms off Barbadoes. The results of these were in the highest degree satisfactory, however, "the extent and variety of material obtained being enough to occupy," in the Professor's words, "half a dozen competent zoologists for a whole year, if the specimens could be kept fresh for that length of time." As anticipated by the Professor in the letter referred to, the most interesting discoveries were certain forms of animals, the allies of which had previously been known in greater part or entirely as fossils of older formations. Among these may be mentioned a remarkable sponge, a crinoid very much like *Rhizocrinus*, a living *Pleurotomaria*, only three having been previously known, although a great many are described as fossil, &c. The crinoid, especially, is one of the very few living representatives of what was originally the prevailing character of

* Communicated by the Scientific Editor of *Harper's Weekly*.

the marine fauna of the silurian and other epochs; and while now they occur only in the very deepest water, they were formerly found crowded in the shallower seas. The inquiry, therefore, suggested itself to the Professor as to the reason of this difference, and he makes the suggestion that in the progress of the earth's growth we may look to such displacement of conditions favourable to maintaining certain low types as may recall most fully the adaptation to former ages, and that the deeper waters of the present constitution of our globe possibly approximate the conditions of animal life in the shallow seas of former ages as nearly as anything can in the present order of things on the earth. The depth of the ocean alone, he thinks, can place animals under the high pressure which the heavier atmosphere of the earlier period afforded. But as such pressure cannot be a favourable condition for the development of life, it is to be expected that the lower forms only will occur in the deep seas. Other causes acting in the same direction are the decrease of light in the greater depths, the smaller amount of free oxygen, the reduced amount and smaller variety of nutritive substances, &c. He does not think, however, that facts warrant the conclusion that any of the animals now living are lineal descendants of those of the earlier ages, nor that we may justly assume that the cretaceons formation is still extant, notwithstanding the similarity of forms. It would be just as true to nature to say that the tertiary period is exhibited in the tropics, on account of the similarity of the Miocene mammalia and those of the torrid zone.—The ninth number of the illustrated work on the butterflies of North America, in course of publication by Mr. William H. Edwards, has just made its appearance, and we are informed that the tenth number, to appear very shortly, will conclude the first volume. This number, like its predecessors, is accompanied by a great many quarto plates in the highest style of pictorial excellence, depicting some extremely beautiful species and varieties of butterflies. Among these are three varieties of *Papilio Ajax*, namely, *Walshii*, *telamonides*, and *Marcellus*. Mr. Edwards, in his paper, makes some judicious remarks upon the uncertainty that exists in regard to the true character of many butterflies which some naturalists consider as perfectly distinct species, and others as mere varieties. He takes the ground that the only way of coming to a satisfactory conclusion is to breed them, and ascertain whether the eggs from the same female develop similar larvæ or not, and whether these, even if different, produce the same perfect insects or different ones. The attempt at discriminating from the perfect insect alone he considers extremely unsatisfactory.

ANNUAL ADDRESS TO THE GEOLOGICAL SOCIETY OF LONDON, FEB. 16, 1872

By J. Prestwich, F.R.S., President

(*Concluded from p.* 472.)

It has been urged as a fatal objection to the discovery of coal in the south-east of England, that the Coal Measures become unproductive and thin out under the Chalk, as they range from Valenciennes towards Calais, and, therefore, that the coal-trough or basins end there. It is perfectly true that the Coal Measures do thin out between Bethune and Calais, but not in the sense of their dying out owing to their deposition near the edge of a basin. In that case, each seam, each stratum, would gradually become thinner and disappear; but such is not the fact. None of the beds of the Belgian coal-field are thick. The average does not exceed $2\frac{1}{4}$ feet. At Valenciennes it is the same; whereas M. Burat states the mean thickness of the beds actually increases westward of Bethune to more than $2\frac{1}{2}$ feet. With respect also to the extreme end of this basin, the lower beds there brought up correspond with the bottom beds of the Hainault basin, where the lower 650 feet consist of unproductive measures. The thinning-out is, in fact, due to denudation, just as the Bristol coal-field thins out at Cromhall to resume in the Forest of Dean, or the coal-field of Liége thins out at Nameche to resume at Namur in the great field of Charleroi and Mons.

The deterioration of the coal in the small coal-field of Hardinghen, near Boulogne, has also been adduced against the occurrence of workable coal in South-Eastern England, but Mr. Godwin-Austen has shown that this Hardinghen coal-field is one of those small local developments of coal-bearing strata intercalated in the Mountain Limestone, and is of older date than the great Belgian coal-field. It has, therefore, no bearing on this part of the question.

Another objection to which much weight has been attached is that the coal-field of Bath and Bristol forms an independent basin, cut off both on the east and on the west by ridges of Millstone Grit and Mountain Limestone, so that there is an end of the eastern extension of the Coal Measures. This is quite correct as far as regards the western edge, and is probably the case on the eastern, although as the edge of the basin is there covered by Secondary rocks, some uncertainty still exists about the disposition of the Palæozoic rocks under them. Admitting, however, the basin to be complete and isolated, that is no proof that the older Palæozoic rocks prevail exclusively to the east; for the Coal Measures of the Somerset basin maintain their full development to the edge of the basin, and are there cut off by denudation, and are not brought to an end by thinning out. They form really part of a more extended mass, of which we have there one fragment; while on the west another portion exists in the Welsh basin, and another in the newly discovered small basin of the Severn valley.

This last basin is entirely covered by the New Red Sandstone; and as the Welsh basin is bounded on the east and the Bristol basin on the west by Mountain Limestone, the same argument as the one above might have been used to show the impossibility of coal occurring in this intermediate area.

But the fact is, it is the very nature of this great line of disturbance to have minor rolls and flexures of the strata at, or nearly at, right angles to it, and so causing breaks in the coal-trough, which would otherwise flank it without interruption; thus the Aix-la-Chapelle coal-field is separated by older rocks from that of Liége, which is again separated by a ridge of Mountain Limestone from that of Hainaut. So in the case of south-western England, we have the several basins of South Wales, Severn Valley, and Bristol, separated by tracts of Mountain Limestone and Old Red Sandstone, the extremes of the intervening belts of older rocks being two miles at Nameche and eighteen miles in Wales. These barriers are clearly only local, and the division of the Coal Measures into separate basins appears to be their ordinary condition along this great line of disturbance. The length of the two known portions of the axis included between Pembrokeshire and Frome, and between Calais and Westphalia, is 472 miles, and in this distance we find eight separate and distinct coal-fields. The combined length of these eight coal-fields is about 350 miles, leaving about 122 miles occupied by intervening tracts of older rocks; so that nearly three-quarters of the whole length is occupied by coal-strata. I consider that a structure which is constant (so far as the axis of disturbance can be traced above ground) is, in all probability, continued under ground in connection with the range of the same line of disturbance; and I see no reason why the coal-strata should not occupy as great a proportionate length and breadth in the under-ground and unknown, as in the above-ground and explored area.

With respect to the possibility of denudation having removed the intervening Coal Measures, enormous as the extent of denudation must have been previous to the deposition of the Permian strata, we cannot admit its exceptional action in this case. Denudation has removed from the crest of the Mendips a mass of strata possibly equal to two miles or more in height, and from that of the Ardennes as much as three or four miles, and it has also worn extensive channels between many of our coal-fields, so that the power of such an agent cannot be denied. But it is a power of planing down exposed surfaces rather than of excavating very deep troughs. Notwithstanding its immense planing-down action on the Mendips and Ardennes, deep troughs of Coal Measures are left flanking their northern slopes. These troughs descend to more than a mile beneath the level of the sea; and I do not think it probable that those underground intermediate portions of the trough where the axis is lower, would have suffered more than those on the higher levels, unless it were to the extent caused by the later denudation which preceded the Cretaceous period. But this would not affect the main bulk of the Coal Measures. The Belgian coal-field, which was exposed to the action of both these denudations, still retains vast proportions.

I may remark that the pre-Cretaceous denudation was very irregular in its action. At one place near Mons the Chalk and Tertiary strata are above 900 feet thick; whilst at another, on about the same level, and at but a short distance, they are not 100 feet thick—an old under-ground hill of highly inclined

Coal Measures causing this difference, and rising in the midst of the unconformable newer strata. This shows that in the English Chalk area we may possibly find irregular old surfaces of this kind, so that the Coal Measures may exist at places nearer the surface than we have estimated.

We have alluded before to the great length and narrow width of the Belgian coal-fields. That of Liége is forty-five miles long, with a mean width of less than four miles, whilst that of Hainaut and Valenciennes is 119 miles long, with a width scarcely greater. The presence of lower Carboniferous rocks under Harwich, and the wider range north and south of the Bristol coal-field, renders it possible that the trough in the intermediate area may have a greater expansion than in Belgium; but we have nothing else to guide us, unless it be that the lateral pressure in the intermediate ground was probably less than in the Ardennes and the Mendips, where it has exercised its maximum elevatory force. In that case the coal-trough in this intermediate area would be less compressed and more expanded; so we might consequently here look to find larger coal-basins than either those of Somerset or Liége. The position of these basins I am disposed, for reasons given in my Report, to place farther north than Mr. Godwin-Austen, and should therefore look for them not in the valley of the Thames, or on the line of the North Downs, but under South Essex, Middlesex or Hertfordshire, Oxfordshire, and North Wiltshire.

The strata on the south side of the Liége coal-field rise abruptly against highly inclined and faulted Devonian rocks, and the north side they rise at a less angle beneath Cretaceous or Tertiary strata. In the Hainaut coal-field the overlying have a greater extension. Under these strata the Coal Measures are succeeded by the Mountain Limestone, and then by Devonian or Silurian strata; but with one or two limited exceptions their outcrop is hidden by the newer strata which stretch uninterruptedly northward over the rest of Belgium. The Palæozoic strata have, however, been met with near Brussels, under Tertiary strata, at a depth of about 600 feet, and at Ostend at a depth of 985 feet, of which 682 consisted of lower Tertiary strata, 210 feet of Chalk, and 93 of coloured marls. It appears, therefore, not improbable, that the Tertiary and Cretaceous strata of all Belgium may repose directly on a floor of Palæozoic rocks; and as there is reason to suppose that all these rocks have a strike parallel with that of the Ardennes, folds in the strata may bring in some under-ground coal-basin or basins in parallel lines to the north, in the same way that small troughs of Coal Measures are brought in again in the Ardennes to the south of the great coal-trough. On the other hand, the great Palæozoic axis of the Ardennes, consisting of Silurian and Devonian rocks, Mountain Limestone, and Coal Measures, passes westward under the Chalk of the north of France, and has been followed under ground as far as to Calais, where it lies at a depth of 1,032 feet; while in the direction of Boulogne it keeps nearer the surface, outcrops from beneath the Chalk downs surrounding the Boulonnais, and disappears westward under an unconformable series of Jurassic and Wealden strata.

We may, I think, look for a prolongation of this old Palæozoic surface of highly inclined, contorted, and faulted rocks at no very great depth under the same Wealden, Chalk, and Tertiary area of the south of England. For, although the old Palæozoic surface descends rapidly from about 300 feet above the sea-level in the Boulonnais to 1030 feet below it at Calais, it rises at Ostend 47 higher than at Calais, and crossing the Channel, it is found at Harwich within a few feet of the same depth as at Calais, from which it is eighty miles distant in a northerly direction. Passing westward from Calais, we find the Palæozoic rocks under London 105 miles distant, and 102 feet higher than under Calais, and 106 feet higher than at Harwich. Allowing for irregularities of the old surface as evinced by the well at Crossness, near Plumstead, which was still in the Gault at a depth of 944 feet, or some 14 feet below the level of the Palæozoic rocks at Kentish Town, we may still consider that in the area between these three points, and possibly throughout the south-east of England, the Palæozoic rocks will probably be found not to be more than from 1,000 to 1,200 feet beneath the sea-level.

Projecting the line another 100 miles westward, we reach the neighbourhood of Bath and Frome, where the Coal Measures are, as before mentioned, lost at a depth of about 450 feet, beneath Liassic and Jurassic strata. In the intermediate area between that place and London no trial-pits and no wells have been carried to a depth of anything like 1,000 feet beneath the sea-level. The deepest well with which I am acquainted is one near Chobham, in Surrey, through Tertiary strata and Chalk to a depth of about 800 feet, or of 550 feet beneath the sea-level.

There are, however, in all this area certain indications of the proximity of old land and of pre-Cretaceous denudation, in the presence of quartz and Lydian pebble-stones, accompanied by Secondary rock fossils in the Lower Greensands of Surrey, and in the like old rock pebbles, with the addition of slate pebbles, in that formation in North Wiltshire; while the banks of shingle, Bryozoa, and sponges of the same age at Farringdon, point to still and sheltered waters, probably of no great depth, and to adjacent dry land. Again, on the north of London, we have in the Lower Greensand of Buckinghamshire and Bedfordshire shingle beds consisting almost entirely of fossils derived from Jurassic strata, with a remarkable collection of larger quartz, quartzite, and other rock-pebbles, derived probably from the old Palæozoic axis.

On the south also of the great Mendip and Ardennes axis coal strata may possibly be found just as they are found on both sides of the Pennine chain; for in either case the measures are cut off and broken through by these chains of hills. In South Wales certain folds of the older strata seem to render it probable that the Coal Measures may pass under the Bristol Channel, forming a trough which prolonged eastward would pass along the south side of the Mendips. Trials in the latter area, have, however, shown that the New Red Sandstone, Lias, and Oolitic series attain an infinitely greater thickness than on the north flank of that range, so that it is not likely that the Coal Measures would lie at a less depth than from 1,500 to 2,000 feet.

On further consideration of the subject, it seems to me a question whether we should not take a still broader view of this great east and west axis, and assign to it a width varying from thirty to eighty miles or more, looking at the Mendips and Exmoor hills as the bounding flexures north and south of the same line of disturbance in South-western England, while the ridges of the Ardennes, the Eifel, and the Hundsruck (in part?) are exhibitions of the same parallel series of anticlinals. In that case the great coal-basins of South Wales and Somerset would represent the synclinal trough on one side of the axis of disturbance, and on the other side we should have the Lower Carboniferous or Coal Measures of Devon; while on the Continent the deep, narrow synclinal trough of the Liége and Aix-la-Chapelle basin may be considered as lying on one side of the arch, and the great coal-basin of Saarbruck on the other. This important coal-basin has already been followed under the New Red Sandstones of the Vosges for a distance of from twenty-four to thirty miles in the direction of Metz, still on the strike of the Ardennes. Further westward, a trial for coal near Doncherry led to the discovery of Palæozoic rocks, at a depth of 1,090 feet under that thickness of Lias and Infralias; but the line of the coal-trough should, I think, pass a few miles to the south of this spot. Thence this underground coal-trough would range in an irregular east and west line, keeping parallel, or nearly parallel, with the Mons and Valenciennes troughs, under the north of Champagne, Normandy, the Channel, between the Isle of Wight and Cherbourg, Dorset, and cropping out again in North Devon. The only deep sections that I know of on this line are those furnished by a well sunk many years since, nine miles east of Dieppe, to a depth of 1,092 feet in the Kimmeridge clay and other strata; and another by a boring at Sotteville, near Rouen, through a thin capping of Cretaceous strata, to a depth of 1,050 feet in the same Kimmeridge clay—in either case without reaching the Palæozoic rocks. At Paris no Palæozoic rocks have been reached at a depth of 2,000 feet.

In this country the newer strata, overlying the Palæozoic rocks on our presumed anticlinal line, have been sunk through, without result, in the lowest beds of the Wealden at Hastings to a depth of 486 feet, in the upper beds at Earlswood, near Reigate, to a depth of about 900 feet, and, on the presumed synclinal line of Carboniferous rocks, through Chalk at Chichester, to a depth of 945 feet, and at Southampton, through Tertiary strata and Chalk to a depth of 1,317 feet.

To the south of all the area we have now described, there existed during the Carboniferous period, the ranges of the older Palæozoic strata of the Hunsdruck and Vosges—of the old crystalline rocks of Central France, fringed on the east and north with small outlying coal basins of the old Palæozoic rocks of Brittany—and of the Silurian rocks of South Cornwall—forming the old land-surface, fringed by the great coal-growths subtended northwards through Northern France, Western Prussia, Belgium, and

England, to the Silurian uplands of Central Scotland on the north, and those of the Welsh and Cumbrian highlands on the west, and possibly to those of the Scandinavian hills on the north-east. After the formation and consolidation of the Coal-strata, the southern area of this great Carboniferous basin was then subjected to that remarkable disturbance which, for a distance of above 800 miles, exercised that excessive lateral pressure by which the older underlying strata were squeezed and forced up into the series of sharp anticlinals forming the axis of the Mendips and Ardennes, while portions only of the Carboniferous series were preserved from the denudation which followed, in deep synclinal troughs flanking the main axis.

The central and northern portions of the great Carboniferous basin, which were not raised by this disturbance, were then overspread by strata of the Permian series ; after which the northern section of the original coal area was traversed by that other great disturbance at nearly right angles to the former one, by which fresh portions of the Coal Measures were brought up in our central and northern counties, still leaving other deeper-seated portions to be afterwards covered by Triassic and Jurassic strata.

At a much later period the emerged southern area of Palæozoic rocks, including the westward prolongation of the great coal-trough of Belgium, or portions thereof, was submerged and covered over by the several formations of the Greensands, Chalk, and Lower Tertiaries now forming the surface of the south east of England.

The trials to discover these possibly productive coal-basins must necessarily be attended with considerable uncertainty. We shall have to feel our way. Of our hope of their ultimate success I have given you the reasons. Nor could such trials near London scarcely fail of some important results ; for, even if we did not hit at first upon the Coal Measures, it is probable that the Lower Greensand would at some spots be reached, so that the inestimable additional benefit of a large and steady supply of pure water might also be obtained, and, with proper care to prevent undue interference, might be maintained for all time.

And now, gentlemen, in retiring from the chair, which I have had the honour to occupy during the last two years, allow me to express the sincere satisfaction I have experienced in witnessing the continued prosperity of the Society, and the unanimity and oneness with which its labours are carried on. It was a post I long hesitated to accept, but which your kind forbearance and the friendly co-operation of your officers, has not only rendered easy, but as pleasant as it has been gratifying. I feel assured of the continued prosperity and usefulness of the Society when I resign my trust into the hands of a nobleman so distinguished as a statesman, so able as a writer, and so long known amongst us an active and zealous geologist, as the Duke of Argyll.

SCIENTIFIC SERIALS

The Lens, a quarterly journal of microscopy and the allied natural sciences, with the Transactions of the State Microscopical Society of Illinois, edited by S. A Briggs. No. 1, January 1872. Chicago, U.S. This long-promised journal has at length made its appearance, and we learn from its first number that it was printed and ready for the mail when the great fire occurred. With the exception of a few copies, the whole edition was destroyed, and on recovery from that disaster had to be reprinted. We have cause, therefore, to congratulate the publishing committee on recovering themselves so speedily as to issue their first number with the new year. Amongst its contents we note the following :—"Conspectus of the families and genera of the Diatomaceæ," by Prof. H. L. Smith. This is an artificial key, and like all such efforts has its good and bad sides. As a help such guides are useful, but they are seldom satisfactory. A table of synonyms is promised in the next number.—"The Flora of Chicago and its vicinity," by H. H. Babcock, is hardly such a subject as we should expect to find in a microscopical journal, since the list of Phanerogamic plants, with localities, here commenced, contains no single note of microscopical observation. To the local botanist it will probably make amends for this by its practical utility.—"On the preparation and preservation of sections of soft tissues," by Dr. J. N. Danforth, contains practical observations on the preparation of morbid animal tissues without artificial hardening.—"Microscopical Memoranda for the use of Practitioners of Medicine," by Dr. J. J. Woodward, U.S. Army, is the first portion of a more elaborate treatment of the same subject, which is to be contained in succeeding numbers. Dr. Woodward's reputation on this side the Atlantic as a practical microscopist is a sufficient guarantee for these memoranda.—"A new fossil Echinus," by O. S. Wescott, is named by the author *Oligoporus Groveri*, and found in the limestone region of Hancock County, Illinois.—"The Diatomaceæ of Lake Michigan," by S. A. Briggs, is simply a list of species. —"A New Method of Illuminating Opaque Objects under high powers," by Dr. H. A. Johnson. This new method consists in sending a beam of light down the oblique body of the binocular upon the prism, by means of a plane mirror or rectangular prism ; by this arrangement objectives as high as $\frac{1}{25}$ in. have been used successfully by daylight and lamplight.—A reprint from the *Monthly Microscopical Journal* and some notes complete the present number. The losses which the Academy of Sciences of Chicago sustained by the late destructive fire are detailed, in so far as the natural history collections and library are concerned. All British naturalists will sympathise with those of Chicago at their irreparable misfortune in such losses as the Smithsonian collection of crustacea, which filled 10,000 jars, and the invertebrates of the U.S. North Pacific Exploring Expedition, besides the thousands of specimens, zoological, botanical, and mineralogical, in the general collection.

Journal of the Chemical Society, Jan. 7, 1872.—Dr. Gladstone has continued his experiments on various essential oils ; amongst others he has examined four new oils, those of citron, lign aloes, pimento, and vitivert ; the author has separated the hydrocarbons contained in most essential oils into three polymeric groups to which the formulæ $C_{10}H_{16}$, $C_{15}H_{24}$, and $C_{20}H_{32}$, have been assigned. The two bodies first mentioned have the vapour density required by theory, the third has not been examined, the three bodies also differ in their solubility in alcohol, and in their expansibility by heat. The physical properties and chemical composition of several oils have been studied in detail, and are here described. Dr. Armstrong contributes a third paper on the nitrochlorphenols, the results obtained, however, are not suitable for useful abstraction. Amongst the abstracts there is one by E. Budde "on the action of light on Chlorine and Bromine." The author has exposed chlorine to the action of various parts of the solar spectrum, he found that when the bulb of gas was exposed to the violet and ultra-violet rays, there was from six to seven times as great expansion as took place in the red and yellow part of the spectrum. An ordinary differential air thermometer and also one charged with carbonic anhydride and ether, placed in the blue and violet parts of the spectrum, showed no increase in temperature. The author is of opinion that the hypothesis which he has advanced in explanation "that the chemically active light actually decomposes the chlorine molecules into chlorine atoms" is not a little supported by the fact that the rays which cause the expansion of chlorine coincide with those which are known to render it chemically active. The author believes that the light causes the separation of the molecules into atoms, and that the isolated atoms combine again with the production of heat, and thus lead to an increase of temperature which would account for the expansion of the gas as observed.

THE articles of most general interest in the *Journal of the Franklin Institute* for January are by Mr. F. A. Genth, on the Mineral Resources of North Carolina ; and by Mr. G. W. Baird, U.S.N., on the Absorption of Gases by Water, and on the organic matters contained therein. The latter contains a series of experiments on the volume of different gases capable of being dissolved in a unit volume of water, and on the amount of oxygen necessary to oxidise the organic matter contained in the water.—The editorial notes contain descriptions of a number of novelties in mechanics and physics.—Mr. J. Farrand Henry continues his series of papers on the Flow of Water in Rivers and Canals, and Mr. J. H. Cooper his article on Belting Facts and Figures.—There is also a report by Mr. W. M. Henderson on some experiments on the explosion of steam-boilers, carried out by a committee of the Franklin Institute at the instance of the engineers of some of the American railways.

THE *American Journal of Science and Art* for February is mainly geological. It commences, however, with some observations by Prof. C. A. Young on Encke's comet, at the Dartmouth College Observatory, accompanied by drawings. He identifies the spectrum with that of Comet II. 1868 (Winnecke's comet) described by Mr. Huggins in the Philosophic Transactions for that year.—Prof. J. D Whitney has a note on the occurrence of the "Primordial Fauna" of Nevada, which he considers indicates most unequivocally the Potsdam period of the Silurian age, and carries the Primordial Fauna much farther west than it had been found before.—Prof. Dana's notice of the address of Prof. T. Sterry Hunt, before the American Association at Indianapolis has already appeared *in extenso* in our columns.—Prof. Roland Irving on the age of the Quartzites, Schists, and Conglomerates of Sauk Co., Wisconsin, holds them to be unquestionably islands in the Potsdam Sea, furnishing beautiful illustrations of wave action on a rocky coast.—Prof. Hayden gives an extremely interesting account, illustrated by maps, of the hot springs and Geysers of the Yellowstone and Firehole Rivers, the result of the recent Government exploration of that district. Prof. T. Sterry Hunt continues his notes on granite rocks, and Mr. A. S. Verrill his contributions to Zoology from the museum of Yale College.

SOCIETIES AND ACADEMIES

London

Royal Society, April 11.—"Researches on Solar Physics."—III. By Warren De La Rue, F.R.S., Balfour Stewart, F.R.S., and Benjamin Loewy.

The authors present in this paper the third instalment of the determination of the areas and heliographic positions occupied by the sun-spots observed by the Kew photoheliograph, comprising the years 1867, 1868, and 1869. They announce that the fourth and last instalment is in active progress, and will be preceded by the final discussion of the whole ten-yearly period, during which the photoheliograph has been at work. This final discussion will contain the determination of the astronomical elements of the sun on the basis of photographic observations, and this work they anticipate will not only settle the question of rotation for a considerable time to come, but will also throw light upon many points which have only recently been brought under the consideration of scientific men. The results in general, they believe, will prove the superiority of photographic sun-observations over previous methods. The second question which will be discussed is the distribution of sun-spots over the solar surface. The facts already brought out indicate that the progress of the inquiry may lead to some definite laws which regulate the distribution; there appear to exist centres of great activity on the sun, and the different solar meridians seem to have various but definite intervals of rest and activity. In conclusion the authors point out the necessity of devoting in future greater attention to the study of the faculæ, and express a hope of seeing photographic sun-observations carried on in this country on a more extended system, connecting from day to day solar phenomena with terrestrial meteorology and magnetism.

Correction to Messrs. De La Rue, Stewart, and Loewy's papers "On some recent Researches in Solar Physics, &c."

The erroneous date given in our paper for one of Professor Wolf's maxima has already been corrected by us, and we give in the subjoined little table the corrections of the few numerical data which are necessitated by the error of fixing the date of maximum at 1846·6 instead of 1848·6.

Prof. Wolf's ratio $\frac{A}{B}$ (p. 86).

Erroneous figures given previously.		Differences.	Corrected figures.		Differences.
I. 1·265	Mean 2·093	—0 728	1·265	Mean 1·548	+0·283
II. 2·615		+0·522	1·478		+0·073
III. 2·400		+0·307	1·900		+0·352

The differences derived from our own results are respectively +0·061, and —0·047, that is, they are still much smaller, and agree singly better with the mean, than if Prof. Wolf's ratio were adopted; hence our conclusion is quite unaffected by this correction.

The remark made by us with reference to this maximum will remain in force even with the corrected date. We stated there that this particular maximum showed alone an appreciable difference from the dates fixed by ourselves, for it will be found that Prof. Wolf's date differs still by about three-quarters of a year from ours.

"Contributions to the History of the Opium Alkaloids."—Part V. By Dr. C. R. A. Wright.

"The Action of Oxygen on Copper Nitrate in a state of Tension." By Dr. J. H. Gladstone, F.R.S., and Alfred Tribe, F.C.S.

In our experiments on the action between copper and nitrate of silver in solution, we frequently noticed that the tips of the silver crystals became red, as though coated with a thin layer of metallic copper.

This apparent deposition of a positive on a more negative metal of course raised our curiosity, and led us to look closely into the circumstances under which it occurred. We found that it took place only when the nitrate of silver was exhausted, and only on those silver crystals which remained in metallic connection with the copper. We found, too, that the cupreous coating formed most readily where air had the freest access; and, in fact, that it would not form at all in vessels from which oxygen was excluded, nor on those white crystals which were far below the surface of the liquid, though they might be in immediate contact with the copper plate. When an inverted jar was filled with nitrate of copper solution and silver crystals resting on branches of copper, and the liquid was displaced by oxygen gas, it was found that the tips of the crystals became red, and the solution gradually filled the jar again by the absorption of the gas. In the same way the oxygen was absorbed from air, or from its mixtures with hydrogen or carbonic anhydride. This action was further studied by employing plates of the two metals instead of copper covered with silver crystals. When the two plates, connected by a wire, were partially immersed in an ordinary aqueous solution of copper nitrate, it was found that a slight yellowish deposit made its appearance speedily all over the silver plate, and went on increasing for a day or two, while at the air line there was a thicker deposit, which gradually grew and extended itself a little below the surface. This deposit changed from yellowish to red, and under the microscope presented a distinctly crystalline appearance. Thinking that this slight crust all over the silver plate was due to the air dissolved in the solution itself, we took advantage of the reaction to prepare copper nitrate absolutely free from dissolved oxygen. An ordinary solution of the salt mixed with some silver nitrate was placed in a narrow cylinder, with a long piece of copper foil arranged somewhat spirally so as to retain the deposited silver on its surface, and allowed to rest for twenty-four hours.

The solution thus obtained was exposed to the action of the conjoined copper and silver plates, but even after some hours there was no diminishing of the lustre of the silver plates, except at the air line, which was sharply defined. The same solution shaken for some time in the air produced a yellowish deposit on the white metal in three minutes.

The colour and general appearance of this crust, together with its formation only where oxygen can be absorbed, showed that it was not metallic copper, but the suboxide.

This was further proved by the action of diluted sulphuric acid, which resolves it at once into red metallic copper and copper sulphate. There is also another curious reaction which can only be properly observed under a microscope.

When treated with a solution of silver nitrate, this cupreous deposit does not give the ordinary crystals of the white metal; in fact, it is only slowly acted upon, but presently there shoot forth thin threads of silver which run through the liquid, often twisting at sharp angles, while the yellowish crystals change to black. This also was found to be a property of the suboxide of copper. This deposition of oxide on the silver is accompanied by a corresponding solution of copper from the other plate.

Thus, in an experiment made with nitrate of copper solution that had been exposed to air, and which was allowed to continue for four days, there was found—

Gain of silver plate, 0·016 grm.
Loss of copper plate, 0·015 grm.

The copper necessary for the production of 0·016 grm. of suboxide would be a little above 0·014 grm.

The wire connecting the two plates in this experiment is capable of deflecting a galvanometer. The current takes place from copper through the liquid to silver, that is, in the same direction as if the copper had been dissolved by an acid, and hydrogen evolved on the silver plate.

If the two plates have their sides parallel, the suboxide is deposited not merely on that side of the silver plate which faces the copper, but after about a minute on the other side also, showing that in this, as in other cases, the lines of force curve round.

It became interesting to consider what started this electric current. The original observations convinced us that it was not due to the action of oxygen on the copper; but to make the matter more certain, bright copper and silver plates in conjunction were immersed, the copper in a pure, *i.e.* deoxygenised, solution of nitrate of copper, the silver in an oxygenised solution; the two liquids communicated through the diaphragm of a divided cell. In half an hour the silver plate was covered with a reddish film, while not a trace of oxidation was perceptible on the copper. On continuing this experiment for three hours, it was found that the copper plate lost 0·003 grm., and the silver plate increased proportionately. On cleaning the plates and reversing their position, the copper was covered with a film of oxide, while the silver remained free from cupreous deposit. We believe, therefore, that through the simultaneous action of the two metals the dissolved salt is put into such a state of tension that oxygen brings about a chemical change which otherwise would be impossible, and that this change is initiated in close proximity to the more negative metal.

Though we have examined only this reaction, we have satisfied ourselves that it is not an isolated fact. Each of the elements concerned may be replaced by others: thus the sulphate may be substituted for the nitrate of copper, or platinum may be used instead of silver. Chlorine may take the place of oxygen with the production of the subchloride instead of the suboxide, and zinc may be employed as the positive metal with zinc chloride as the salt in solution, in which case copper may be taken as the negative metal, and on its surface will form a deposit of oxide of zinc.

Linnean Society, March 21 and April 4.—Mr. Bentham read the continuation and conclusion of his notes on Compositæ, comprising their History and Geographical Distribution. The ancient history of the order is more purely conjectural than that of many other large groups of plants. The geological record is remarkably scanty. The only remains that can be plausibly referred to Compositæ are the impressions of achenes with their pappus figured by Oswald Heer from the upper Miocene deposits of central Europe, which, supposing, as is probably the case, that the identifications are correct, would only show that at that tertiary epoch Compositæ existed in Europe of the same general character as those which are there now to be met with; and that they had thus already attained that highly differentiated character they now possess, and consequently must already have been of an old date. In the absence, therefore, of direct evidence, we are left to judge of the antiquity and origin of Compositæ in general, as well as of the subordinate races they comprise, from their comparative structure and geographical distribution. The paper then proceeds to pass in review in great detail the thirteen tribes of Compositæ, and the several subtribes and principal genera into which they are divided; after which some conjectures are put forward, as derived from the data thus supplied, as to the comparative antiquity of the principal races of Compositæ. Concurring with the arguments which have been brought forward by French and other botanists, to show that the great consolidation and uniform structure of the essential organs of fructification in Compositæ are evidences of their greatest perfection and consequent comparatively recent origin, it is shown that this consolidation and uniformity is least marked in Helianthoideæ, especially in the small subtribe of Petrobieæ, and most so in Cichoraceæ; and this conjecture that the former represent the most ancient, the latter the most recent, races of the order, is confirmed in some measure by the peculiarities of their respective geographical distribution. The study of the various details given would further lead to the supposition that the primitive form of Compositæ had regular gamopetalous flowers with an inferior ovary, the calyx, corolla and uniseriate stamens isomerous and probably pentamerous, the pistil bicarpellary, but the ovary already internally reduced to a single cell with a single erect anatropous ovule, and the seed exalbuminous, enclosed in an indehiscent pericarp, and containing a straight embryo with an inferior radicle; and that it is in the gradual course of subsequent consolidations that the bracts have crowded round the condensed flowers and usurped the functions of the calyx-limb, which has become obliterated or transformed so as to be better adapted to its new duties; the corollas have become contracted, or the outer ones variously developed in forms and colours adapted to assist in the process of cross-fertilisation; the anthers, brought into close contact by the compression of the flowers, have become united, and their styles modified so as to assist them in the discharge of their pollen, and the conversion from hermaphroditism to unisexuality may in various races have variously preceded or followed some or all of these changes, and produced those numerous diversities observed in the order. We might be further led to imagine that several of these changes had taken place at a very early period previously to the disruption of or stoppage of communication between the tropical regions of the globe, that, besides the parent forms above supposed to be represented in some Helianthoideæ, and perhaps a few Cotuleæ, Compositæ then existed, showing several important modifications, such as—(1) the regular and uniform tubular development of the corolla, accompanied by more or less of suppression of the inner bracts, and of the normal calyx-limb, with a substitution of a pappus in the latter case; (2) the reduction of the corolla limb, attended frequently by a sexual dimorphism and occasional oblique development of the outer female flower; and (3) perhaps at a later period, the uniform unilateral development of the whole of the corollas, accompanied usually by a suppression of the inner bracts and conversion of the calyx limb into a pappus. From the first of these modifications would have sprung the Eupatoriaceæ in America, the Vernoniaceæ in the New and the Old World, the Cynaroideæ in the northern, and the Mutisiaceæ in the southern hemisphere. From the second modification would have arisen—first, the more slightly altered Helianthoideæ chiefly in America; secondly, the Helenioideæ in America, and the Anthemideæ in the Old World, with the thinly paleaceous modification or total suppression of the inner bracts and calyx limb; and thirdly, the cosmopolitan Senecionideæ, Asteroideæ, and the majority of Inuloideæ, with an almost universal suppression of the inner bracts and conversion of the calyx limb into a setose pappus. The third general modification, with a very few slight exceptions, has settled down into those Cichoraceæ whose absolute uniformity had been already pointed out. In the third and concluding portion of the paper the present Regions, or chief centres, or areas, of the principal races of Compositæ are passed in review. The position of these great centres is evidently influenced by the prevalent constitution of the order, and the consequent effects of climatological and other physical causes on the gradual migrations of its species. Rarely arborescent and gregarious, still more rarely aquatic, Compositæ are, in a great measure, excluded from the vast forest-clad lowlands of the Amazon region of America, or of eastern tropical Asia, and the species are few in the swampy bogs of the northern hemisphere. Their favourite haunts are treeless or thinly-clad mountain regions, and especially the lower or broken grounds, rocky ridges, or open campos of warm extratropical or subtropical districts. They may be met with, it is true, at the highest altitudes or latitudes which will bear phænogamic vegetation as well as in the warmest tropical deserts, and a few species, as ready colonists, are perfectly ubiquitous in the traces of man; but there are large tracts of open country especially abounding in highly differentiated races of very limited areas, others again where Composite genera and species are as numerous and ill-defined in their subordinate races as wide and vague in their geographical range. These tracts of country severally constitute the centres of differentiation or areas of preservation, of which the definition is attempted as Regions of Compositæ. After alluding to the difficulties arising from the interchange of races across the frontiers of adjoining regions, or from the occasional reappearance of identical genera and species at enormous distances, as well as from our imperfect acquaintance with the Compositæ of certain districts, these regions are severally passed in review, in a series of tables of the genera they contain, either endemic or common to other regions, followed by such general observations as the comparisons may have suggested, commencing with the primary division into the New and the Old World, the former including the Sandwich as well as the more nearly placed Pacific Islands, whilst the Atlantic islands, Australia and New Zealand, are compris d in the Old World. After a general table of the genera of and estimated number of species in each division, a series of tables shows—(1) the connections between the tropical regions of the two divisions, as exemplified by identical genera; (2) the same connections in identical species; (3) the northern, and (4) the southern connections of the New and Old Worlds. Generally Compositæ are nearly equally divided between the two, about 430 genera in the New and 410 in the Old, with at least 4,700 species in the former, 4,400 in the latter; new discoveries being likely to add more to the latter. Of these numbers about 75 genera are common to the two divisions, but the identical species are under 70 out of at least 9,100. These common species are chiefly Arctic, or high northern, the tropical ones being very few and mostly very generally diffused, and ready colonists, such as *Eclipta alba*, *Ageratum conyzoides*, *Adenostemma viscosum*, *Siegesbeckia orientalis*, In the separate distribution of Compositæ in America and the Old World there is one striking difference in the two divisions with regard to the extratropical or subtropical races which form the great bulk of the order. In America the northern and southern tribes are the same, although in different proportions, and there are a considerable number of identical genera and even species in the north and in the south. In the Old World on the contrary two large northern tribes, Cynaroideæ and Cichoraceæ, are all but absent from the south, whilst the southern Aretotideæ, as well as several subtribes of other tribes, are wanting in the north. The genera common to the Mediterranean and South African regions (except such cosmopolitan genera as *Senecio*) are very few, and the common species scarcely any. This great difference in the two divisions of the globe may be due in a great measure to the direction of the great chain of mountains which in America, running north and south, facilitates or has facilitated means of intercommunication to races of the constitution of Compositæ, to which the east and west mountain ranges plains seas and deserts of the Old World only oppose obstacles. The regions of which the Compositæ are severally tabulated and commented upon are, in America: (1) the Mexican region including California, a portion of Western Texas and Central America north of Veraguas, remarkable for the large number of endemic genera, 135 out of 240, and the small average number of species; (2) the United States region, comprising the general area of North America from the Oregon and Texas eastward and northward, with about 118 genera, out of which only 25 are endemic, or nearly so, but the average number of species more than double that of the Mexican genera; (3) the West Indian region, of which the three principal islands, Cuba, St. Domingo, and Jamaica, have 13 endemic genera of one to three species each; and three South American regions, the Andine, the Brazilian, and the Chilian, not so distinct from each other, nor showing any such remarkable contrasts as the two northern ones. In the Old World six regions are distinguished—(1) the Mediterranean, extending from Spain to Affghanistan, with at least 140 genera, more than half of them endemic, and an average of nearly 10 species to a genus; (2) the great Europæo-Asiatic region, extending from Western Europe to Eastern Asia, with a large number of species, but only 10 endemic genera out of 87; (3) the Tropical African, with 18 endemic genera out of 109; (4) the Tropical Asiatic, with only 9 out of 78 endemic or nearly so; (5) the South African, the smallest in extent but the richest in endemic highly differentiated genera and species, 100 out of 148 genera being limited to that locality, and out of about 1,400 species not above a dozen common to other regions; and, lastly, (6) the Australian region, with 39 out of 83 genera endemic, and, notwithstanding its isolation, nearly 60 species common to other countries, chiefly tropical Asia and New Zealand. The Compositæ of the principal Oceanic islands are also separately tabulated and considered; after which, in the general summary, it is conjectured that Africa, Western America, and possibly Australia may have possessed the order at the earliest recognisable stage, Africa showing the greatest variety of individual isolated remnants of extinct races; Andine America, and some of the scattered Oceanic islands, exhibiting a few of what may be deemed the nearest approach to the previously mentioned conjectural primitive form of the order; that at this early period there must have been some means of reciprocal interchange of races between these regions; that since the disruption of this intercourse between the two great divisions of the globe, there must have been for a time a certain continuity of composite races from north to south across the tropics—a continuity which was probably further prolonged in America than in the Old World; that as Compositæ began to disappear from these tropical regions which thenceforth opposed to them impassable barriers, they became rapidly differentiated to the northward and southward, with greater structural divergences in the Old than in the New World, owing to the isolation being more complete in the former than in the latter; and that those forms, those more or less differentiated races, which had reached and accommodated themselves to high northern latitudes or mountain altitudes, retained some means of communication and interchange between the Old and the New World, long after it was broken off in the warmer parts of the globe. Finally the homes where Compositæ now flourish in the greatest luxuriance of specific variety and individual numbers, appear to be tropical America, exclusive of the great alluvial low grounds and forest regions, the United States, South Africa, the Mediterranean region, West Central Asia, and extra-tropical Australia.

Geologists' Association, April 7.—The Rev. J. Wiltshire, F.G.S., president, in the chair. "On the Excavations at the Site of the New Law Courts," by W. H. Hudleston, F.G.S., and F. G. H. Price. The authors commenced with a general description of the area in question, which faces the Strand for 500 feet, and is in shape a rough square of some seven acres in extent. The floor of the excavation is about 33 feet above ordnance datum line at the south-east corner. Four varieties of beds are recognised. (1) Brick rubbish, &c.; (2) gravels and sands; (3) red clay; (4) blue clay. The nature of the changes which the London clay undergoes in its upper portions was noticed, and the chemical agencies acting upon the clay and its included septaria pointed out. The changes from blue to red were thus summarised:—Conversion of dyad iron, existing partly as carbonate partly as as a basic element of the silicate, into tryad iron, oxidation of the included pyrites, removal to a considerable extent of the resulting sulphuric acid and diminution of the carbonate of lime and magnesia. The several sections carefully examined by the authors showed that on the north side the gravels have a thickness varying from 9 to 13 feet, and thin out and disappear before the Strand is reached. The contour of the London clay is irregular, one line of 30 yards giving a variation of 7 feet in the thickness of the overlying gravels, due to this cause. Deposits of oxide of manganese and sulphide of iron occur in the gravels; the former, it was contended, due to natural causes, while the latter was probably owing to sewage contamination. The bones of *Bos*, *Capra*, and *Equus*, were found in the gravels, and in the underlying clay twenty-three species of mollusca, including *Fusus bifasciatus* and *Pyrula smithii*, characteristic, in the opinion of Mr. C. Evans, of a line of the London clay 130 feet above the base. The gravels belong to the west London block of the Middle Level gravels, the ascertained thickness of which at various points was compared with the thickness of the Lower Level gravels at South Kensington, Battersea, and Westminster. These latter the authors concluded were double the thickness of the western block of the Middle Level gravels. In conclusion the authors drew attention to the results of the operations of the existing river, and several accurate measurements of the bed of the Thames were given in illustration.—Mr. E. Charlesworth brought before the notice of the Association some sharks' teeth from the Red Crag, having certain perforations which, should they be proved to be the result of human agency, would seem to carry the advent of man on the earth back to Pliocene times.

Society of Biblical Archæology, April 2.—Dr. Birch, F.S.A., president, in the chair. "Notice of a Curious Myth respecting the Birth of Sargina, from the Assyrian Tablets containing an account of his Life." By Henry Fox Talbot. In this paper Mr. Talbot showed that Sargina the First was a very ancient king of Babylonia. The date of his reign is uncertain, but it may be roughly estimated at fourteen or fifteen centuries before the Christian era. He was a legislator and a conqueror, and his arms appear to have reached the distant Mediterranean. He fixed his capital at Agani, in Babylonia, a city whose site has not yet been discovered. His history, like that of other ancient conquerors and legislators, has become partially involved in fable. An account of his birth and infancy, preserved on a tablet in the British Museum, offers a great similarity to that of the infancy of Moses, as related in the second chapter of Exodus. This account agrees very closely with the conduct of Sargina's mother as described by the Assyrian tablet. "In a secret place my mother had brought me forth. She placed me in an ark of bulrushes; with bitumen she closed up the door. She threw me into the river, which did not enter into the ark. The river bore me up, and brought me to the dwelling of a kind-hearted fisherman. He saved my life, and brought me up as his own son," &c. The inscription appears to have been a long one, but only a small portion of the beginning has been well preserved.—"On the Rise of Semitic Civilisation, chiefly considered upon Philological Evidence." By the Rev. A. H. Sayce. The author stated that comparative grammar has shown that the Semitic language belongs to a late period in the history of the development of speech, and presupposes a parent-language, possibly connected with the old Egyptian and the sub-Semitic dialects of North Africa. Many objections, however, lie against the biliteral theory, and most of the biliteral roots are probably of foreign origin. This is Accadian, also the source, it would seem, of the early Semitic traditions. Thus two at least of the rivers of Paradise are Babylonian, and the Sisuthrus of Berosus (the Biblical Noah), is the Accadian Susru or Na (Anu). Like the traditions, a large proportion of the words in the Semitic languages which express the objects of civilised life are borrowed from the Accadian—the ordinary terms for "city," "weighing," "measures," "ciphers," &c., come from this source. We are thus enabled to gauge the primitive civilisation of the Semitic nomads, and to determine that their home had no great rivers or mountains, like the deserts of Northern Arabia.

Paris

Academy of Sciences, April 1.—M. Serret presented a continuation of M. A. Mannheim's geometrical researches upon the contact of the third order of two surfaces.—A paper was read by M. C. Decharme on the spontaneous ascensional movement of liquids in capillary tubes. The author here stated as the result of his experiments that each liquid possesses a proper ascensional velocity, which he proposes to call its "capillary velocity," and he indicated the peculiarities presented by certain liquids as regards the relation between this velocity and the length of the column, &c. An aqueous solution of hydrochlorate of ammonia has the greatest capillary velocity, and next to it chloride of lithium; both these have a greater velocity than pure water.—A note by M. de la Rive on the theory of polar auroras was read; the author maintains the atmospheric nature of the phenomenon.—The second part of a paper by M. A. Crova on the phenomena of interference produced by parallel nets was read.—M. Faye gave a long account of an association recently founded in Italy under the title of "Societa dei Spettroscopisti Italiana," and also presented a memoir on the hypothesis of persistent winds on the sun.—In a second communication on the history of fermentation, M. E. Chevreul described in some detail the chemical labours of Stahl, and especially his theories of fermentation and combustion, which the author regarded as physical rather than chemical.—M. Joseph Boussingault presented a note on sorbite, a saccharine material allied to mannite, obtained from the juice of the berries of *Sorbus aucuparia*.—A note was read by M. A. Clermont, on some metallic trichloracetates, and M. Balard presented a note by M. E. Reboul on the identity of the brominated hydrobromate and hydriodate of propylene, with dihydrobromate and iodohydrobromate of allylene, and on the dihydrobromate of acetylene.—A note by M. Duval-Jouve, on the anatomy of the dissepiments presented by the leaves of certain species of *Juncus*, was communicated by M. Duchartre.

April 8.—M. Serret presented a note by M. E. Combescure on a peculiar system of equations with partial differences; and a paper entitled "Investigations upon substitutions," by M. C. Jordan, was read.—M. Le Verrier communicated two notes by M. Diamilla-Müller, one on terrestrial magnetism, the other on the cosmical origin of auroras. In the latter he claimed priority in having put forward the notion of these phenomena being due to solar influences.—M. J. Silbermann read a continuation of his memoir on the laws of atmospheric tides; and M. C. Sainte-Claire Deville communicated a note by M. O. Silvestri, giving a chemical and microscopic analysis of the sand-shower which fell in Sicily on March 9, 10, and 11 in the present year.—M. Chevreul read a second note on the crystallisation of barytic salts, the acids of which originate from the maceration of dead bodies.—A memoir on the alteration of the sulphurous waters of Eau-Bonnes in contact with a limited atmosphere, by the late M. Louis Martin, was read.—M. H. Sainte-Claire Deville presented notes by M. A. Ditte on the apparent volatilisation of selenium and tellurium, and on the dissociation of their hydrogenated compounds; by M. B. Renault, on the reducing properties of hydrogen and vapours of phosphorus, and on their application to the reproduction of drawings; by M. de Tommasi, on a compound of binoxide of chrome and potassic dichromate, kalichromic dichromate $[(CrO^2)^3 (CrO^3)^2 K^2O] H^2O$; and by M. L. Grandeau, on the function of the organic materials of the soil in the nutrition of plants.—M. Cahours presented a note by MM. S. Cloëz and E. Guignet on the chemical composition of the Chinese green (*lokao*).—An interesting note on the polymorphism of *Mucor mucedo*, by MM. P. Van Tieghem and G. Le Meunier, was communicated by M. Decaisne.—M. A. Vulpian read a memoir on the alteration of the muscles produced under the influence of traumatic or analogous lesions of the nerves, and on the trophical action of the nervous centres upon the muscular tissue; and M. Gauthier de Claubry presented some observations on M. Champouillon's recent remarks as to the rapid decomposition of the bodies of alcoholised subjects. He adduces facts which seem to show that the difference in the rate of putrefaction may be otherwise accounted for.—M. A. de Lapparent read a note on the date of the elevation of the district of Bray.

BOOKS RECEIVED

English.—History of the Birds of New Zealand, Part 1.: W. L. Buller (Van Voorst).—The Teeth, and How to save them: L. P. Meredith (W. Tegg).

Foreign.—Verhandlungen der k. k. zoologisch-botanischen Gesellschaft in Wien, Band 21.—Die Grundlagen der Vogelschutzgesetzes (Ritter v. Frauenfeld)—Die Pflege der Jungen bei Thieren (Ritter v. Frauenfeld).—Ueber die Weizenverwüsterin Chlorops tæniopus: Prof. Max Nowicki—La Photographie appliquée aux études géographiques: Jules Girard.—(Through Williams and Norgate.)—Die Metamorphose der Squilliden: Prof. C. Claus.

DIARY

THURSDAY, April 18.

Royal Society, at 8.30.—On the Connection between Explosions in Coal Mines and Weather: R. H. Scott, F.R.S., and W. Galloway.—On the Fossil Mammals of Australia. Part VII. Genus Phascolomys. Species exceeding the existing ones in size: Prof. Owen, F.R.S.

Royal Institution, at 3.—On Heat and Light: Prof. Tyndall, F.R.S.

Society of Antiquaries, at 8.30.—Test of Certain Centurial Stones: H. C. Coote.

Linnean Society, at 8.—On *Begoniella*, a new genus of Begoniaceæ: Prof. Oliver.—On three new genera of Malayan plants: Prof. Oliver.—On *Camellia scottiana* and *Ternstrœmia coriacea*: Prof. Dyer.

Chemical Society, at 8.—Notes from the Laboratory of the Andersonian University; On a Compound of Sodium and Glycerine; and On Benzylisocyanate and Isocyanurate: E. A. Letts.

FRIDAY, April 19.

Royal Institution, at 9.—On the Sulphurous Impurity in Coal Gas and the means of removing it: A. V. Harcourt, F.R.S.

SATURDAY, April 20.

Royal Institution, at 3.—The Star-Depths: R. A. Proctor.

Government School of Mines, at 8.—On Geology: Dr. Cobbold.

SUNDAY, April 21.

Sunday Lecture Society, at 4.—On the Hindûs—Ancient and Modern—their Manners, Customs, &c.: Dr. F. J. Mouat.

MONDAY, April 22.

Royal Geographical Society, at 8.30.—Letter from Dr. Kirk on the Movements of Dr. Livingstone.—On Recent Explorations of the North Polar Regions: Capt. Sherard Osborn, R.N.

Anthropological Institute, at 8.—On the Hair and some other peculiarities of Oceanic Races: Dr. J. B. Davis, F.R.S.—On the Hair of a Hindostanee: Dr. H. Blanc.—On the Descent of the Esquimaux: Dr. Rink.—Le Sette Communi: Dr. R. S. Charnock.

TUESDAY, April 23.

Royal Institution, at 3.—On Statistics, Social Science, and Political Economy: Dr. Guy.

Society of Antiquaries, at 2.—Anniversary Meeting.

WEDNESDAY, April 24.

Geological Society, at 8.—Notes on the Geology of the Colony of Queensland: R. Daintree: with Descriptions of the Fossils, by R. Etheridge, F.R.S.—Notes on Atolls or Lagoon Islands: S. J. Whitnell.

Society of Arts, at 8.—On Nuts; their Produce and Uses: P. L. Simmonds.

Royal Society of Literature, at 4.30.—Anniversary Meeting.

London Institution, at 12.—Anniversary Meeting.

THURSDAY, April 25.

Royal Society, at 8.30.

Royal Institution, at 3.—On Heat and Light: Prof. Tyndall, F.R.S.

London Institution, at 7.30.—Turner and Mulready: Dr. Liebreich.

CONTENTS

THURSDAY, APRIL 25, 1872

A PHYSICAL OBSERVATORY

AT the last meeting of the Astronomical Society, a paper was read by Lieut.-Colonel Strange on "The Insufficiency of existing National Observatories." The title is perhaps suggestive of an attack on Greenwich, but this idea the paper at once dispels, the Royal Observatory, and the administration of its eminent director, being spoken of throughout in terms of the strongest approval, in which all astronomers must join.

The aim of the writer was to show that, though Greenwich provides most efficiently and amply for the elder Astronomy, it is now time for us to consider whether her younger sister should not also be permanently provided for. When Greenwich was founded the Physics of Astronomy, which now attract so much attention, had no existence. This department of science is entirely of modern growth; but it has already attained such wide proportions and so deep a significance that it cannot any longer with propriety be left to the chance cultivation of individual zeal. In putting forward these ideas, Colonel Strange has only given expression to what has been for some time in the thoughts of every one interested in astronomy and its correlated sciences. He is right in pointing out to the Astronomical Society that in this direction its influence can and ought now to be exerted. And he gives two very cogent reasons why this should be done at once. First, that the system of photoheliography, which has for some years been carried on at Kew by the zeal of individuals, and partly maintained by private means, has now been brought to a close. Second, that the Royal Commission on Science being now at work on the question of the advancement of science, the present opportunity is very favourable for bringing this matter forcibly before Government through that body—an opportunity which will probably not recur in a generation.

The discussion on the paper, as might be expected, was prolonged and animated. The Astronomer Royal, who spoke several times, was doubtful whether the object for which such an observatory was sought to be founded was sufficiently "secular" to ensure success; but on its being urged with great force and truth by Mr. De La Rue and Captain Toynbee—both connected officially with the Meteorological Office—that the study of the sun, as had been insisted on by Colonel Strange in his paper, must greatly aid meteorological research, Mr. Airy candidly admitted that if that pretension can be made good, there will exist a claim on behalf of Meteorology for the establishment of a Physical Observatory, similar to, and as "secular" as, that on behalf of Navigation on which Greenwich was founded.

It is certainly a little disheartening to find a great leader in science insisting so much on direct utilitarianism as the sole basis of national science, and withholding his testimony to the enormous moral and intellectual benefits of philosophical research, and even omitting all consideration of the indirect material results which have invariably followed vigorous and systematic study of natural phenomena of whatever kind. The average Englishman is prone enough to hug what in his untaught stupidity he calls "practical ideas," and will not be improved by being told by one of the first of living philosophers that such ideas are the standard by which he should measure every proposal for advancement. But it is impossible to suppose that these are the ideas which the Astronomer Royal will on mature reflection apply to the question before us, when deliberately presented to him with a view to action.

It is to be hoped, indeed, that the late discussion will be followed by action. Our Royal Astronomical Society should be the acknowledged head of modern astronomical activity. It has higher functions to perform than those on which its energies have been rather too exclusively exercised—the reading, discussing, and publishing of detached dissertations. It should from time to time take stock of the territory it occupies, in order to see what encroachments need fencing off and what expansions are required. And, above all, it should constitute itself more than it does the guide and encouraging counsellor of the Government in matters which it must understand better than they. We hope to see it awake to its moral obligations in regard to the most important matter which has been so opportunely submitted to it. We do not hesitate to say that if by its interposition a well-equipped Observatory for Physical purposes should be established, this will be the greatest service it will have ever conferred on Astronomy, and not on Astronomy only, but on a vast sphere of scientific inquiry, not obviously, but still indisputably, connected therewith.

In Meteorology such an observatory would ultimately, if not immediately, create a revolution. Instead of the dreary columns of thermometer readings piled upon us by well-meaning but aimless industry, we shall see men of thought labouring to refer to the great source of all energy, the great maintainer of all harmony, the great exciter of all variation—to the sun itself—those phenomena, at present the most difficult in the universe to interpret, which hitherto it has been assumed that any one with 5*l.* to spend on "a complete set of meteorological instruments" can help to elucidate.

Should the want now spoken of be made apparent to those who can supply it, there will be several important preliminary questions to deal with, such as (1) What should be the scope of such an observatory? (2) Should it be engrafted on Greenwich, or be independent? (3) Should Meteorology and Magnetism be engrafted on it and severed from existing connections? (4) Should a system of sun observations—the primary, though, of course, not the sole object of such an observatory—be extended to India and other British possessions, so as to ensure that continuity of facts on which Messrs. De La Rue, Balfour Stewart, and Loewy have laid so much stress in their striking memoirs on Solar Physics recently communicated to the Royal Society?

LANKESTER'S PHYSIOLOGY

Practical Physiology; being a School Manual of Health, &c. By Edwin Lankester, M.D., LL.D., F.R.S. Fifth Edition. Pp. 152. (London: Hardwicke, 1872.)

THE new title adopted by Dr. Lankester for this little work is somewhat misleading. It has nothing to do with Practical or Experimental Physiology, the subject on which interest has lately so much revived in this country, and on which we hope before long to see a treatise by competent hands. Nor would it be fair to compare this "School Manual" with the admirable "Lessons in Elementary Physiology" of Prof. Huxley. The latter, though intended for boys' and girls' schools, is only of use in the few instances in which dissection and microscopic anatomy are taught; and its chief value is for University men who do not specially take up Biology, and as the best introduction to the subject for medical students. But Dr. Lankester addresses the wider circle of the general public. He shows in the Introduction how an elementary knowledge of the functions of the body and of the rules of health may be taught in primary schools; and proceeds to demonstrate the advantage of this knowledge to statesmen, clergymen, lawyers, architects, newspaper writers, common councilmen, and artisans. Perhaps the most important part of this introduction is that in which the author urges the importance of some knowledge of what is necessary to health for women in all stations of life. A skilful teacher would be able to teach girls of average intelligence a large part, and that the most valuable, of the contents of this Manual. They would probably learn it more readily than boys, and when all memory of the tissues and their names had passed away, it may be hoped that the dogmatic injunctions and prohibitions on food and air and drains and clothing would, at least in part, survive.

The first chapter contains a fair sketch of the constituents of the human body; the second deals with food, and gives sensible advice on many points; but here there are marks of imperfect adaptation of Liebig's theories to more recent facts. The third chapter, on Digestion, is also clear and practical. The next on the Circulation is too technical for the purpose of the book, and might, we think, be relieved of many anatomical terms. The two which follow on Respiration and the Skin, are chiefly sanitary, and might be read with advantage even by those ignorant of physiology. In the seventh chapter, on Movement, Dr. Lankester gives a very uncertain sound on the subject of boat-racing (pp. 76 and 77), in the former passage going so far as to assert that "in all gymnastic exercises competition in feats of strength should be avoided." The public have been already frightened as much as they are likely to be by certain letters on the dangers of boat-racing, which appear at intervals in the *Times* newspaper. It may be said of this, as of other athletic sports, that when competition is avoided gymnastics will cease to be practised. It is surely better to attempt wisely to regulate these contests than to condemn what are just as valuable or as injurious as competitive examinations in mental athletics.

The last two chapters of this manual, which deal with the difficult subjects of the nervous system and the senses, are pleasantly written, and give much useful information; but there are more errors here than in the rest of the book. Thus the decussation of nerve fibres is made to take place in the *corpus callosum*, the arachnoid is described as a "spongy membrane," and the pathology and causes of apoplexy given on the same page are not correct. Again, the physical cause of short sight is not the cornea being too rounded, but the whole eyeball being too long, and if the reader "looks into a living human eye, through the pupil," as directed in p. 104, he will be disappointed of the promised result. In these as in other particulars the work would have been better if the writer had taken more pains. Beside a number of curious misprints, there are several minor inaccuracies scattered through the book, which a competent physiologist would correct in looking through it. Only two lines of poetry from Shakspeare and Milton occur, and both are misquoted. Similar inaccuracies are to be found in the classification of the animal kingdom printed at the end of the volume, with which it appears to have no very close connection. The glossary, on the other hand, and the questions for examination, will probably be found of practical use. The tables of the ultimate and proximate constituents of the body, also given in the Appendix, are too exact to be correct, and the same may be said of that showing the daily supply and waste. Moreover, 12lb. of fat would make but a meagre man; and 31oz. of water is more than there is reason to suppose that the lungs excrete. The woodcuts which have been added to the present edition are taken from well-known, chiefly French, sources; they are roughly reproduced, but answer their purpose well enough.

In a future edition, which we hope will be called for, it would be well to restore the original title of the work, and correct some of the inaccuracies we have referred to. It might also be desirable to give fuller directions on the choice and preparation of food, and especially of the food suitable for infants and invalids. A chapter on the general management of a sick-room as to warming, ventilation (now often carried to injurious excess), feeding, disinfection, &c., would also be a valuable addition. A short and admirable pamphlet, issued a short time ago by Dr. Bridges ("A Catechism of Health, adapted for Primary Schools," 1870), contains just those points of sanitary knowledge which are most important, and Dr. Lankester's experience as a coroner would be of great service (as it has already been) in enabling him to enlarge upon these most pressing topics, and to illustrate them by well-chosen examples. P. S.

OUR BOOK SHELF

Jahrbuch der kaiserlich-königlichen geologischen Reichsanstalt, xxi. Band. Nro. 4; October, November, December. (Vienna, 1871.)

DR. NEUMAYR occupies the greater portion of this number of the "Jahrbuch" with the third part of his elaborate "Jurastudien." In this paper he describes what he calls "der penninische Klippenzug," a name derived from Penninberge, near Szczawnica, on the borders of Western Galicia and Hungary. The structure of this region is treated of at considerable extent. A long list of some two hundred and fifty papers, notices, &c., accompanies the memoir. Herr Franz Toula gives some account of the Randgebirges, near Karlsburg and Rodaun; and the work done in the Chemical Laboratory of the Institute is described by Karl Ritter v. Hauer. The mineralogical communications which accompany the "Jahrbuch" contain, amongst other papers, one by C. W. C. Fuchs, on the mechanical and chemical changes which lava undergoes in passing from the fluid to the solid state; and another by G. Tschermak on the problems of mineralogical chemistry. We have also descriptions of various minerals by Prof. Zirkel, Victor v. Lang, and Richard v. Drasche, and a number of miscellaneous "notices."

The Higher Ministry of Nature: viewed in the Light of Modern Science, and as an Aid to Advanced Christian Philosophy. By John R. Leifchild. (London: Hodder and Stoughton, 1872.)

MR. LEIFCHILD is already known as a careful writer on matters connected with economic geology; he now appears before the public in the avowed character of ambassador between the opposing forces of Theology and Science. This bulky volume of upwards of 500 pages appears to be a kind of commonplace-book of thoughts which have occurred to him in solitary wanderings; the title means to express that the author concerns himself with subjects higher than those which "subserve our present individual and collective interests." We must acknowledge that works of this kind, endeavouring to reconcile in detail the conflicting theories of theologians and men of science, are little to our taste; we suppose, however, they have their public; and in the case of the volume before us, the large type, wide margins, and handsome binding, are all in its favour. With this preliminary objection, that portion of Mr. Leifchild's work which comes within our scope—for the greater part does not—seems treated with considerable care and knowledge, and with a higher degree of impartiality than is usually to be met with in such works. The Darwinian doctrines of evolution and natural selection of course come in for some severe criticism; we are surprised that Mr. Leifchild should reiterate the superficial and often refuted objection that geology has not yet revealed a single fossil *in transitu* from one species to another, as if it were possible that geology should reveal anything but the successive connecting and connected links, which it has done, and is doing every day. Those who delight in speculations on the border-land between the natural and the supernatural will find much to interest them in the volume, and to such we commend it.

LETTERS TO THE EDITOR

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

Spectroscopic Nomenclature

YOUR columns were not long since opened to a discussion, rather long drawn out, on a point of nomenclature. They are now, as ever, open to all reasonable discussion on that most interesting aspect of Nature presented by the spectroscope. I cannot help thinking that some advance might be made if the faculties exhibited in the one were now brought to bear on the other. There seems to be a lamentable tendency in zealous but disorderly minds to pay as little attention as possible to those aids to reasoning—those signs of ideas, which ought to be current coin.

I do not in the least propose to myself to attempt to mount the breach just now. But I would fain challenge attention, and urge a fair amount of consideration, on some few points in which I have noticed very diverse methods of expressing the same thing. And in so doing I may find it necessary to give my voice in favour of one or the other. But it is not my object to advocate so much as to indicate.

Observations have recently been made of the sun during eclipse of a kind which, if not so novel as some think, is intensely interesting, and must be constantly referred to. I mean with a free prism. Now it occurs to me that it would be easy to reserve the spectroscope for that instrument which we have been accustomed to call such and to characterise these other observations as *prismatic*, as distinct from *spectroscopic*. It would then be known at the very outset that there was no slit. This would not prevent a juvenile disciple of Newton from repeating his prismatic examination of a chink, and getting his linear spectrum; it would only keep before him the origin and constitution of that spectrum in a way which the sole use of the spectroscope appears not to do. The prismatic and the spectroscopic methods of examining a luminous object are totally distinct. Thus the Poodocottah observations were of one kind, those at Dodabetta of the other; those at Bekul, of both. It is of no consequence, for this matter, where the prism is, it is the absence of the slit that makes the difference. Thus, for the purpose of illustration, I may allude to the planetary nebula seen *prismatically* unaffected in the midst of a star cluster turned into streaks. And the prominences seen in an open slit are to all intents seen prismatically. It is obvious that there is here a distinction of idea which may be advantageously fixed by a distinctive use of words. Let the spectroscope mind its own business, which is to make and examine linear spectra. The moment it ceases to do so it ceases to be a spectroscope.

This brings me to the next point. Since the prism does not require a slit,—on the contrary, is a very valuable tool, as we have seen, without,—it ought never to see *lines*, except as it sees other forms, *i.e.*, out lines. There is a confusion of ideas—rather, I should say, a contraction of ideas—in setting a prism to look for lines. It is the spectroscope which sees lines, the prism sees images, forms. It is an accident of the case if the form happens in any of its parts to be at the same time linear, and having its linear portion in a certain direction. Thus, when in a prismatic examination of the solar crescent immediately before eclipse, the cusps become linear—albeit curvilinear—there is a failure of grasp in speaking of the dark cusp-images as dark lines; or, at any rate, there is an opportunity lost of exemplifying the principle which pervades the whole of the phenomenon, and of fixing the prismatic idea.

The same kind of misuse of terms I have had occasion to point out on the occasion of the first prismatic examination of an eclipse, when what are now called, happily, zones, were unhappily and mistakenly called by the technical term "bands."

I now pass on to the confusion which exists in the nomenclature of lines. The subject fully treated would embrace the whole range of spectral analysis; but I must confine what I have to say to solar spectra.

In the early days of solar examination with the spectroscope, I made my venture, in the direction which I am now pursuing, and it failed. Ignorant that I was already distanced—no matter how or why—I suggested certain symbols for certain lines, foreseeing somewhat of what has come to pass. Aiming to avoid an affiliation which further knowledge might prove false, but admitting the great probability that the lines at C, F, 2796 (K) were really due to hydrogen, I would have called these solar bright lines α, β, γ, the hydrogen lines being already known as $H\alpha$, $H\beta$, $H\gamma$; that which is now variously called "D_3," "D^3," "near D," or sometimes plain "D," I would have had known, in the same category, as δ. And other Greek letters expressed, and would have sufficed to express, as many more as the memory would require to hold. The venture failed, as I say; and considering that no little confusion has resulted, I cannot help thinking it a pity that it did. Soon after appeared a work on spectrum analysis, in which $H\gamma$ is ignored, and the bright solar line which corresponds with 2796 (K) and with $H\gamma$ is persistently called and identified with G, to the great scandal of the ghost of Fraunhofer and (I doubt not) the living Plücker. The blunder has often been repeated since, indeed I have seen it in NATURE more than once in the last few days. If it was not to have a Greek letter, at least it had a better right to be known as "2796 (K)" than has the coronal line to be called "1474 (K)." Failing that, it has been paraphrased, the shortest form being "near G." Surely it is time this were put right.

And now we have "1474." No one knows what the true position of that line is. The line 1474 (K) is an iron line, and it is to the last degree improbable that the coronal line is identical with it. The misnomer has carried with it, naturally, the idea that the source is iron. As this is an improbability of a higher order still—because there is evidence against it in the absence of a few hundred other iron lines—a false idea is in process of being fixed.

And all this arises, and much more will follow, from the laziness of mind, if I may so call it without offence, which adopts a name belonging to something already, instead of first reserving judgment, and giving it an independent standing with a name of its own.

Then there is the confusion of idea, and uncertainty in understanding exactly what is intended in speaking of the extension of the spectrum, and of position in it, as right and left, or left and right, as the case may be; or the confusion is avoided by the precise but cumbersome reference to degree of refrangibility, This is quite unnecessary. This is so exact an analogy between the *degree* of refrangibility and the *degree* of heat that no one ought to experience the least difficulty in using the simple adjectives "higher" or "upper" and "lower" for the parts of the spectrum, and the simple prepositions "above" and "below," where required. There is no possibility of misconception, and no explanation is needed.

Probably we have got beyond the stage in which misconception is likely to arise from the careless use of words expressing continuity or otherwise in a spectrum; but I would suggest the word "diffuse" where it is not intended to express anything precise. Thus the coronal spectrum is diffuse until we know it to be solar. M. Janssen testifies to dark lines seen in the (diffuse) spectrum. J. HERSCHEL

Camp Nandair, Hyderabad, March 19

Turner's Vision

I HAVE been waiting since the appearance of a report of Dr. Liebreich's lecture in NATURE of March 21 expecting that an animated discussion would be provoked, affording me an opportunity of slipping in obscurely as a minor combatant, the subject being one on which I am but very indifferently qualified to speak, although thirteen years ago I did incidentally suggest an explanation of the peculiarities of Turner's later pictures which, simple as it is, still appears to me sufficient. On page 67 of "Through Norway with a Knapsack," published in 1859, speaking of some of the peculiar midnight sunset effects of the North, I said that "Turner, like an eagle, has dared to face the sun in his full glare, and to place him in the middle of his pictures, showing us how we see a landscape with sun-dazzled eyes, when everything is melted into a luminous chaos, and all the details blotted out with misty brightness."

In all these peculiar pictures that I have seen the sun is thus placed in the middle of the picture, and just sufficiently above the horizon (from about 10° to 20°, or at most 25°) to pour his rays about perpendicularly to the curvature of the eye-ball, when the face is in position to contemplate a landscape. I have frequently repeated the experiment of contemplating a landscape under such circumstances, and on every occasion of submitting to such torture have seen all the effects of even the most extravagant of Turner's later pictures, which are so well described by Dr. Liebreich. I have seen the "vertical streakiness, which is caused by every illuminated point having been changed into a vertical line," with an "elongation, generally speaking, in exact proportion to the brightness of the light," and that "there proceeds from the sun, in the centre of the picture, a vertical yellow streak." These appearances may arise from an affection of the crystalline lens of my eye similar to that attributed by Dr. Liebreich to Turner, or it may be due to something else much simpler, and which is more or less common to all human eyes. If the simpler explanation based upon normal conditions covers the facts, it certainly must be the more acceptable.

My explanation of the vertical streaks is this. When we thus look full faced at the sun, the dazzle produces slight inflammation or irritation, and a flow of tears. The liquid accumulates, and rests upon the lower eyelid, forming a little pool, the surface of which has a considerable vertical curvature, *i.e.* the lower part of the retained tear curves upwards from the surface of its base at the root of the lower eyelashes to its summit contact with the conjunctiva. Thus in a vertical direction it must act as a lens of very short focus, it must refract and converge the rays of light in a vertical plane, and thus produce a vertical magnifying effect, the definition of which will of course be very confused and obscure, on account of the irregular curvature, and the fact that the eye is focused to the distant objects. This want of directive focusing will limit the distortion to the bright objects whose vertically magnified images will be forced upon the attention.

To test this explanation let any one select a bright afternoon, and at about 6 P.M. or a little later, at this season, gaze sunward upon any landscape free from London smoke or other medium of solar obscuration. At first, if his eyes are not very sensitive, he will see a circular sun, but presently, as the tears accumulate, the vertical elongation of the sun and general "vertical streakiness" will appear. When I tried the experiment last week the sun appeared like a comet with a brilliant vertical conical tail, the point of which rested on the horizon. But I was then slightly troubled with what is called "a cold in the head," and my eyes watered very vigorously, and thus the conditions for producing fine Turneresque effects were highly favourable. On carefully drying my eyes these effects were, for a moment, considerably diminished.

I have adopted another method of testing this explanation. Having caused the eyes to become somewhat suffused, I bring the upper and lower eyelids so near together that the liquid shall occupy a sensible depth, *i.e.*, from the conjunctiva to the base of both upper and lower eyelashes, and by compression be bulged or curved outwards in the vertical direction. On looking through this tear-filled chink at a gaslight, the vertical elongation is remarkably displayed, and it extends upwards or downwards or both according to the position of the liquid. When looking at the sun and landscape with the eyes fully opened (which is very painful), the elongation is chiefly downwards, and obviously connected with the tear on the lower eyelid; but if the eyelids be nearly closed to diminish the intensity of the light, an upward elongation is also commonly visible.

The other phenomena represented by Turner are, I think, simply a faithful copying of the effects of glare and suffusion produced by painful sun-gazing and the looking at a landscape where the shadows are, so to speak, nowhere, or all behind one's back. W. MATTIEU WILLIAMS

The Adamites

As "M. A. I." prefers to keep his incognito, I shall not seek further to induce him to reveal himself. He has now, however, pointed out what he conceives to be errors in my paper, and I will reply to his criticism.

In the first place, as to the word *pi-ta*, I neither said nor inferred that the final syllable is not a suffix. My remark was that it retained a primitive root, *ta*, which is found also in the Semitic *'ata*, and I submit still that I am perfectly correct. The suffix *târ* in Sanskrit denotes nouns of agency, as Bopp shows in his "Comparative Grammar," and I am quite justified, when I find in various other languages a root word similar both in sound and sense, in inferring that the Sanskrit suffix was originally of the same character. I have hitherto been under the impression that comparative philology had established that suffixes were at one time independent words, but it appears that I am wrong. To show, however, that I have erred in good company, I would refer to Prof. Max Müller's "Stratification of Language" (p. 32), where it is said, "suffixes and affixes were all independent words, nominal, verbal, or pronominal; there is, in fact, nothing in language that is now empty, or dead, or formal, that was not originally full, and alive, and material." I must plead guilty of ignorance of "M. A. I.'s" scientific method.

As to *Taata*, when it is shown that *Tamata* or *Tangata* was the original form of the Polynesian deity's name, I shall be better able to reply to your correspondent's criticism. In any case, the final syllable is evidently the word denoting "spirit," and I see no difficulty in *Ta* becoming either *Tam* or *Tang* as the result of phonetic change. The mere fact that *Taata* and *Tiki* are different gods with different attributes really amounts to nothing, since such a division of personality and characteristics is a common fate of the divinities of heathen mythologies. I see no reason to change my opinion that the name of the Polynesian great ancestor has preserved the same primitive root as that which is to be found in the name of the first man, Adam, of the Semites, or rather of the *Akkad* forerunners.

While replying to "M. A. I.," it may be well to notice the criticism of his advocate, Mr. Jenkins, for whose explanation of the meaning of the word *Adam* I am much obliged, although, if he will take the trouble to read my paper, he will see that I was not ignorant of what he states. But the acceptance of the Hebrew meaning of the word as the original one does not lead me to place much reliance on Mr. Jenkins's judgment. If the Old Testament narrative proves anything beyond a knowledge of the tradition as to Adam, it is that the narrator was a bad philologist, and that finding the Hebrew word *adamah*, he forthwith inferred that the first man was made of ground-dust, which gave to him its red colour. For my part, I entirely ignore the authority on such a point of the Hebrew writer, and in justification I beg to refer to the statement made by the Rev. A. H. Sayce before the Society of Biblical Archæology, as reported in the last number of NATURE (p. 495), that the early Semitic traditions are derived from an Akkadian source, as are also most of the biliteral roots of the Semitic language. If the traditions are taken from that source, the probability is that the proper names they enshrine have had the same origin; and I submit, therefore, that I am quite justified in tracing the meaning of the word Adam to the old Chaldean tongue, in which, as Mr. Norris's Assyrian dictionary shows, and as my paper asserts, *Ad* signifies "a father."

In conclusion, I may add that there is nothing improbable to my mind in peoples even so distant from each other as the Polynesian Islanders and the Gauls retaining in their traditions a name which had been applied to their mythical common ancestor, nor unreasonable in supposing that they and other peoples mentioned in my paper were alike derived from some region in Central Asia. My argument is simply cumulative, as there are many facts of a different kind pointing in the same direction.

I am sorry my communication has reached such an inordinate length; but having replied to "M. A. I.'s" objections, which, after his first letter, forcibly remind me of the mountain in labour bringing forth a mouse, I shall not trouble you with further correspondence on a subject which I fear is far from interesting to a majority of your readers. C. STANILAND WAKE

Meteor

As I was going along the road towards Greystoke Castle at half-past eight P.M. on Friday last, April 19, I noticed a very fine meteor in a south-east direction. It was about the size of a common hand-ball, its centre being of an exceedingly brilliant white colour, surrounded by a circle of a bluish tinge, while short flickering radiations were distinctly visible on its circumference in all directions, reminding me of the sphero-stellate spiculæ of certain sponges. It was falling in a perpendicular direction, but I was not fortunate enough to see it at the beginning of its course. Its downward motion was slow and quite gradual, apparently not swifter than an ordinary india-rubber ball would fall by the gravity of its own body. There was no trail whatever left behind in its course. After two or three seconds it suddenly disappeared, before reaching the ground, without any explosion or expansion of its body. The night was very close and still, a muddiness covering the whole sky, interspersed here and there with long stratus clouds, and a beautiful halo surrounding the moon. THOMAS FAWCETT

Blencowe School, Cumberland, April 22

A Waterspout

ON Saturday last, April 16, whilst fishing in the river Elwy, at a point about two miles above the well-known Cefn caves, and five from St. Asaph by the river, I witnessed a very singular phenomenon. My attention was suddenly called up-stream by a remarkably strange hissing, bubbling sound, such as might be produced by plunging a mass of heated metal into water. On turning I beheld what I may call a diminutive waterspout in the centre of the stream, some forty paces from where I was standing. Its base, as well as I could observe, was a little more than two feet in diameter. The water curled up from the river in an unbroken cylindrical form to a height of about fifteen inches, rotating rapidly, then diverged as from a number of jets, being thrown off with considerable force to an additional elevation of six or seven feet, the spray falling all round as from an elaborately arranged fountain, covering a large area. It remained apparently in the same position for about forty seconds, then moved slowly in the direction of the right bank of the river, and was again drawn towards the centre, where it remained stationary as before for a few seconds. Again it moved in the former direction, gradually diminishing and losing force as it neared the bank, and finally collapsed in the shallow water. Strange to say, its course was perpendicular to the bank and not with the current.

At the time of the occurrence the river was still high, from the recent heavy rain, though the depth of water at the spot where I first observed it was not more than four feet. The current, of course, was stronger than usual, but presented a comparatively smooth surface. The day was fine and sunny, with a slight breeze from the S.E. The event occurred about 12.15, and lasted seventy or eighty seconds, as well as I could judge. The atmosphere in the immediate vicinity seemed, from the way in which the spray was scattered, to be somewhat agitated; but my impression was that such agitation was the result of the phenomenon, rather than its cause. I had fished over the spot a few minutes previously, and examined it afterwards with great care, but saw nothing to account for the wonder.

St. Beuno's College, St. Asaph, April 9. J. GRAY

Cuckoo's Eggs

THE discussion raised by Prof. Newton on the coloration of cuckoos' eggs has been very interesting doubtless to many readers of NATURE; a mite of information from New Zealand, concerning one species of the Cuculidæ, may not be out of place.

The German theory that "the egg of the cuckoo is approximately coloured and marked like those of the birds in whose nest it is deposited, that it may be less easily recognised by the foster parents as a substituted one," does *not* hold good in respect to our *Chrysococcyx lucidus*, Gml., pipiwharaupa, the whistler or small cuckoo.

The dupe is the piripiri, or gray warbler, *Gerygone flaviventris*, Gray, its eggs are white, dotted with red spots; the egg of the whistler of much larger size, is of a greenish dun.

However, I think it should be stated that the nest of the dupe is somewhat of a pear-shaped structure, firmly and thickly built, with a small entrance near the middle, well sheltered with feathers. Here discrimination betwixt eggs may be difficult for the foster parent, if it possesses the faculty and uses it. In the Trans. N. Z. Institute (vol. ii. pp. 58 and 65) reasons have been advanced by the writer for the selection of the warbler's nest by our brightly plumed cuckoo; may "the dim obscure" of its interior supply another reason? THOMAS H. POTTS

Ohinitahi, Feb. 5

Sun-spots and the Vine Crop

As the connection of sun-spots with terrestrial phenomena is now largely occupying the attention of scientific men, the following facts may be of some interest. The years in which the wine crop in Germany was unusually good seem (in this century, at least) to have returned at regular intervals. The close coincidence of these years with the years of minimum sun-spots is shown by the following table:—

Minimum of Sun-spots.		Wine-years.	Minimum of Sun-spots.		Wine-years.
1784·8	...	1784	1833·8	...	1834
1798·5	...	(?)	1844·0	...	1846
1810·5	...	1811	1856·2	...	1857, 1858
1823·2	...	1822	1867·2	...	1868

I may add that the gentleman who first remarked the regular recurrence of wine-years at intervals of about eleven years was not aware of the periodicity of the sun-spots, and could not therefore have been in any way prejudiced. The years given in the above table are the only ones known in Germany as good wine-years.

These facts agree with the results of Messrs. Piazzi Smyth and Stone, who found that the mean temperature on the surface of the earth was subjected to a period of eleven years.

ARTHUR SCHUSTER

Owens College, Manchester, April 23

Tide Gauge

IN NATURE of the 18th is a letter from Mr. Pearson respecting Tide Gauges. As very little appears to be known of such instruments, we beg to inform you that we have made them for many years, and have now two finished, one for the Indian Government, and the other for the Australian Government, and we shall be happy to show them to any one wishing to see them. We think they could be made self-acting at a much less cost if the *exact* time of high water is not required.

449, Strand, W.C., April 19 ELLIOTT BROTHERS

Colour of the Hydrogen Flame

IN a communication from my zealous science-master, which I find in your issue of Thursday the 11th, it is stated that pure hydrogen has *no* tinge of blue in its flame (that colour being due to the presence of sulphur), and he concludes his note with a gushing tribute of his own, and the younger boys' gratitude for the "simply delightful Science Primers of Profs. Huxley, Roscoe, and Balfour Stewart." Let me call his attention to the fact that on page 26 of his Chemistry Primer, Prof. Roscoe distinctly states that "Hydrogen is inflammable, and burns with a pale blue flame." A GRATEFUL PUPIL OF MR. BARRETT

The "Cheironectes pictus"

SINCE I communicated to you an account of a fish which I caught in the Gulf weed during the homeward voyage of H.M.S. *Charybdis*, I have seen, in the February number of the *American*

Journal of Science and Arts, a description of a fish-nest which Prof. Agassiz obtained from the seaweed of the Sargasso Sea in December last.

In this interesting paper Prof. Agassiz identifies the embryos which he acquired from the nest as the young of the *Cheironectes pictus*, which, as its name implies, has fins like hands. From the description given I have no doubt but that my specimen is the *Cheironectes*, and I lose no time in forwarding to you the result of my reading. J. E. MERYON

H.M.S. *Duke of Wellington*, Portsmouth

OCEAN CURRENTS

IN the *Philosophical Magazine* for October 1870 and 1871 I have examined at considerable length the arguments which have been advanced in favour of the theory that Oceanic Circulation is due to differences of specific gravity between the ocean in equatorial and polar regions. Since then a point in reference to the influence of the earth's rotation has suggested itself to my mind which appears to be wholly irreconcilable with the gravitation theory of currents.

It is one of the properties of a fluid that the resistance which it offers to motion is equal in all directions. It follows, therefore, that when an ocean current is flowing in any particular direction, the forces acting on the moving water must be greatest in the direction of motion. According to the theory that oceanic circulation is due to difference of specific gravity, resulting from the difference of temperature between the equatorial and polar waters, the direction of motion at the surface of the ocean is from the equator to the poles, and at the bottom from the poles to the equator, subject to a deflection caused by the earth's rotation. According to this theory gravity tends to impel the water from the equator towards the poles along the line of meridian; while rotation tends to deflect the water towards the east. If the total amount of work performed on the moving water by these two forces were equal, then the water on the northern hemisphere would take a north-easterly direction, and that on the southern hemisphere a south-easterly direction. But owing to the way in which the two forces vary in relation to each other, the path taken is not a straight line but a curve, the particular character of which has been determined with great labour by Mr. Ferrel.

But whatever view we may adopt in regard to the influence of rotation on the moving waters, whether it be that advocated by Dr. Colding and others, or that propounded by Mr. Ferrel, it is evident that if we assume the amount of the impelling energy of gravity to be not greater than the deflecting energy of rotation, we shall be led to the conclusions that there can be no such general interchange of equatorial and polar water in the Atlantic as Dr. Carpenter maintains. For under such conditions water leaving the equatorial regions for the Arctic seas would move as rapidly eastward as northward, and would consequently be deflected against the western coast of the old continent, and arrested in its progress before it reached even the latitude of England.

I need not, however, dwell further on this point, for I do not suppose there are any advocates of the gravitation theory who will not freely admit that the impelling energy is at least equal to the deflecting energy, and if this be admitted, it is all that is necessary for my present argument.

What proportion then does the impelling energy of gravity bear to the deflecting energy of rotation?

The velocity of rotation at the equator is about 1,526 feet per second, and at lat. 60°, about 773 feet per second. Were water frictionless, and did it offer no resistance to motion, then a pound of water flowing from the equator in the direction of the pole would, on arriving at latitude 60°, have, according to hitherto received ideas, an easterly velocity relative to the earth's surface of 763 feet per second. Mr. Ferrel has, however, shown that the relative velocity would be much greater. But not to run the risk of over-estimating the velocity, I shall be content to take it at 763 feet. Water flowing from the equator towards the poles, instead of having an actual velocity of 763 feet per second on reaching latitude 60°, has, at the utmost, a velocity not over one or two feet. If we suppose the velocity to be, say, 3 feet per second, then 760 feet per second of velocity derived from rotation is consumed by friction and other resistances in the passage of the water from the equator to that place. A pound of water moving with a velocity of 760 feet per second possesses in virtue of that velocity 9,025 foot-pounds of energy. This enormous amount of energy is all consumed, not in impelling the pound of water from the equator to latitude 60°, but in simply deflecting it to the east during its motion. Consequently 9,025 foot-pounds is the amount of energy required to perform the work of deflection. But since the resistance offered by a fluid to motion is equal in all directions, the resistance offered to the impelling force must be as great as that offered to the deflecting force. It is, I trust, admitted that in the passage of the pound of water from the equator to latitude 60°, the distance traversed by the water under the influence of the impelling force is as great as the distance traversed under the influence of the deflecting force, or, in other words, the distance from the equator to latitude 60°, measured along the meridian, is as great as the distance to which the water is deflected to the east during its passage. Then, if this be the case, 9,025 foot-pounds of energy of the impelling force must be also consumed in overcoming the resistance to the motion of the pound of water; that is, the impelling force requires to perform 9,025 foot-pounds of work before it can convey a pound of water from the equator to latitude 60°. Can gravitation, therefore, be the impelling force? Can gravity, according to Dr. Carpenter's theory, perform 9,025 foot-pounds of work on a pound of water in impelling it from the Equator to latitude 60°?

Taking Dr. Carpenter's own data as to the temperature of the ocean at the poles and equator, and the rate at which the temperature at the equator decreases from the surface downwards, I have shown* that 9 foot-pounds is the greatest amount of work which gravity can perform on a pound of water (placed under the most favourable circumstances) in carrying it from the equator to either pole. Assuming the slope from the equator to the poles to be uniform, 6 foot-pounds will be the total amount of work that gravity can perform upon a pound of water in its passage from the equator to lat. 60°. But this is only $\frac{1}{1500}$ part of the amount of energy required. Hence, if there is any circulation of water between the equatorial and polar regions, it must be produced by a cause 1,500 times more powerful than the one to which he appeals.

But in reality the amount of energy impelling the water must be far more than 1,500 times greater than what can be derived from gravity, for the water moves more in the direction of the impelling force than in the direction of the deflecting force, thus proving that the impelling force is greater than the deflecting force.

Although it will be admitted that the resistance offered by fluid friction is equal in all directions, yet it may be urged that, owing to the influence of the winds or some other cause or causes which I have not taken into account, the actual resistance to motion may be greater in some directions than others. This no doubt may be the case, but it cannot possibly affect the conclusion at which I have arrived, unless it be shown that the resistance to pole-ward motion is 1,500 times less than the resistance to eastward motion.

But these results are as conclusive against the theories of Maury, Colding, Ferrel, and in fact against every possible form of the gravitation theory, as against the theory of Dr. Carpenter. And I need hardly add that they are equally fatal to the theory that ocean currents are caused

* Phil. Mag., Oct. 1871.

by the heaping up of the water by the winds; for any amount of power which could possibly be derived from such a source must fall enormously short of that required.

It may be noticed that we have here a means of making a somewhat rough estimate of the absolute amount of resistance offered to oceanic circulation, a rather obscure point. It shows that the resistance to motions arising from friction is far greater than was hitherto supposed. The amount of the work of the resistance to a pound of water passing from the equator to lat. 60° cannot be less than twice 9,025 foot-pounds.

It follows also that if the resistance to motion in the waters of the ocean be as great as it has thus been proved to be, then there is no warrant for the generally received opinion that a force such as that of the winds acting on the surface of the ocean cannot produce motion extending to any considerable depth. For if the resistance to motion be as great as the foregoing consideration shows it to be, it is impossible that the upper layers of the ocean can be constantly pushed forward in one direction without dragging the underlying layers after them.

The inadequacies of the gravitation theory may be shown in an even still more striking manner. Conceive a column of water in any part of the ocean extending from the surface to the bottom. Suppose the column to be a foot square, and the depths of the ocean to be four miles. We have in this case a column a foot in thickness, and four miles in height measured from its base. According to Dr. Carpenter's theory, gravity tends to move the water forming the upper part of the column in the direction from the equator to the pole, and the water forming the under part from the pole to the equator. What then is the amount of force exerted by gravity on the entire column? In the next part of my paper on Ocean Currents in the *Philosophical Magazine* I shall demonstrate by an exceedingly simple and obvious method, that the total amount of force exerted by gravity on the whole mass of water constituting the column is only $\frac{9}{16}$ of a grain. That is, $\frac{9}{16}$ of a grain on 600 tons of water.

Edinburgh, April 15 JAMES CROLL

THE FOSSIL MAMMALS OF AUSTRALIA

THE substance of this communication was given orally at the meeting of the Royal Society, April 18, 1872.

Prof. Owen commenced by alluding to the series of fossils brought in 1836 by the then Surveyor-General of Australia, Sir Thomas Mitchell, from the bone caves discovered by him in Wellington Valley, New South Wales. The determination of these remains required study of the osteology and dentition of the existing marsupial animals, which formed the subject of papers in the "Transactions of the Zoological Society" (vol. ii., 1838, and vol. iii., 1845).

In these papers indications were given of a second species of living wombat, distinct from the type peculiar to Tasmania, such indications being yielded by a skull sent from Australia. In 1853 the author published, in his "Osteological Catalogue of the Museum of the College of Surgeons," the cranial characters of a third living species of *Phascolomys*, also from a skull, which, like that of the second species, was from the continent of Australia. These materials seemed to some naturalists inadequate for the acceptance of a *Phascolomys latifrons* and a *Phascolomys platyrhinus*, in addition to the first discovered Tasmanian *Phascolomys vombatus*; and Gould in the part published in 1855 of his great work, "The Mammals of Australia," containing the fine figure of that species, hesitated to admit more, although a drawing which he had received of the head of a wombat killed in South Australia "afforded good reason for concluding that the continental animal is really distinct." In 1859 this distinguished naturalist was able to publish in Part XI. of his work a figure of a wombat from the southern parts of the continent of Australia, which he recognised as distinct from the small wombat of Tasmania, and referred to the *Phascolomys latifrons*; it was, however, the larger bare-nosed species, *Phascolomys platyrhinus*.

In 1865 and 1866 specimens were received at the Zoological Gardens of London, of both the continental Australian wombats, which the able Prosector, Dr. Murie, showed to have respectively the cranial characters of *Phascolomys latifrons* and *Phasc. platyrhinus*. The *Ph. latifrons* had the nose or muzzle clothed with hair. This confirmation greatly encouraged the speaker in the investigation and comparison of the cranial and dental characters of the fossil remains of the genus; and in November 1871, he felt that he had grounds for submitting to the Royal Society such characters of four other species of wombat, not exceeding in size the largest of the existing kinds, which four species appeared to have become extinct on the continent of Australia. The differentiation of the actual platyrhine and latifront species from some of the extinct forms was not the less interesting and instructive; though it seemed small in degree, it was, however, definite, in comparison with other fossil remains which could not be distinguished from the existing *Phascolomys platyrhinus* and *Ph. latifrons*.

The determination of the species propounded on cranial and dental characters in the present paper was much easier and more decisive, by reason of the marked superiority of size of the fossils. These large and gigantic wombats were differentiated, not only by bulk, but by modifications of the skull and proportions of certain teeth, notably the incisors and premolars.

On these grounds the author characterises a *Phascolomys medius*, which, although markedly larger than *Phascolomys platyrhinus*, was intermediate in bulk between the two now known extremes of size in the genus. Next followed a *Phascolomys magnus*, and finally a *Phascolomys gigas*. Of the latter species a restoration was given in a diagram of the natural size, which was that of a tapir or small ox. The dental and certain cranial characters were illustrated by highly finished drawings of the fossils.

With respect to the large extinct wombats described in his present paper, the author remarked that it was not likely they could have escaped detection if still existing in any of the explored parts of the Australian Continent. The knowledge that such species have existed may excite to research and help to their discovery, if any of them should still be in life, in the vast tracts of the northern and warmer latitudes of Australia.

The author exhibited in a tabular view the localities of the known existing and extinct Australian wombats as follows:—

Where found	By whom found	Species of Phascolomys.
Breccia Cave, Wellington Valley, N. S. Wales	Sir Thomas Mitchell, C.B., 1836	*Mitchelli*
Lacustrine Bed, Victoria	E. C. Hobson, M.D., 1845...	*Gigas*
Drift Deposits, Queensland ...	Geo. Bennett, M.D., F.L.S., 1861	*Mitchelli*
Ib. King's Creek, Darling Downs	S. Turner, 1847 ...	*Parvus, Medius*
Ib. Gowrie, *Ib.*	Fred. Neville Isaac, 1861	*Mitchelli*
Ib. Eton Vale, *Ib.*...	Ed. S. Hill, 1865 ...	*Platyrhinus, Medius, Magnus, Gigas*
Ib St. Jean Station, *Ib.*	M. Satche St. Jean, 1864	*Gigas*
Ib. Drayton, Queensland	Sir Danl. Cooper, Bt., 1865	*Thomsoni, Medius Magnus, Gigas*
Freshwater Beds, Clifton Plains, *Ib.*...	F. Nicholson, 1866 ...	*Gigas*
Caves, Wellington Valley, N.S. Wales	Professor Thomson, G. Krefft, 1867	*Mitchelli, Kreffti, Latifrons*

The author then touched upon some generalisations suggested by the present stage of discovery. The disappearance of the larger species was explicable on the principle of the "contest of existence," as applied by him to the problem of the extinction of the fossil birds of

* "On the Fossil Mammals of Australia," No. VIII.: Genus *Phascolomys*; species exceeding the present in size, by Prof. Owen, F.R.S.

New Zealand (Trans. Zool. Soc., vol. iv., 1850), and subsequently by Darwin to the incoming of new species, as "the battle of life." He next entered upon the relation of the present discoveries in Australia to the law of Geographical Distribution in the new Tertiary or Quaternary periods of extinct and existing animals.

The wombat was a more characteristic Australian form of mammal than the kangaroo, for the latter is represented by species in New Guinea; and species of *Phalanger* range farther from Australia, though still bound to the same great natural, and mainly submerged, division of the earth's surface. But no kind of wombat, recent or fossil, has been detected out of Australia and Tasmania. The present Continental kinds, and species near akin to them, existed in Australia during a very long period, reckoned by the terms of historical time, if we may judge from the state of petrifaction of the fossils, and the great depths at which some have been met with in well-digging; where, after 30 ft. or 40 ft. of black rich soil have been bored through, such fossils occur at 100 ft. lower down in sandy drift, which has been accumulated to that or greater vertical thickness beneath the loam. On the assumption that air-breathing animals perished in a general deluge some 5,000 years ago, and that their dispersion then began anew from the exceptional few individuals preserved in the Ark, we must suppose the wombats then living in Australia to have contributed miraculously their pair or pairs to the Asiatic menagerie, and to have been as miraculously restored to their proper continent on the subsidence of the Noachian flood.

It is neither creditable nor excusable that so great a divergence should still be maintained, chiefly through theological teaching, in the ideas of the majority of men "of ordinary culture" as to the cause and conditions of the distribution of living species over the globe, from those suggested by the clear and multiplied demonstrations of Science. On this topic the author referred to a paper in "Annals and Magazine of Natural History," 1850, "On the Gigantic Birds of New Zealand, and on the Geographical Distribution of Animals."

THE CONNECTION BETWEEN COLLIERY EXPLOSIONS AND WEATHER*

AFTER a preliminary reference to previous papers on the subject, and especially to the diagrams published by Mr. Joseph Dickinson, and by Mr. Bunning, of Newcastle-on-Tyne, the authors of the paper referred specially to Mr. Dobson's paper, published in the reports of the British Association. They showed that the periodicity alleged by him to exist in these explosions had no real foundation in fact; for, on plotting the dates of the explosions for the last twenty years in two ten-year periods, very slight resemblance was seen between the two curves. The number of accidents (all fatal ones) on which the statement was based was 1,369.

In the progress of this inquiry it had come out that the number of serious accidents, involving the loss of ten lives or more, had materially increased during the last five years, the numbers being:—

1851-55	13.	1856-60	15.
1861-65	12.	1866-70	21.

These numbers appear to be well worthy of remark.

For the special purpose of the paper, the continuous records from Stonyhurst, one of the observatories in connection with the Meteorological Office, were taken, and the curves for the barometer and thermometer were plotted for the three years, 1868-70. The records of fatal explosions were obtained from the published reports of the inspectors, while the dates of the non-fatal accidents were obtained from the inspectors themselves, who, almost without exception, replied to the communications addressed to them, and furnished the desired information.

Mr. Dobson, in his paper, having spoken of the explosions occurring principally at the commencement of a storm, the authors showed that it was not, in some cases, until two or three days after the barometer had reached its lowest point that the accident happened. They showed also why, during a period of continued violent oscillation of the barometer, the passage of each successive barometrical minimum is not characterised by an equal number of explosions, the largest groups of accidents being reported when a serious break occurred after a period of calm weather.

The effect of a high temperature of the air in interfering with ventilation, and especially with natural ventilation, was also explained, and it was shown how the first hot days in spring were marked by explosions.

The actual dates of the explosions for the three years in question were then compared with the meteorological records, and it was shown that out of 550 explosions—

266, or 48 per cent., might be attributed to the state of the barometer;
123, „ 22 „ to the state of the thermometer;
161, „ 30 „ remained unaccounted for on meteorological grounds.

The next point touched upon in the paper was the action of a more or less impure ventilating current in increasing the explosive character of the air in all parts of the pit, and possibly in causing an explosion in a place which would have remained safe had the ventilating current itself remained pure. It was shown how, when an explosive mixture had been formed in places and under conditions similar to those described, some time, possibly several days, must elapse before the contents of such an accumulation of dangerous gases shall have been rendered innocuous again.

The effect of warm weather in stopping natural ventilation was explained. The natural temperature of a mine of the depth of 50 fathoms being 55°, that of one of the depth of 200 fathoms 70°, and so on (speaking generally), it was shown that if the temperature of the air rose to 55° natural ventilation must cease in shallow pits, and similarly in other cases. Accordingly, if a warm day occurs in the cold season of the year, and the furnaces are not in action, an explosion is very likely to occur.

These statements were illustrated by one instance of a fatal explosion, the cause of which had been declared by the inspector to be inexplicable, the pit having "strong natural ventilation." It appeared, however, that the explosion occurred on a warm day, while the inspector visited it twice on colder days after the explosion; so that the state of ventilation which he witnessed had no reference to that which must have prevailed when the accident happened.

The paper concluded by stating that it appeared that the evidence fairly justified the view that meteorological changes are the proximate causes of most of the accidents, it being remembered, as has before been observed, that the records contain no account of the number of times when the pits have been too dangerous for the men to go down, and so explosions have not happened.

Whatever be the meteorological changes, it is absolutely necessary to keep a most careful watch over the amount of air passing through the workings.

Thirty years ago George Stephenson said, in a letter to the South Shields Committee, referring to explosions:—"Generally speaking, there has been some fault in the ventilation of the mines when accidents have occurred;" and the same opinion is held by many of the most experienced authorities at the present day. In this matter the one cry, whether we look to security against explosion, or to the affording to miners an atmosphere which they can breathe without injury to health, is "More air!"

* "On the Connection between Colliery Explosions and Weather," by Robert H. Scott, F.R.S., and Mr. W. Galloway. Read at the meeting of the Royal Society, April 18, 1872.

THE TEMPERATURE OF THE SURFACE OF THE SUN

IT will be recollected that Messrs. M. E. Vicaire and Sainte-Claire Deville read some papers before the Academy of Sciences at Paris last January, showing that the temperature of the solar surface does not exceed that produced by the combustion of organic substances. Their reasoning being based on the law of radiant heat established by the investigations of Dulong and Petit. I have in the meantime instituted a series of experiments on a comparatively large scale, in order to test the correctness of the said law. Accordingly, the dynamic energy developed by the radiation of a mass of fused iron weighing 7,000 pounds raised by "overheating" in the furnace to a temperature of 3,000° F., has been carefully measured.

Sir Isaac Newton assumed that the quantity of heat lost or gained by a body in a given time is proportional to the difference between its temperature and that of the surrounding medium. Some eminent scientists, however, accepting Dulong's conclusions and formula, assert positively that the stated assumption is incorrect. In so doing they apparently overlook the conditions inseparable from the Newtonian doctrine, namely, that the conducting power of the radiating body should be perfect; that at every instant the temperature pervading the interior mass should be transmitted to the surface.* It needs no demonstration to prove that if the conducting power of a body be so perfect that the temperature of the centre is at all times the same as that of the surface; in other words, that the fall of temperature at the centre, occasioned by radiation, is as rapid as the fall of temperature at the surface, the rate of cooling of such a body will be very different from that observed by Dulong and Petit. The investigation instituted by those experimentalists has in reality established only the degree of conductivity of the radiators employed, under certain conditions, but by no means their true radiant energy at given temperatures. M. E. Vicaire and Sainte-Claire Deville, therefore, commit a serious mistake in assuming that the *quantity* of heat transmitted by the radiation of incandescent bodies at high temperatures has been determined. It may be observed that the relation between the time of cooling and the *quantity* of heat transmitted by radiation which Dulong and Petit established, also misled Pouillet regarding the temperature of the solar surface, which he computed at 1,461° C., or at most 1,761° C. It will be well to bear in mind that Pouillet had himself ascertained with considerable accuracy the temperature produced by solar radiation on the surface of the earth; and also the retardation suffered during the passage of the rays through the terrestrial atmosphere. He was therefore able to demonstrate that the dynamic energy developed by solar heat amounts to nearly 300,000 thermal units per minute for each square foot of the surface of the sun. Considering the imperfect means employed by Pouillet, his "pyrheliometre," the exactness of his determination of solar energy is remarkable. The truth is, however, that the near approach to exactness was somewhat fortuitous, the eminent physicist having underrated the energy of radiant heat on the surface of the earth, while proportionately over-estimating the retarding influence of the terrestrial atmosphere. The true dynamic energy developed by radiation at the surface of the sun, exclusive of the absorption of the solar atmosphere—no doubt exceedingly small—determined by the solar calorimeter mentioned in a previous article, is 312,500 thermal units per minute upon an area of one square foot. It will be proper to notice that this amount is not a mean result of a number of observations, but the greatest energy developed at any time during observations continued upwards of three years, namely February 28, 1871. It will be proper to add that this result has been withheld from publication until it could be verified by a second observation indicating an equal energy. Fortunately the sky at noon, March 7, 1872, proved to be as clear as on the previous occasion referred to, the indicated energy differing only a few hundred units from that developed February 28, 1871.

Temperature being a true index of molecular and mechanical energy, conclusively established by the exact relation between the degree of heat and the expansive force of permanent gases under constant volume, it is surprising that Pouillet did not perceive that an intensity of 1,461° C. or 1,761° C., could not possibly develop on a single square foot of surface the enormous energy represented by 300,000 thermal units per minute. M. Vicaire, adopting like Pouillet Dulong's formula, states in the paper presented to the French Academy that "an increase of 600° is sufficient to increase the radiation a hundred fold;" and that Pouillet has verified Dulong's law to more than 1,000°. "Supposing," he observes, "that beyond this temperature the law ceases to be true, it cannot be absolutely remote from the truth for the temperatures of from 1,400° to 1,500° which we deduce by adopting the law." Sainte-Claire Deville concludes his essay on solar temperature thus:—"In accordance with my first estimate I believe that this temperature will not be found far removed from 2,500° to 2,800°, the numbers which result from the experiments of M. Bunsen, and those published long ago by M. Debray and myself." The French *savans* then agree that the temperature of the surface of the sun does not exceed the intensity produced by the combustion of organic substances, their grounds for this assumption being, as we have seen, Dulong's formula relating to the velocity of cooling at high temperatures. But Dulong and Petit did not carry their investigations practically beyond the temperature of boiling mercury; hence their formula relating to high temperatures is mere theory, the soundness of which we have now been enabled to test most effectually by measuring the radiant power of a mass of fused metal raised to a temperature of 3,000° F., 30 inches in depth, presenting an area of 900 square inches.

Before describing the means which have been employed in measuring its radiant power, let us briefly consider the condition of the fused mass during the experiments. In the first place, the temperature has been sufficiently high to produce an intense white light, luminous rays of great brilliancy being emitted by the radiant surface during the trial; (2) the bulk of the fused mass being adequate, the intensity of radiation has been sustained without appreciable diminution during the time required for observation; (3) the temperature being higher than that which the French investigations assign to the surface of the sun, while the bulk, as stated, is sufficient to maintain the temperature of the fused mass, it may be reasonably asked, why an area of one square foot of our experimental radiator should not emit as much heat in a given time as an equal area on the solar surface, if its temperature be that assumed by Pouillet? It may be positively asserted, moreover, that an increase of the dimensions of our radiator to any extent, laterally or vertically, could not augment the intensity or the dynamic energy developed by a given area. Again, Dulong's formula, as applied by scientists, shows that the emissive power of a *metallic* radiator raised to a temperature of 3,000°, reaches the enormous solar emission computed by Pouillet.

Let us now briefly examine the calorimeter constructed

* The writer has just completed a set of experiments with a spherical radiator, 2·75 in. in diameter, composed of very thin hammered copper, charged with water kept in motion by a wheel applied within the sphere, revolving at a rate of 30 turns per minute, the centrifugal action of which brings the particles of the central portion of the fluid so rapidly in contact with the thin spherical shell, that the apparently absurd condition of perfect conductivity has been practically fulfilled. The result of carefully conducted experiments with this radiator, enclosed in an exhausted vessel kept at a constant temperature, has established that Newton's law relating to radiant heat, up to a differential temperature of 100° Fahr. (beyond which the investigation has not extended), is rigorously correct. The subject will be fully discussed in a future article.

for ascertaining the mechanical energy developed by the radiation of the fused mass under consideration. Fig. 1 represents a vertical section, and Fig. 2 a perspective view. *a* is a cylindrical boiler, having a flat bottom, composed of thin sheet-iron 0·012 inch thick, coated with lamp-black. The cylindrical part of this boiler is surrounded by a concentric casing *b*, the intervening space being filled with a fire-proof non-conducting substance. A horizontal wheel, *c c*, provided with six radial paddles, is applied within the boiler, attached to a vertical axle, *d*. An open cylindrical trunk, *g*, is secured to the perforated disc which supports the paddles. The vertical axle passes through the top of the boiler, a conical pinion being secured to its upper termination. By means of a vertical cog-wheel, *h*, attached to the horizontal axle *k*, and geared into the conical pinion, rotary motion is communicated to the paddles. The centrifugal action of the latter will obviously cause a rapid and uniform circulation of the water contained in the boiler—indispensable to prevent the intense radiant heat from burning the bottom. The boiler and mechanism thus described are secured to a raft, *l l*, composed of fire-bricks floating on the top of the fluid metal. By this means it has been found practicable to keep the bottom of the boiler at a given distance, very near the surface of the fused mass, while by moving the raft from point to point, during the observation, irregular heating resulting from the reduction of temperature of the surface of the metal, under the bottom of the calorimeter, has been prevented. The radiant heat being too intense to admit of the axle *k* being turned directly by hand, an intervening shaft, eight feet long, provided with a crank handle at the outer end, has been employed for keeping up the rotation of the paddle-wheel during the trial. It is scarcely necessary to observe that, the intervening shaft should be coupled to the gear work by means of a "universal joint," to admit of the necessary movement of the raft. The experiment, repeated several times, has been conducted in accordance with the following explanation. The boiler being charged,

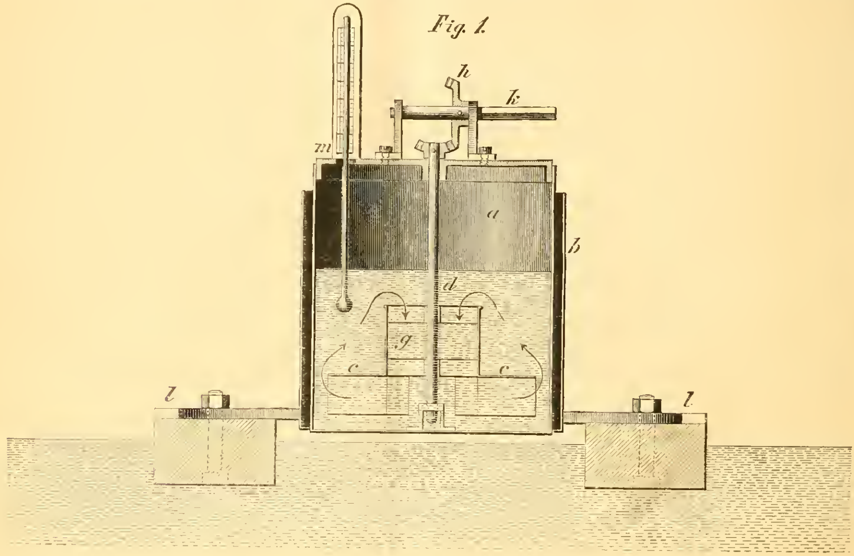


Fig. 1.

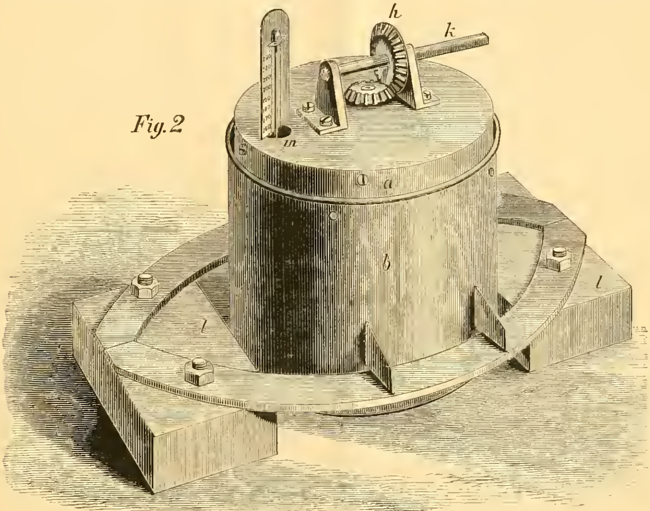
the paddle wheel should be turned at a moderate speed while observing the temperature of the water, the thermometer employed for this purpose being introduced through an opening, *m*, at the top of the boiler. The temperature being ascertained, the instrument should be quickly placed on the raft, and the time noted. As soon as vapour is observed to escape through the opening at *m*, the instrument must be instantly removed, the time again noted, and the temperature of the water within the boiler ascertained. It will be well to keep the paddle-wheel in motion until the last observation has been concluded.

The temperature of the fused metal having been as high during our experiments as that of the solar surface computed by Pouillet and his followers, while the thin substance composing the bottom of the calorimeter has been brought almost in contact with, and consequently received the entire energy transmitted by, the radiant surface, the reader will be anxious to learn what amount of dynamic energy has been communicated in a given time, on a given area. The desired information is contained in the following brief statement:—The necessary corrections being made for heat absorbed by the materials composing the paddle-wheel, &c., the instituted test shows that the temperature of a quantity of water weighing 10 pounds avoirdupois has been elevated 121° F. in 164 seconds (2·73 minutes), the area exposed to the radiant heat being 63 square inches. Hence a dynamic energy $\frac{10 \times 121}{2 \cdot 73} \times \frac{144}{63} = 1013$ thermal units per minute, has been developed by the radiation from one square foot of the surface of the fused metal maintained at 3,000° F., against 312,500 units developed by the radiation of one square foot of the solar surface, the temperature of which, agreeably to the calculations of the French *savans*, is less than that of our experimental radiator.

Having thus ascertained practically the amount of dynamic energy developed by the radiation of a metallic body raised to the high temperature of 3,000°, we have only to show in a similar manner the amount of energy developed by a metallic radiator of a low temperature, to be enabled to demonstrate the correctness or fallacy of Dulong's formula. Numerous experiments have been made for this purpose with apparatus of different forms, the results having proved substantially alike. The device most readily described consists of a spherical vessel charged with water, suspended within an exhausted spherical enclosure kept at a constant temperature. Repeated trials show that, when the differential temperature is 65°, the enclosure being maintained at 60°, while the sphere is 125°, the dynamic energy transmitted to the enclosure by a sphere the convex area of which is one square foot, amounts to 5·22 thermal units per minute. The accuracy of this determination is confirmed by the fact that during the summer solstice at noon, when the sun's differential radiant intensity is 65°, the solar calorimeter indicates a dynamic energy of 5·12 units per minute on one square foot of surface.

Our practical investigations, then, show that a differential temperature of 3,000° developes by radiation a dynamic energy of 1,013 thermal units per minute upon an area of one square foot; and that a differential temperature of 65° develops 5·22 units per minute upon an equal area. The ratio of radiant energy at the first mentioned intensity will therefore amount to $\frac{1013}{3000} = 0·337$ units for each degree of differential temperature; while for the low intensity it will be $\frac{5·22}{65} = 0·080$ unit for each degree of differential temperature. Consequently, the ratio o the radiating energy will be $\frac{0·337}{0·080} = 4·21$ times greater at 3,000° than at 65°. Now, M. Vicaire, on the authority of Dulong, states that the ratio will be a hundred fold greater for an increase of only 600°. According to Newton's theory, based on dynamic laws, the proportion between the differential temperature and the radiant energy of bodies is constant; while Dulong and Petit, basing their conclusions upon an erroneous estimate of the time of cooling, assert that the ratio of energy increases several thousand times when the temperature is increased from 65° to 3,000°. Newton, then, as our experiments prove, is incomparably nearer the truth than the French experimenters; and possibly future research will prove that his law, when properly applied, will be found absolutely correct. It should be mentioned that the result of our experiments with the fused metal, compared with the result of other experiments with solid metals at various temperatures, show that the emissive power of cast iron is relatively greater in a state of fusion than when solid, or merely incandescent. This observed increase of emissive power, now being thoroughly investigated, will no doubt account for the deviation from the Newtonian law indicated by the preceding comparison, which, let us recollect, is based upon the difference of radiant energy of fused metal at 3,000°, and solid metal at 65°. Considering this extreme range of temperature, and the totally different conditions of the radiators, the observed discrepancy is not too great to admit of satisfactory explanation.

Fig. 2



The fallacy of Dulong's formula relating to high temperatures having been conclusively shown, it will not be necessary to examine the calculations of Messrs. M. E. Vicaire and Sainte-Claire Deville, presented to the Academy of Sciences at Paris. Besides, the question of solar temperature cannot be properly investigated without considering the leading points connected with the propagation of radiant heat through space—a subject of too wide a range to be discussed in this article. It should, however, be mentioned that the result of the measurement of solar intensity March 7, 1872, before referred to, proves the correctness of our previous demonstrations, showing that the temperature of the surface of the sun is at least 4,036,000 F.

J. ERICSSON

THE CYCLONE IN THE WEST INDIES

A CORRESPONDENT in your number of October 12, 1871, expresses a wish for an article to appear in your paper, on the Cyclone which passed over Antigua, and several other of the Leeward Islands in the West

Indies, on the 21st of August last. If no other better qualified person has complied with that wish, I beg leave to tender the following account.

Perhaps a few preliminary observations in reference to the working of the barometer in these parts of the Tropics are necessary. A well-regulated mercurial barometer, at or about the sea level, under all ordinary conditions of the atmosphere, with the trades blowing from the east, stands at 30·00 or 30·10. A south-east wind, and the approach of heavy rains, will cause the barometer to fall, at times, to 29·80. At other times a N.E. trade wind, if not a storm wind—though it may bring occasional heavy showers—will cause the barometer to rise to 30·30. Thus the range of the mercury in these islands, when no cyclone is passing, is limited to five-tenths of an inch; but the variation seldom exceeds three-tenths. The atmospheric tide (if I may so call it), which causes the barometer to rise and fall half-a-tenth twice in the twenty-four hours, is very distinctly marked in these islands. The barometer being the highest at 10 A.M. and 10 P.M., and the lowest at 4 A.M. and 4 P.M. Any variation from this rule during the hurricane season calls for vigilance.

The following observations of the movements of the barometer during the late cyclone were taken at an elevation of about twenty-five feet above the sea-level.

The hurricane season of this year was preceded by a long dry season, and though the months of June and July were very hot, and sometimes oppressive, we had very little thunder and lightning. During the month of July we had some very squally weather, but the barometer was not much influenced by it. During the latter part of July and the first weeks of August, the wind often shifted towards the north, which is quite unusual at that time of the year, the barometer at the same time falling below 30·00. These indications caused some anxiety in the minds of those who were accustomed to observe the state of the weather.

The first indications of the approaching storm were noticed at 10 o'clock on the morning of the 20th. A light, but unsteady, wind was blowing at the time from E.N.E., the barometer had not risen after 4 A.M. as usual, and though standing at 30·00, the surface of the mercury was concave, indicating a fall. During the day the wind continued to blow moderately, but in gusts; the barometer slowly falling. Between 4 and 5 o'clock P.M. there was a heavy squall of wind and rain from N. by E., followed by a comparative calm. The appearance of the sky at sunset was most remarkable and alarming to those who understood anything of the indications of an approaching storm. A pale, sickly light, of a coppery hue, was spread over every object, and continued some time after sunset; and at the same time there was the appearance of a wind-gale in the east. At this time I sent a notice of the approaching storm to those living on the North-east coast, a part of the island likely to be very much exposed to its fury. Some persons did the best they could to secure their houses; but because there was no heavy swell in the sea, the fishermen disregarded the warning, and consequently lost their boats.

It is a singular fact, that about 6 P.M. the barometer not only ceased to fall, but a slight rise was perceptible, which at first led to the supposition that the storm might be only passing, and not approaching, the island. This hope was soon dissipated by the increasing force of the gusts of wind, with another squall of wind and rain about 9 P.M. with a falling barometer.

At midnight the barometer had fallen to 29·50, or about half an inch. Between two and three o'clock A.M., the wind shifted more towards the east, blowing with increased violence, breaking off the branches from the trees, and stripping shingles from the houses; but up to this time no great damage had been done. About 3·30 A.M., a singular circumstance occurred—one which I have never witnessed before, though, during a residence of thirty-three years in these islands, I have experienced many cyclones. The barometer ceased to fall for half an hour; the mercury standing firm at 29·30. This, for the time, led to the conclusion, which soon proved to be erroneous, that the centre of the storm was then passing, and that we had experienced the worst of it. At 4 A.M., the barometer again began to fall, at first slowly, and afterwards rapidly, until, at 6·40, it stood at 28·57, having fallen about an inch and a half below its usual height.

As the barometer fell, the gusts of wind became more violent, shaking large and strongly-built houses to their very foundations, tearing off verandahs, spouting, and window-shutters, and, in some instances, carrying them to great distances. Between 5 and 6 A.M. we experienced the full force of this fearful storm, and it was about this time that a large number of houses, both in town and country, with churches, school-rooms, and estate works, were destroyed.

It was soon after 5 A.M. that the writer was able, from a sheltered position, to have a full view of the awful grandeur of the storm. Low, black clouds, like dark ocean billows driven rapidly overhead; the driving rain like sheets of water; the trees whirled round and beaten nearly to the earth, until rooted up or broken off; the constant flashing of intensely red lightning, with the heavy crash of thunder, mingling with the roaring of the wind—altogether, formed a scene grand and terrific in the extreme; but which was well worth the risk to witness.

About 7 A.M. the centre of the storm passed the south of the island; the barometer began to rise, and the wind changed to S.E. and S. The storm had entirely passed over by 10·30 A.M.

The centre of this storm just touched the extreme south of Antigua; passed directly over St. Kitts, where a calm of twenty minutes was experienced, before the wind burst from the opposite quarter; and also over St. Thomas and Tortola. From thence it passed over the southern islands of the Bahama group. After that I have not been able to trace its course.

Antigua was the first island over which the hurricane passed. Being a comparatively level island—all the high land being situated at the extreme south—it suffered the most severely. Nevis and St. Kitts having mountains from 2,000 to 3,000 feet high, which broke the fury of the storm, only suffered severely in certain parts, principally on the north and east coasts. As the destruction caused by this hurricane has been fully detailed by the newspapers, I need not dwell on that subject in the present paper, but will proceed to state some interesting particulars in reference to the movements of this cyclone.

Its course appears to have been nearly from E. by S. to W. by N. As there was no heavy sea on the shores of Antigua, within a few hours of its arrival, it is evident that it originated within 200 or 300 miles of the island, and during the first hours of its existence was by no means a violent storm.

Its progressive movement was also very slow at first. The first circles struck Antigua soon after 4 P.M. on Sunday, but the centre did not pass until 7 A.M. on Monday; whilst the last half of the storm was only three hours in passing over. It is also evident that from 3·30 to 4 A.M., during the time that the barometer ceased to fall, its progressive movement was altogether suspended, though the rotary motion continued.

After 4 A.M. it began to move with great rapidity, and travelled at a speed, which, as far as I know, has not been equalled by any previous hurricane among these islands. The centre of this cyclone passed Antigua at 7 A.M., and arrived at St. Kitts at 9 A.M., having travelled at the speed of thirty miles per hour. In that island the lofty range of mountains not only broke the force of the rotary motion, but also impeded its progress; so that between St. Kitts and St. Thomas, a distance of 160 miles, it travelled at a speed of a little more than twenty-two miles per hour, the centre arriving at St. Thomas about 4 P.M. on the 21st. What was the speed and force of its rotary motion, I have no means of correctly ascertaining; but there is no doubt that near the centre it very greatly exceeded that of its progressive motion. The diameter of the storm was about eighty miles, the outer circles taking in at the same time Montserrat in the south, and Barbuda in the north; but was not felt beyond those islands. In its progress towards the west and north it may have extended itself, as is frequently the case with these storms.

On the afternoon of September 25, we again had indications of an approaching cyclone, though not so marked and distinct as on the former occasion. The gale set in about 10 P.M., from N. by E., and continued till 10 A.M. on the 26th, the wind changing to N.N.W. and S.W. The centre just touched the north of the island at 4 A.M. on the 26th. The force of the wind was at no time very great, and did not prove destructive on land—though causing much anxiety and alarm during its progress. The barometer did not fall on this occasion more than half-an-inch.

G. W. WESTERBY

Antigua

PROFESSOR S. F. B. MORSE

INTELLIGENCE has already been received in this country of the death of Samuel Finley Breese Morse, the eminent electrician, who died at New York on the 2nd inst. at the age of eighty-one. Prof. Morse was the son of the Rev. Jedediah Morse, well known as a geographer, and was born at Charlestown, Massachusetts, on the 27th of April, 1791. He was educated at Yale College, but, having determined to become a painter, he came to England in 1811, formed a friendship with Leslie, and in 1813 exhibited at the Royal Academy a colossal picture of "The Dying Hercules." He returned to America, and for a few years followed the profession of a portrait painter. In 1829 he again visited England, and on his return voyage was accompanied by Prof. Jackson, the eminent American chemist and geologist, through whose influence he turned his attention to the conduction of electricity through metallic wire, a subject in which the chemical tastes displayed by him while at College gave him additional interest, and to which he now devoted the whole powers of his mind.

Between 1835 and 1837 Prof. Morse invented several machines which more or less foreshadowed the electric telegraph; and obtained from Congress a vote of 30,000 dollars, with which to make an experimental essay between Washington and Baltimore. The first electric telegraph completed in the United States was the line between these cities, which was finished in 1844. Since that time the Recording Electric Telegraph of Morse has been adopted over the whole country, and at the time of his death there were not less than twenty thousand miles of electric wires, stretching over the States between the Atlantic and the Pacific Ocean.

Prof. Morse received during his life recognition of his services to science from a large number of foreign Governments and scientific societies, not the least remarkable being the one inspired by the late Emperor of the French. At his suggestion delegates from France, Russia, Sweden, Belgium, Holland, Austria, Sardinia, Tuscany, the Holy See, and Turkey, met at Paris, and voted an award of 400,000 frs. to Prof. Morse as a testimonial of appreciation of his services.

A record of Prof. Morse's scientific career would not, however, be complete, without referring to a controversy which some years ago occupied the attention of the scientific world in the United States, in which he was engaged with Prof. Henry, now President of the Smithsonian Institution at Washington. So much personal matter was introduced into the dispute that a special committee of the Board of Regents of the Smithsonian Institution was appointed to investigate the matter, the report of which now lies before us. The result of this investigation is summed up as follows:—

"We have shown that Mr. Morse himself has acknowledged the value of the discoveries of Prof. Henry to his electric telegraph: that his associate and scientific assistant, Dr. Gale, has distinctly affirmed that these discoveries were applied to his telegraph, and that previous to such application it was impossible for Mr. Morse to operate his instrument at a distance; that Prof. Henry's experiments were witnessed by Prof. Hall and others in 1832, and that these experiments showed the possibility of transmitting to a distance a force capable of producing mechanical effects adequate to making telegraphic signals; that Mr. Henry's deposition of 1849 . . . . is strictly correct in all the historical details, and that, so far as it relates to Mr. Henry's own claim as a discoverer, is within what he might have claimed with entire justice; that he gave the deposition reluctantly, and in no spirit of hostility to Mr. Morse; that on that and other occasions he fully admitted the merit of Mr. Morse as an inventor; and that Mr. Morse's patent was extended through the influence of the favourable opinion expressed by Prof. Henry."

The conclusion therefore which must be arrived at, and it is one of no small importance in the history of electrical and telegraphic science, is that to Prof. Henry, and not to Prof. Morse, is unquestionably due the honour of the discovery of a principle which proves the practicability of exciting magnetism through a long coil, or at a distance, either to deflect a needle or to magnetise soft iron.

Prof. Morse's services to science as a successful applier of this principle in its practical details are so unquestionable, that we feel we are but doing a duty in setting this question right on this side the Atlantic.

NOTES

THE following are the names of the candidates who have been selected by the Council of the Royal Society for admission into that body at the forthcoming annual election:—Surgeon-Major Andrew Leith Adams, Prof. W. G. Adams, F. Le Gros Clarke, M.R.C.S., Prof. John Cleland, M.D., Dr. M. Foster, Dr. Wilson Fox, Dr. Arthur Gamgee, Rev. Thomas Hincks, Prof. W. Stanley Jevons, Prof. T. Rupert Jones, Dr. George Johnson, Major T. G. Montgomerie, R.E., Dr. E. L. Ormerod, E. J. Routh, and Dr. W. J. Russell.

AT the meeting of the Royal Geographical Society, held on Monday evening last, a letter was read addressed to the President by Dr. Kirk, H.B.M. consul at Zanzibar, in which that gentleman expressed himself very hopefully of Dr. Livingstone's safety. He thinks there is nothing discouraging in the last news received of him, and that we cannot expect to hear again unti the war at Unyanyembe is closed.

H.R.H. THE DUKE OF EDINBURGH will hold a reception on Saturday evening next in the Picture Galleries of the International Exhibition and in the Royal Albert Hall, on behalf of the Prince of Wales and the Royal Commissioners.

WE understand that Lieut-Colonel Strange, F.R.S., will exhibit at the ordinary meeting of the Royal Society on Thursday, May 2nd, the Great Theodolite designed by him for the Great Indian Trigonometrical Survey of India, and will read a paper descriptive of it.

THE electors of the Waynflete Professorship in Chemistry at Oxford have given notice that it is their intention to proceed to the election of a Professor some time in Act term next. The endowment assigned to the Professorship is 600*l*. per annum, payable out of the revenues of Magdalen College. The residence required by the College ordinance is six calendar months, at least, between the 10th day of October in every year and the first day of the next ensuing July. By the same ordinance the College may require certain services from the Professor; but the functions and duties of the office are mainly regulated by a statute of the University, the provisions of which, as well as of the College ordinance, may be obtained from the President of Magdalen, to whom persons intending to become candidates are requested to send in their names, and any papers which they may wish to present to the electors in support of their application, on or before the 18th of May.

THE next triennial prize of 300*l.* under the will of the late Sir Astley T. Cooper, Bart. will be awarded to the author of the best essay or treatise on "Injuries and Diseases of the Spinal Cord." The essays or treatises shall contain original experiments and observations, which shall not have been previously published, and each essay or treatise shall be illustrated by preparations, and by drawings, which shall be added to the Museum of Guy's Hospital, and shall, together with the work itself, become henceforth the property of the Institution. Essays must be sent in to Guy's Hospital on or before January 1, 1874. If written in a foreign language they must be accompanied by an English translation.

RADCLIFFE Studentships for persons studying medicine, and desirous of making use of the museum and lectures at Oxford, have been awarded to Mr. Francis T. Carey, of Guy's Hospital, and Mr. C. R. B. Keetley, of St. Bartholomew's Hospital, on the recommendation of Sir James Paget, Sir William Gull, and Dr. John Ogle; and to Mr. Farington M. Granger, of the Hospital of Leeds, on the recommendation of T. P. Teal, M.A., M.B.

M. STEWART of Rossall School has been elected to the Exhibition of 50*l.*, at St. John's College, Cambridge, tenable for three years, for Natural Science. The examiners also honourably mentioned Anderson, of Rugby School, and King's College, London. There were seven candidates. The examiners were —Chemistry, Mr. Main; Physics, Mr. A. Freeman; Physiology, Dr. Bradbury; Geology, Mr. Bonney; Botany, Mr. Hiern.

CAPTAIN G. S. NARES will, on his arrival in England from the Mediterranean, commission the unarmoured screw corvette *Challenger* for special exploring and surveying duties in the Pacific. The *Challenger* is a vessel of 2,306 (1472) tons and 1509 (400) horse power.

IT is proposed, according to the *American Naturalist*, to add a department of Science to the executive branch of the United States Government. It is to be composed of the Storm Signal Corps of the army, the Lighthouse Board, and the Coast Survey Bureau of the Treasury, and the Hydrographic Bureau of the Navy.

WE hear that a proposition is on foot to establish an Agricultural-Meteorological station at Montrouge, near Paris, under the superintendence of M. Ch. Moureaux.

A COMMUINCATION to the Corporation of Brown University, in America, was recently presented from Colonel Stephen T. Olney, making a munificent offer of his herbarium and books on botany, on condition that a suitable building should be provided for their reception. It was referred to a committee.

THE officers of the Boston (U.S.) Young Men's Christian Union, recognising the importance of scientific studies and the need of encouraging scientific tastes, have determined to establish in the rooms of the Union a natural history cabinet. Their object in providing such a collection is to foster the growing taste for science among the young men of Boston, and to open a new source of instruction and amusement to the members of the Union.

A VERY interesting collection of water-colour drawings made by Mr. W. Simpson, on the various excavations below the modern city of Jerusalem, which have been carried on for the past three years by the Palestine exploration, under the superintendence of Captain Warren, of the Engineers, is now placed for exhibition in the Gallery, 48, Pall Mall. Most of the drawings are taken in the excavations or in the sacred tombs and caves; but the artist has made his series complete by two or three which represent the massive walls of Jerusalem as they are now to be seen above ground, as well as that part of them which has been discovered at the depth of 125 ft.

AT the meeting of the Manchester Literary and Philosophical Society on March 19, the President, Mr. E. W. Binney, read an elaborate paper, entitled "Additional Notes on the Lancashire Drift Deposits."

AT the Annual Meeting of the Bengal Social Science Association, Dr. Ewart, the president, delivered an excellent address on the necessity for the introduction into the schools preparing students for the entrance examination of the University of Calcutta, of the study of the rudimentary principles of the natural and physical sciences. Although the University was avowedly founded on the model of the London University, the traditional policy of the older Universities is apparent in the exclusion of Science; and a movement commenced last year to introduce examination in various branches of science has at present failed. Dr. Ewart points out with great force the injury which the higher education has suffered in England from a similar course, and that the "existing metaphysical system of education is fast flooding the country with a class of gentlemen who cannot find occupation suitable to the kind and nature of the training they have received." "Are we to wait here," he inquires, "simply to follow in the wake of England in this matter? Is India to go through a long embryo state of preparation like the Western nations, extending over many centuries?"

A NEW technical paper has been started at Brussels, entitled *Chronique de l'Industrie*, answering to our English papers, the *Engineer* and *Engineering*.

DR. L. PFEIFFER, of Cassel, has published the two first parts of a work which will be indispensable to every systematic botanist, "Nomenclator Botanicus," being an alphabetical enumeration of the names of all classes, orders, tribes, families, divisions, genera, sub-genera, and sections of plants, published down to the end of the year 1858, with copious references to the authorities, systematic arrangement, synonymy, and first publication. From the care evidenced in the parts already published, the work will supply a desideratum long felt in botanical literature; and the author, who is an amateur man of science holding no official position, deserves the thanks of all botanists. Arrangements are made by which the work may be carried down to the present time.

MR. C. P. HOBKIRK, of Huddersfield, announces as in course of preparation, A Synopsis of the British Mosses, in 1 vol. 8vo.

THE long-expected translation of Le Maôut and Decaisne's "Traité Général de Botanique," by Mrs. J. D. Hooker, is announced by Messrs. Longman as in the press.

MESSRS. BRADBURY AND EVANS have in the press "Botany for Beginners," by Dr. Maxwell T. Masters, F.R.S., a portion of which has already appeared in the columns of the *Gardener's Chronicle*.

A VERY interesting series of articles on the animals contained in the Crystal Palace Aquarium, by Mr. Edward Newman, F.L.S., is now being published in *The Field*.

THE Catalogue of Microscopical Preparations in the Quekett Microscopical Club, consisting of about 2,000 slides, is chiefly of interest from the plan on which they are classified. The arranging and cataloguing a large collection of microscopical preparations in a satisfactory manner is admitted to be a work of considerable difficulty. In the present instance the catalogue is divided into five groups, containing Vertebrata, Invertebrata, Phanerogamia, Cryptogamia, and Mineral Substances. Each of the groups Vertebrata and Invertebrata is again subdivided into orders; and the Phanerogamia into stems, fibres, cell structure, cuticles, hairs, pollen, seeds, and starches. The Cryptogamia are subdivided into ferns, mosses, fungi, characeæ, algæ, desmids, and diatoms. The minerals are without subdivision. Although this classification is open to some objections, yet, on the whole, it was perhaps about the best which could have been done with the materials; and the catalogue, which is on sale for the benefit of microscopists generally, will furnish a long list of objects for those who are collecting. In some of the subsections—as, for instance, the hairs of bats, fructification of ferns, and microscopic seeds—the cabinet appears to be remarkably complete. Indeed, it is doubtful whether these sections can be equalled in the cabinet of any other society.

In a letter to the Minister of Foreign Affairs, brought before the French Academy on February 26, a report is given of the earthquake shock felt at Malaga on January 28 at $3^h\ 1^m$ P.M. The undulatory movement lasted from four to six seconds, and subterranean noises were heard previously to the shock. The direction was from north to south. No damage is reported.

The *American Journal of Science* gives an account of the earthquake of the 9th of January experienced in New England. It occurred over a considerable portion of Eastern New England and the St. Lawrence Valley, at a few moments before 8 P.M. on January 9. It was felt along the St. Lawrence River to a distance of 200 miles north-east and 60 miles south-west from Quebec, and at various points of New Hampshire and Maine. The disturbance was greatest at Quebec, where some walls were cracked, and large fissures caused in the ice bridge over the river. The shock occurred there at 7·54 P.M. and lasted about thirty seconds, being accompanied by a low rumbling sound. At Lancaster, in New Hampshire, there were two distinct shocks, each lasting but a few seconds, the last being the most violent. The direction of vibrations was well defined, and approximately west to east. Probably the true direction was from a point somewhat south of west, which would coincide nearly with the course of the St. Lawrence River, and with the shorter diameter of the region shaken. At Quebec and Bangor slight shocks were felt at 3 P.M. and 11 P.M. on the same day.

The towns of Dresden, Perna, Schandau, Chemnitz, Bodenbach, Wurmar, and Rudolstadt, were visited almost simultaneously by a succession of earthquake shocks between three and four o'clock on Wednesday, March 6. They continued to recur during an hour, and in some instances during several hours. Little damage was done.

A correspondent of the *Times*, telegraphing from Alexandretta, states that half the town of Antioch was destroyed by an earthquake on April 3. Fifteen hundred persons were killed. Great distress prevails in consequence. The shock was also felt at Aleppo, but without any damage being done.

We have received the second part of the Proceedings of the Bristol Naturalists' Society for 1871. Like other similar publications which we have had occasion to notice recently, it contains no original articles bearing on the natural history of the district, or containing original observations. The proceedings of the Sections are also rather scant, though they bear evidence of some work having been done in Geology and Entomology.

We have received a series of chemical labels published by Messrs. Mottershead and Co., of Manchester. The labels are printed in good legible type, better than is usual in such cases, the backs being gummed ready for use. No definite system of chemical nomenclature has been used, in many instances the common or old names of the reagents are given, although perhaps in some cases to the sacrifice of scientific accuracy. Contrary to the usual practice, no symbols are attached, the publishers preferring to leave space for the insertion of these, according to the views held by each chemist. At the end of the reagents a number of slips are attached, with the words "pure," "commercial," &c., to qualify the foregoing labels. The total number given is considerable, forming a very good and cheap series (9*d.*)

OBSERVATIONS OF THE AURORA BOREALIS OF FEBRUARY 4 & 5, 1872 *

The splendid aurora by which our sky was illuminated yesterday evening was remarkable for the great variety of appearances which it displayed, for its intensity, its duration, and lastly for the large expanse of sky over which it spread. In fact it exhibited collectively all the principal phenomena observed in former appearances of this meteor: that is to say, luminous arches of various colours, dark arches, moveable clouds of red and green colour, bright rays both isolated and united in large bundles, dark rays, diverging and converging rays, red pillars, changes of colour, &c., &c. It lasted from about six o'clock till after midnight, whereas in most cases the duration of the phenomenon does not exceed a few hours, and is not unfrequently less than an hour. The auroral light, under various forms and colours, extended over nearly the whole of the sky, whereas it is usually limited to the northern region.

The want of magnetic instruments prevented me from foreseeing by their perturbations the approach of the phenomenon, so that I did not perceive it till about 6h. 30m., by which time it was already developed in vast proportions. I arrived, however, in time to observe all the principal phases, and to analyse the various coloured lights with the spectroscope.

Although the numerous phenomena observed in this aurora did not present anything actually new, still their detailed description will be of great advantage to science, as exhibiting the order of their succession and their mutual relations and dependences. Such scope, however, could be attained only by a long and systematic description; and for the present I must limit myself to an account of my spectroscopic observations.

The greenish yellow light which illuminated certain arches and isolated clouds, and likewise the part of the sky near the magnetic meridian, appeared, when examined by the spectroscope, to be monochromatic, its spectrum being almost wholly concentrated in a beautiful green line, the position of which was very near the division 1241 of Kirchhoff's scale.

In the brightest parts nearest to the magnetic meridian, and in a few yellow rays near the zenith, I discovered, by means of the spectroscope, a second green line situated towards the blue, and corresponding very nearly with the division 1820 of Kirchhoff's scale. The line 1241 is near a known line of iron, and 1820 is near a known line of atmospheric air.

The second green line was very much less bright than the first, but nevertheless very distinct. Between these two lines were traces of several other faint lines, the position of which I was unable to determine.

On the decidedly red mass I could not make out any distinct bright line, but only certain bands of continuous spectrum. On the yellow-red mass I detected the bright line 1241, without any distinct lines in the red.

The spectroscopic observations were continued with the same results till about the middle of the night, when the aurora had almost entirely vanished.

The atmospheric conditions during the phenomenon were normal, only a few masses of cloud being seen from time to time near the horizon; and I observed an appearance which seemed to me especially worthy of notice—namely, a continuous glow proceeding from the horizon towards the S.S.E., by which some clouds and a stratum of mist were lighted up almost continuously

* Translated from the *Gazetta Ufficiale del Regno d'Italia*, Feb. 6.

and with considerable brightness, chiefly from half-past eight to ten o'clock.

During the phenomenon several falling stars were observed, and a magnificent bolide in the Great Bear at 8h. 30m., but this was probably accidental.

This evening, in the expectation that the aurora borealis might reappear, I began to observe the sky as soon as twilight was over, and I perceived a faint glow, a kind of phosphorescence, diffused over the whole sky, but without any decided appearance of boreal light.

While waiting for more imposing phenomena, I directed the spectroscope towards the zodiacal light, to ascertain whether its spectrum could be observed at Rome, as it had been observed on the Red Sea on the evening of the 11th, and the morning of the 12th January last.

Angström, in 1867, found the spectrum of the zodiacal light to be monochromatic, consisting of a single green line, to which he assigned approximately the position 1259 on Kirchhoff's scale, the same that he had determined for the green line of the aurora borealis; and I myself, on the days above mentioned, was able to perceive in the zodiacal light, not only this green line, but near it and towards the blue, a band or zone of apparently continuous spectrum.

This evening at seven o'clock, I was able to discern the same spectrum in the light above mentioned; and on directing the spectroscope to other points, I found that this spectrum showed itself in all parts of the heavens from the horizon to the zenith, more or less defined in different parts, but everywhere as bright as in the zodiacal light. The observatory assistant, Dr. di Legge, likewise observed this spectrum distinctly, in various parts of the heavens.

This fact, which corroborates an analogous observation made by Angström in 1867, appears to me of the greatest importance, inasmuch as it demonstrates the identity of the zodiacal light with that of the aurora, and thereby tends to establish the identity of their origin, and to unite into one these two mysterious phenomena.

L. RESPIGHI

Observatory of the Royal University of Campidoglio,
Feb. 5, 1872.

PHYSIOLOGY

Note on Recurrent Vision*

In the course of some experiments with a new double plate Holtz machine, belonging to the college, I have come upon a very curious phenomenon, which I do not remember ever to have seen noticed. The machine gives easily intense Leyden jar sparks from seven to nine inches in length, and of most dazzling brilliance. When, in a darkened room, the eye is screened from the direct light of the spark, the illumination produced is sufficient to render everything in the apartment perfectly visible; and what is remarkable, every conspicuous object is seen *twice* at least, with an interval of a trifle less than one quarter of a second—the first time vividly, the second time faintly; often it is seen a third, and sometimes, but only with great difficulty, even a fourth time. The appearance is precisely as if the object had been suddenly illuminated by a light at first bright, but rapidly fading to extinction, and as if, while the illumination lasted, the observer were winking as fast as possible.

I see it best by setting up in front of the machine, at a distance of eight or ten feet, a white screen having upon it a black cross, with arms about three feet long and one foot wide, made of strips of cambric. That the phenomenon is really subjective, and not due to a succession of sparks, is easily shown by swinging the screen from side to side. The black cross, at all the periods of visibility, occupies the same place, and is apparently stationary. The same is true of a stroboscropic disc in rapid revolution; it is seen several times by each spark, but each time in the same position. There is no apparent multiplication of a moving object of any sort.

The interval between the successive instants of visibility was measured roughly as follows:—A tuning fork, making 92½ vibrations per second, was adjusted, so as to record its motion upon the smoked surface of a revolving cylinder, and an electromagnet was so arranged as to record any motion of its armature upon the trace of the fork: a key connected with this magnet was in the hands of the observer. An assistant turned the machine slowly, so as to produce a spark once in two or three seconds, while the observer manipulated the key.

In my own case the mean of a dozen experiments gave $0^s{\cdot}22$ as the interval between the first and second seeing of the cross upon the screen; separate results varying from $0^s{\cdot}17$ to $0^s{\cdot}30$. Another observer found $0^s{\cdot}24$ as a result of a similar series.

Whatever the true explanation may turn out to be, the phenomenon at least suggests the idea of a *reflection of the nervous impulse* at the nerve extremities—as if the intense impression upon the retina, after being the first time propagated to the brain, were there reflected, returned to the retina, and from the retina travelling again to the brain renewed the sensation. I have ventured to call the phenomenon "Recurrent vision."

It may be seen, with some difficulty, by the help of an induction coil and Leyden jar; or even by simply charging a Leyden jar with an old-fashioned electrical machine, and discharging it in a darkened room. The spark must be, at least, an inch in length.

Hanover, February 9

* From the *American Journal of Science and Art* for April. By Prof. C. A. Young, of Dartmouth College.

SCIENTIFIC SERIALS

Annales de Chimie et de Physique, July and August, 1871. This number contains the second portion of a very lengthy memoir by M. Berthelot on explosive agents in general; this half of the communication deals with dynamite, gun cotton, picric acid and potassic picrate. At the end of the memoir a general table is given which shows the amount of heat generated and the volume of gas formed by one kilogram of substance; the product of these two numbers will of course give the relative effects produced by each compound; the numbers given show that if nitroglycerine produces an amount of force equal to 94, picric acid equals 54, gun cotton 50, potassic picrate 34, whilst gunpowder has only an explosive force equal to 14. M. Janssen contributes a very valuable paper on the atmospheric lines in the solar spectrum. He finds that the bands observed by Brewster and Gladstone can be resolved into fine lines comparable to the solar lines properly so called, and that the atmospheric lines are more numerous than the solar lines in the red, orange, and yellow portions of the spectrum. The atmospheric lines are always visible in the solar spectrum, some lines it is true almost disappear when the sun is very high, but they are those which are never very intense; the author finds that the intensity of the atmospheric lines observed at the horizon is about fifteen times as great as when observed in the meridian. M. Janssen has also examined the spectrum of the moon and stars, and more particularly of Sirius and α in Orion; he has not succeeded in observing any new lines whatever in the spectrum of the moon, proving that our satellite cannot have any appreciable atmosphere. M. Raoult has found that a solution of cane sugar sealed up in vacuo and exposed to light for five months is partially changed into glucose. Amongst the other original memoirs there is a very long one by Dr. de Coppet on the temperature of congelation in saline solutions. There are also a considerable number of abstracts of papers from foreign journals, making up altogether a very bulky number.

THE *Journal of the Quekett Microscopical Club*, No. 18, April 1872, contains the following three communications:—"Observations on the Polyzoa, by A. H. H. Lattey, M.R.C.P." This paper is chiefly devoted to the preparation of the Polyzoa for the microscope, so as to exhibit them in permanence with the tentacles expanded.—"On the so-called 'nerve' of the Tooth," by T. C. White, Hon. Sec. The principal elements met with in a microscopical examination of what is popularly termed the "nerve" of the tooth, are here indicated, and suggestions are given to assist in the more complete examination of tooth-structure.—"On the Internal Structure of the *Pulex irritans*," by W. H. Furlonge. This is a second communication on the structure of the flea which has been submitted to the club by its author. The first was occupied chiefly in the examination of external organs, the present is devoted to internal structure, commencing with the alimentary and digestive system, then a few remarks on the respiratory system, and finally observations on the reproductive system. The embryology is left untouched, to form the subject of a third and concluding paper, which will then embrace the life history of one of the commonest, but not the least interesting, of British insects. The club announced its list of excursions for the season commencing April 6, and terminating October 5. There are fifteen excursions, of which fourteen are announced for Saturday afternoons, one whole day excursion, and one day excursion ending with the excursionists' annual dinner. The annual *soirée* of the club was held at University College on Friday evening, March 22, and was attended by about 1,200 persons.

Journal of the Chemical Society, February. — Dr. Armstrong contributes a paper "On the nitration products of the dichlorophenolsulphonic acids," being a continuation of his researches published in recent numbers of this journal. The next communication is on Eulyte and Dyslyte, by H. Bassett, being a re-examination of these bodies, which were briefly described by Baup in 1851. The third and last original communication is by Dr. Howard, "On Quinicine and Cinchonicine and their salts." Some time since the author gave an account of an amorphous alkaloid from cinchona bark, the properties of which distinguished it from those already described. Further investigations, however, have shown that it is probably identical with quinicine, first obtained by Pasteur by the action of heat on quinine. The author finds that the quinicine obtained from quinine, and that obtained from quinoidine, are identical in their properties. Several salts of cinchonicine have been prepared; there is considerable resemblance between them and the quinicine salts, although the former are somewhat more soluble. The same identity is observed between the cinchonicines obtained from cinchonine and from cinchonidine as was observed in the case of quinicine. The action of these alkaloids on polarised light confirms the identity already mentioned. Thus, the quinicines prepared, either from quinine (which possesses a strong left-handed rotation), or from quinidine (which has a right-handed rotation), exhibit a eeble right-handed rotation, which, in each case, is almost identical. The abstracts of papers in foreign journals occupy seventy-pages, and, as usual, are of great interest.

Verhandlungen der k. k. geologischen Reichanstalt zu Wien. Nos. 3 and 4. The articles in these numbers are for the most part of local interest; but we notice a short sketch of the geological structure of East Greenland by F. Toula—some of the fruits of the last German expedition—which will be read with interest. Literary and other notices, as usual, occupy considerable space in the proceedings.

THE *Geological Magazine* for April 1872 (No. 94) opens with an excellent article from the pen of Mr. W. Davies, of the British Museum, on the rostral prolongations of the singular Liassic Fish, described by Agassiz under the name of *Squaloraia polyspondyla.* The two projecting processes from the snout of this fish were regarded by Dr. Riley and Prof. Agassiz as forming a single rostrum; but Mr. Davies argues with justice that the upper one is really a cephalic spine analogous to that met with in a similar situation in the male Chimæridæ, and that it was employed, as by them, in conjunction with the elongated rostrum, for securely clasping the female. Mr. Davies refers to other points in the anatomy of this curious fish, which he illustrates with a large plate.—Prof. Dyer commences the description of some remains of coniferous plants from the lithographic stone of Solenhofen; the form here described is named by him *Araucarites Haberleinii.*—From Mr. Searles Wood, jun. we have a paper on the climate of the post-glacial period, and a reply to Mr. James Geikie's Correlation of the Scotch and English Glacial beds, whilst the last-mentioned author contributes a fifth paper on Changes of Climate during the Glacial epoch.—Some points in the Geology of the East Lothian coast, form the subject of a paper by Messrs. G. W. and F. M. Balfour, in which they describe the peculiar relations existing between the porphyrite of Whitberry Point and the adjacent sedimentary (sandstone) rocks, the latter being found to dip on all sides towards the mass of porphyrite. The authors suppose the porphyrite to have been erupted through a small orifice, and to have caused the depression of the sedimentary beds by pressure.

THE original articles in the March number of the *American Naturalist* are not so numerous as usual. Prof. J. D. Biscoe commences with a description of the breathing-pores or stomates of leaves.—Prof. H. W. Parker describes the meteorological phenomena witnessed in the western prairies, including the very common occurrence of parhelia in mock suns.—Dr. R. H. Ward has some remarks on uniformity of nomenclature in regard to microscopical objectives and oculars, of considerable interest to microscopists.—The most important article is "On the Stone Age in New Jersey," by Dr. Chas. C. Abbott, illustrated with a number of woodcuts of the rude implements and utensils found throughout that State, the relics of its original Indian inhabitants.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, April 10.—"Notice of some of the Secondary Effects of the Earthquake of the 10th January, 1869, in Cachar." Communicated by Dr. Oldham, of Calcutta, with remarks by Mr. Robert Mallet, C.E., F.R.S. This earthquake was a severe one, being strongly felt in Calcutta, distant from the meizoseismic area about 200 miles, and far into the plain of Bengal. The effects were examined on the spot a few weeks after the shock by Dr. Oldham, who anticipates being able to fix the position and depth of the centre of impulse by following the same methods as those first employed by Mr. Mallet with respect to the great Neapolitan earthquake of 1857. These results have not yet been received; but Dr. Oldham has forwarded an extremely interesting letter on the circumstances of production of very large earth-fissures, and of the welling up of water from these, derived from the water-bearing ooze-bed, upon which reposed the deep-clay beds in which the fissures were formed. Dr. Oldham rightly views all these fissures, which were all nearly parallel to and not far distant from the steep river banks, as "secondary effects," and not due to fractures produced by the direct passage of the wave of shock. He also shows that the welling up or overflowing of the water in the fissures was a secondary effect also, and negatives the notion entertained on the spot of mud-volcanoes, &c., having originated at those fissures. The chief aim of Mr. Mallet's remarks was to point out the importance to geologists of rightly comprehending the dynamics of production of these phenomena, and to show that the older notions of geologists as to earthquake-fissures are untenable. He explained clearly, aided by diagrams, the train of forces by which the elastic wave of shock, on passing out of the deep-clay beds where these have a *free side* forming the steep river banks, dislodges certain portions and throws them off towards that free side—and that this is but a case of the general law in accordance with which such elastic waves behave towards more or less incoherent deposits reposing on inclined or on level beds, under various conditions. Mr. Mallet also explained the dynamic conditions under which the water from water-bearing beds, such as that of ooze beneath the Cachar clay beds, becomes elevated in the fissures formed, and gave approximate expressions for the minimum height to which the water can rise in relation to the velocity of the elastic wave particle. The paper concluded with some explanatory remarks upon the continual noises, like the irregular fire of distant artillery, heard long after the shock had passed, and when the country had become perfectly quiescent. The noble collection of photographs which were made by Dr. Oldham, and forwarded to Mr. Mallet, illustrative of the physical features of the huge earth-fissures and other effects of this earthquake, were exhibited to the Fellows present, and are well worthy of attentive study. Sir Henry James inquired whether there was any trace of fissuring in the lower beds beneath the slimy ooze. Mr. Scott wished to ascertain the author's opinion as to the possibility of predicting earthquakes on meteorological grounds, as had been done by M. Boulard, several of whose prophecies were said to have been fulfilled. Mr. D. Forbes gave some details of the earthquake of Mendoza, a town situated on a vast alluvial plain at the foot of the Andes, in which the phenomena remarkably coincided with those detailed by Dr. Oldham. In that case he found that the rumours as to fire and smoke having been emitted from fissures were entirely without foundation, the presumed smoke having been nothing but dust. The earthquake was felt over a distance of 1,200 miles; and wherever the firm rock came to the surface there was no trace of fissure, though portions of the rock were overthrown. But in the plain, consisting of 30 or 40 feet of alluvial soil, the whole ground was in places fissured, and in some districts the surface completely furrowed, and even the turf turned over. He had witnessed numerous earthquakes, and in some cases had been in deep mines during their occurrence, when the sound only could be heard, and he could testify to their effects being confined to the surface. The direction of the fissures was invariably at right angles to the line of shock. In South America all the earthquakes could be traced to volcanic centres. The President inquired as to the distinction to be drawn between the primary and secondary effects of earthquakes, and whether the author thought that no fissures were attributable to the direct action of earthquakes. As to the cause of the sound, like that of a cart carrying iron bars or of an artillery waggon, he wished for further information. Mr. Mallet, in reply, explained that fissures only take place where masses were comparatively free in one direction. They might extend to enormous depths, though they often closed in rapidly. With regard to the power of predicting earthquakes, he disbelieved in it wholly, and considered that any fulfilment of such prophecies must be due to accident; earthquakes are so numerous, that the chances of such fulfilments are great. The blow or impulse originating earthquakes could not be attributed solely to one cause. It arose often from deep subterranean volcanic action; but it also—especially in the case of long-continued tremors, like those of Comrie or Pignerol—arose from the breaking up or the grinding over each other of rocky beds at a great depth, through the tangential pressures produced in the earth's crust by secular cooling. The arrested impulse of the fall of the Rosberg in Switzerland produced a sensible earthquake. Fissures in hard rock could not be produced *directly* by the shock, because the velocity of impulse in such rock greatly exceeded that of the elastic wave particle. The earth's crust was at present not in a state of tension, but of compression, through secular cooling.

Zoological Society, April 16.—Dr. E. Hamilton, vice-president, in the chair. A letter was read from Dr. R. Schomburgh, of the Botanic Gardens, Adelaide, South Australia, containing an account of the apparently reasonable conduct of a monkey kept in the gardens.—Mr. A. H. Garrod, Prosector to the Society, read a paper on the mechanism of the gizzard in birds, in which he endeavoured to show that the ordinary action of this organ was that of compression, and not of trituration as usually understood.—A communication was read from Dr. John Anderson, on a supposed new monkey from the Sunderbunds to the east of Calcutta, allied to *Maccacus rhesus*.—A communication was read from Mr. W. H. Hudson, containing remarks on the birds of the Rio Negro of Patagonia, as observed during a recent visit to that locality. To this was added an appendix, by Mr. Sclater, giving a scientific account of Mr. Hudson's collections.—A communication was read from Mr. R. Swinhoe, containing descriptions of two new pheasants (*Phasianus ellioti* and *Pucrasia darwini*) and a new *Garrulax* (*G. picticollis*) from the vicinity of Ningpo, China.—A paper by Mr. F. Moore was read containing the descriptions of a large number of new species of Indian Lepidoptera.—Mr. E. W. H. Holdsworth read notes on a Cetacean observed on the west coast of Ceylon, remarkable for possessing a long, straight dorsal fin, and known to the natives as the "Palmyra fish."—Dr. A. Günther read a paper on a collection of reptiles and amphibians made at Metang, in the district of Sarawak, Borneo; to which was added a synopsis of the known species of these classes hitherto recorded from that island. These were stated to be altogether 153 in number.—Sir Victor Brooke, Bart., gave a description of a supposed new species of gazelle from Ugogo in Eastern Africa, which he proposed to designate *Gazella granti*.

Linnean Society, April 18.—Mr. G. Bentham, president, in the chair. Mr. M. E. Grant-Duff, M.P., was elected a fellow.—The President announced the death of Prof. v. Mohl, one of the foreign members of the society.—Prof. Oliver described four new genera of plants recently received at the Kew Herbarium. 1. A new genus of Begoniaceæ, from New Grenada, of special interest, as the order at present consists only of the large genus *Begonia*, and another monotypic one from the Sandwich Isles. It resembles in habit the series of *Begonia* with thin membranous leaves not cordate at the base; but is very aberrant from the typical genus in possessing a single monophyllous perianth, and being monœcious, the male flowers possessing only four stamens, which are apparently didynamous, and give the plant an external resemblance to Gesneraceæ, the ovary, however, is that of typical *Begonia*. Prof. Oliver gives this new genus the name *Begoniella*. It does not appear to throw any light on the difficult affinities of the order. The three other genera are from Dr. Maingay's collection from the Malay Peninsula. The first is a new genus of Hamamelidæ, *Maingaya*, in which the calyx is perfectly closed in the bud, and afterwards ruptured. The order is of interest as going back at least to the Miocene period, and still existing in both hemispheres. The two other new genera belong to the order Olacineæ. The first, *Ctenolophon*, is one of the few genera of the order with opposite leaves. The second, *Pteleocarpa*, includes two species from Malacca and Borneo.—Prof. Thiselton Dyer on the Assam tea-plant. The Chinese tea-plant is not known in the wild state. The Assam tea-plant may be its indigenous form, but presents well-marked differences.—Dr. Braithwaite on *Zoopsis*, Hook. and Tayl., a genus of Hepaticeæ.

Chemical Society, April 18.—The president, Dr. Frankland, F.R.S., in the chair.—The secretary read two papers by Mr. E. A. Letts, "On benzyl isocyanate and cyanurate," and "On a compound of sodium and glycerine."—Prof. Hunly, of Kiel, who spoke in German, gave an account of a new method of determining the carbonic acid in sea-water, and of an apparatus for collecting the water at great depths, which could be immersed to the required distance below the surface, and then closed by means of stop-cocks. These are turned by powerful springs released at the proper moment by an electro-magnet.—Dr. E. T. Thorpe followed with notes on the action of phosphorus pentasulphide on tetrachloride of carbon, and on the degree of solubility of silver chloride in strong nitric acid.—Dr. Hofmann, F.R.S., then gave a brief account of the new phosphorus bases, which he had recently obtained by the action of alcoholic iodides on iodide of phosphonium on the presence of zinc oxide, and illustrated his remarks by several striking experiments.

Mathematical Society, April 11.—Prof. Cayley, F.R.S., vice-president, in the chair.—Prof. Cayley gave an account of a paper "On the Mechanical Description of certain Sextic Curves."—Mr. Roberts then exhibited an apparatus for the description of such curves as had been alluded to by Prof. Cayley; and further drew attention to an analogous manner in which certain surfaces of the fourth degree may be generated.—A discussion followed upon some questions proposed in which the chairman, Prof. Crofton, Messrs. Cotterill, Merrifield, Sprague, and others took part.

Photographic Society, April 9.—James Glaisher, F.R.S., in the chair. A paper on Merget's Mercury-Printing Process was read, and some photographs produced by its means were shown. The photographic image is produced by the reduction of silver, or other precious metal, salts, by mercuric vapour, which has been in the first place collected upon a cliché obtained in the camera. The process is not yet sufficiently elaborated to be of much practical value.—A paper "On the Photographic Image upon a Bichromate Film" was read by Mr. H. Baden Pritchard, who demonstrated by a few examples the rapidity with which the image, once started by light upon a carbon tissue, continues to acquire vigour after the latter has been withdrawn from the action of the solar rays.

Victoria Institute, April 15.—The Rev. J. G. Wood "On the Rationality of the Lower Animals." He gave various instances of the instinct and rationality of different animals inhabiting various portions of the globe, and dwelt principally on the latter, which he considered many animals to possess, though in a very limited sense. An interesting discussion followed, in which Captain Petrie pointed out that had the animal creation no rationality, or rather intelligence, it would be without an attribute, which helped to make it more subservient to man's wants. The Rev. C. A. Roe pointed out that the reasoning powers of man were different from the reason possessed by animals, which was exceedingly limited, and of a peculiar nature.

MANCHESTER

Literary and Philosophical Society, March 5.—E. W. Binney, F.R.S., president, in the chair. "On Changes in the Distribution of Barometric Pressure, Temperature, and Rainfall under different Winds during a Solar Spot Period," by Joseph Baxendell, F.R.A.S.—"Further Experiments on the Rupture of Iron Wire," by Mr. John Hopkinson.

Physical and Mathematical Section, November 7, 1871.—Alfred Brothers, F.R.A.S., vice-president, in the chair. "On Changes in the Distribution of Barometric Pressure, Temperature, and Rainfall, under different winds, during a Solar Spot Period," by Joseph Baxendell, F.R.A.S.

December 5, 1871.—Mr. Thomas Carrick in the chair. "On the Distribution of Rainfall under different Winds, at St. Petersburg, during a Solar Spot Period," by Joseph Baxendell, F.R.A.S.

February 27.—E. W. Binney, F.R.S., vice-president of the section, in the chair. "Results of Observations, registered at

Eccles, on the Direction and Range of the Wind for 1869, as made by an Automatic Anemometer for Pressure and Direction," by Thomas Mackereth, F.R.A.S.—"On Black Bulb Solar Radiation Thermometers exposed in various Media," by G. V. Vernon, F.R.A.S.—Note "On the Relative Velocities of different Winds, at Southport, and Eccles, near Manchester," by Joseph Baxendell, F.R.A.S.

CAMBRIDGE

Philosophical Society, March 11.—Mr. E. H. Morgan, of Jesus College, and Mr. J. W. Cartmell, of Christ's College, were elected fellows. The following communications were read:—(1) By Mr. Hiern, "A monograph of the *Ebenaceæ*." This elaborate paper will shortly appear in the Society's Transactions. (2) By Dr. Bacon, "The influence of human generations on the production of insanity." The author brought forward statistics to prove that insanity was proportionate to poverty—the greatest number of insane persons being found in the poorest districts. Hence he considered that ameliorating the condition of the people was of the first importance in the attack on this disease. (3) By Mr. J. W. L. Glaisher, "Supplement to a table of Bernouilli's numbers."

EDINBURGH

Royal Society, March 18.—Sir Robert Christison, Bart., president, in the chair.—"On the Extraction of a Square Root of a Matrix of the Third Order," by Prof. Cayley.—"Second Note on the Strain-Function," by Prof. Tait.—"Note on the Rate of Cooling at High Temperatures," by Prof. Tait.—"Notice of a Whinstone Boulder with Artificial Markings and Grooves on it," by Mr. D. Milne Home, LL.D.—"Notice of the Fruiting of the Ipecacuan Plant in the Edinburgh Royal Botanic Garden," by Prof. Balfour.

Royal Physical Society, March 27.—Mr. C. W. Peach, president, in the chair. Note on the occurrence of the Hoopoe (*Upupa Epops*) at Freugh, Stoneykirk, Wigtownshire, by Rev. George Wilson. The specimen, a male in perfect plumage, was shot by Mr. Cunningham on March 16.—Notice of a species of Mason Ant on the Isle of May, by James M'Bain, M.D. Dr. M'Bain visited the Isle of May on Feb. 16, and obtained specimens of the ants, with eggs, larvæ, and attendant aphides. The ants since then had been kept in glass vessels, and one of the artificial Formicarias was exhibited to the Royal Physical Society. There appeared to be two species of ants in the colonies, one of which corresponded with the specific characters of the yellow ant, *Formica flava*, and, being in doubt as to the specific name of the brown ant, specimens of each were sent to the British Museum. Mr. F. Smith, a distinguished authority on the *Hymenoptera*, stated that "there are two species and two genera in the quill—one is *Formica flava*, the other is *Myrmica ruginodes*. The Formica is at once known by its single lamina, node (or scale) between thorax and abdomen; the Myrmica has two nodes, and also a sting. These ants commonly occupy opposite sides of the same hillock."—"On the Vegetable and Animal Life found in Natural Waters," by Dr. Stevenson Macadam.—Notes of a Tour in Auvergne, with an exposition of some of the most illustrative minerals of Central France; and remarks on the nidification of some species of the family *Mytilidæ*, by Mr. D. Grieve.—Analysis of "The Albert Limestone," Balmoral, by Mr. J. Falconer King.—Prof. Walley exhibited a curious example of malformation in a newly-born calf. The upper part of the skull was undeveloped, it had no apparent forelegs, only rudimentary and imperfect hindlegs, a rudimentary tail, and was otherwise imperfectly developed.

GLASGOW

Geological Society, March 21.—Mr. James Thomson, vice-president, in the chair.—"Some Recently-exposed Sections in the Paisley Clay-beds, and their Relation to the Glacial Period," by the Rev. William Fraser, of Paisley. These clays presented the following general order:—(1) Underlying all was the old boulder clay or till, the conditions of which were altogether unfavourable to life. It represented a cold, bleak, and in part tumultuary period. (2.) Immediately above this was a laminated clay, whose texture was in every way distinct from the preceding. It was generally shell-less and stoneless and beautifully laminated, the structure being at times so regular as to resemble the edge of a closed book, and specimens kept for a year or two have shown a texture and taken a polish like jasper. (3.) Above the laminated clay, which was useful in brickmaking, there occurs a thick bed in which shells of arctic and boreal types are found—*Tellina proxima*, *Panopæa norvegica*, *Pecten islandicus*, *Cyprina islandica*, and others too numerous to specify. Geologists loved the layer for its shells, which the brick-field proprietors regarded with an intense dislike. (4.) Next in order is the clay chiefly used in brickmaking. In it the glacial shells are not to be found; the last which disappears is the *Cyprina islandica*. But in these clays, indeed in all above the laminated clay, small and large stones, up to boulders of several tons in weight, are abundant. In some instances they bear longitudinal scratches, but they are deposited so irregularly that their lines lie in every direction; showing that while the origin of the lines or striæ was to be ascribed to the period and the processes of the boulder clay, the transport and distribution of the materials was connected with subsequent movements and the melting of floating masses of ice. At the close of the formation of this clay, and on its surface, appeared patches of a well-known shell, *Mytilus edulis*, the common mussel. (5.) Closing the series is a covering of varying thickness, and composed of various materials. There sometimes appeared near the surface a coarsely laminated clay, which had occasionally been mistaken by observers for the more finely laminated clay to be found at the commencement of the series. A long period, however, must have intervened between the two, and he suggested a careful scrutiny as to the facts connected with these two distinct clays.

DUBLIN

Royal Geological Society of Ireland, February 14.—Francis M. Jennings, F.C.S., in the chair. The honorary secretary, Dr. Alexander Macalister, read the annual report of the council. The following officers for the ensuing year were then elected by ballot:—President—Dr. Alex. Macalister. Vice-presidents—Earl of Enniskillen, Colonel Meadows Taylor, J. Emerson Reynolds, Rev. H. Lloyd, F.R.S., and Sir Richard Griffith, Bart. Treasurers—William Andrews and Dr. Samuel Downing. Secretaries—Rev. S. Haughton, F.R.S., and Edward Hull, F.R.S. Council—Sir Robert Kane, F.R.S., Alphonse Gages, B. B. Stoney, W. Frazer, Dr. Alex. Carte, W. H. S. Westropp, C. R. C. Tichborne, F.C.S., Rev. Maxwell Close, Francis M. Jennings, F.C.S., Dr. Ramsay H. Traquair, Dr. J. Barker, J. Ball Greene, W. H. Baily, F.G.S., W. Ogilby, F.G.S., and R. A. Gray.—Prof. Hull, Director of the Geological Survey of Ireland, read a paper on a remarkable fault in the New Red sandstone of Whiston, Cheshire. The position of this fault is marked on the geological survey maps of Lancashire (one inch map 80 N.W.) as forming the boundary between the little isolated tract of coal measures, one mile west of Rainhill Station and the New Red sandstone. The fault ranges in a nearly meridional direction, and on the west the upper coal measures, with spirorbis limestone (first discovered by Mr. Binney, F.R.S.), are brought to the surface, and on the east the upper mottled sandstone of the Bunter division of the Trias. The Corporation of St. Helen's, in order to increase the water supply of the borough, commenced sinking a well, on Mr. Hull's recommendation, at a distance of 200 yards from the fault in the New Red sandstone close to Cumber Lane Bridge.* This well was carried down 75 yards, and from the bottom a bore hole, 18in. diameter, was driven 35 yards farther; but at 104 yards from the surface it passed through the fault, and entered hard micaceous sandstone of a purple colour belonging to the upper coal-measures. As the horizontal distance from the outcrop of the fault where it crosses the railway is 200 yards, and the depth 104 yards, it appears that the slope of the fault is about two horizontal to one vertical, or 28° from the horizontal. The usual slope of the faults in South Lancashire being two vertical to one horizontal, such a result was unexpected, and as the thickness of New Red sandstone was thus reduced below the calculated amount the quantity of water obtained (about 400,000 gallons per day) was consequently much less than that required and anticipated.

February 22.—A paper was read from Mr. G. H. Kinahan "On the Formation of Valleys and Lake-basins, with special reference to Lochlomond." The author dissented from the views which had been put forth by several eminent geologists as to sub-aerial denudation; and held that the principal valleys both in Scotland and Ireland lay along lines of faults or fissures

* This site was selected, not as being the best for water supply, but the best available.

in the subjacent rocks. In the highlands of Scotland, so far as he had observed, there was not a valley, ravine, or lake-basin unconnected with a break or fault in the strata; and instanced particularly the Caledonian Canal, Loch Awe, Glencoe, Loch Fyne, and Lochlomond. He considered the deep parts of the latter lake were due to the meeting or crossing of two or more breaks, where consequently the rocks were fractured to a greater extent. Some of the dislocations, he was inclined to think, had been post-glacial.

PARIS

Academy of Sciences, April 15.—M. de Saint-Venant read a memoir on the intensity of the forces capable of deforming ductile cylindrical blocks placed under various conditions.—M. J. Boussinesq read a memoir on the influence of the centrifugal forces upon the varied permanent flow of water in prismatic channels of great width.—M. de Saint-Venant presented a note by M. E. Combescure on a process of integration by successive approximations of the equation

$$4\left(\frac{d^2\psi}{dxdz}\right)^2 + \left(\frac{d^2\psi}{dx^2} - \frac{d^2\psi}{dz^2}\right)^2 = 4K^2$$

in plastic dynamics.—MM. P. A. Favre and C. A. Valson presented a continuation of their researches upon crystalline dissociation. This paper contains the first part of their investigations on the alums.—A note by M. Lecoq de Boisbaudran on the spectrum of the vapour of water. The spectrum was obtained by passing an induction current through a tube filled with rarefied aqueous vapour; the spectrum consists of white stratifications, the light of which is resolved into four principal lines.—Several papers relating to auroras were read, including a note by M. Chapelas on polar lights observed in Paris on the evening of April 10; one by M. Tarry, communicated by M. Le Verrier, on the prevision of magnetic auroras by means of terrestrial currents, as applied to the aurora of April 10 by M Sureau; a general investigation of auroras by M. Heis, including a long list of parallel occurrences of such phenomena in the northern and southern hemispheres during the years 1870 and 1871; and a note by M. Linder on the theory of auroras, in which the author concludes that they are electro-magnetic phenomena which have their seat chiefly in the upper regions of the atmosphere.—M. Loewy presented a note on the discovery of two new planets, 119 and 120, one observed in Paris on April 9 by M. Paul Henry, the other at Marseilles on April 10 by M. Borelly. The positions of these planets on April 11-13 are given.—M. Berthelot read a note on the heat of formation of the oxygenated compounds of nitrogen.—M. A. Gillot presented a claim of priority with respect to a paper read by M. Gruner on January 22 on the action of oxide of carbon on iron and its oxides.—M. Cahours presented a note by MM. L. Dusart and C. Bardy on the phenoles.—M. Boussingault presented a note by M. A. Muntz on the statics of the cultivation of hops, containing a statement of the elements assimilated by the hop plants grown upon thirty-eight ares, and upon one hectare of land at Worth.—A note by M. C. van Bambeke on the first effects of fecundation upon the ova of fishes and on the origin and interpretation of the mucous or glandular lamella in the osseous fishes was presented by M. de Quartrefages. The author stated, as the result of his researches, that under the influence of fecundation the germinal disc of the egg in osseous fishes divides into two layers, of which the upper one becomes segmented, whilst the lower one forms an intermediate layer between the segmented blastoderm and the vitelline sphere, and accompanies the former in its development around the latter. He regards the thin central portion of this intermediate layer as the homologue of the mucous lamella.—M. A. Gaudry read a paper on the fossil animals of the Léberon in Vaucluse. These fossils are chiefly mammalian, and present a remarkable anology to those of the Miocene deposits of Pikermi in Attica, investigated some years since by the author.—M. A. Brongniart presented a note by M. de Saporta "On the more precise determination of certain Jurassic Coniferous Genera by Observation of their Fruits." The genera here referred to are *Brachyphyllum*, *Pachyphyllum*, *Echinostrobus*, *Cunninghamia*, *Widdringtonia*, *Palæocyparis* (a new genus proposed for some species described as belonging to *Thuyites*), and *Phyllostrobus* (a new genus allied to *Thuja*).—M. de Quatrefages communicated a note by MM. E. Massenat, P. Lalande, and Cartailhac "On the Discovery of a Human Skeleton of the Reindeer period at Laugerie-Basse in the Dordogne."—M. A. Milne-Edwards read some researches upon fossil birds, containing a sort of summary of the results of his long-continued investigation.—M. L. V. Turquan submitted to the judgment of the Academy the description of an apparatus for indicating the presence of fire-damp in mines.

BOOKS RECEIVED

FOREIGN.—(Through Williams and Norgate.)—Anatomische-systematische Beschreibung der Alcyonarien, 1te Abtheilung, die Pennatuliden: A. Kölliker.—Morphologie u. Entwickelungs-geschichte des Pennatulidenstammes, nebst allgemeinen Betrachtungen zur Descendenzlehre: A. Kölliker.—Beiträge zur Anatomie der Plattwürmer: Sommer u. Landois.—Iodex der Petrographie u. Stratigraphie der Schweiz u. ihrer Umgebungen: B. Studer.—Geschichte der Himmelskunde nach ihrem gesammten Umfange, Parts 1-3: J. H. v. Mädler.—Hydra, eine anatomische entwickelungs-geschichtliche Untersuchung: Dr. N. Kleinenberg.

DIARY

THURSDAY, APRIL 25.

ROYAL SOCIETY, at 8.30.—On a Supposed Periodicity in the Elements of Terrestrial Magnetism, with a Period of 26¼ Days: The President.—Contributions to Formal Logic: A. J. Ellis, F.R.S.
LONDON INSTITUTION, at 7.30.—On the Effects of Certain Faults of Vision on Painting, with especial reference to the Works of Turner and Mulready: Dr. Liebreich.
ROYAL INSTITUTION, at 3.—On Heat and Light: Prof. Tyndall, F.R.S.

FRIDAY, APRIL 26.

ROYAL INSTITUTION, at 9.—On the Genius and Character of the Modern Greek Language: Prof. Blackie, F.R.S.E.
QUEKETT MICROSCOPICAL CLUB, at 8.

SATURDAY, APRIL 27.

ROYAL INSTITUTION, at 3.—The Star-Depths: R. A. Proctor.
GOVERNMENT SCHOOL OF MINES, at 8.—On Geology: Dr. Cobbold.

SUNDAY, APRIL 28.

SUNDAY LECTURE SOCIETY, at 4.—On Geographical Influences on History: Prof. John Young, M.D.

MONDAY, APRIL 29.

ZOOLOGICAL SOCIETY, at 1.—Anniversary Meeting.
LONDON INSTITUTION, at 4.—Elementary Botany, with special reference to the Classification of Plants: Prof. Bentley, F.L.S.

TUESDAY, APRIL 30.

ROYAL INSTITUTION, at 3.—On the Development of Belief and Custom amongst the Lower Races of Mankind: E. B. Tylor, F.R.S.

WEDNESDAY, MAY 1.

ROYAL INSTITUTION, at 2.—Annual Meeting.
SOCIETY OF ARTS, at 8.—On Telegraphy without Insulation, the means of cheapening Internal Communication: H. Highton.
MICROSCOPICAL SOCIETY, at 8.

THURSDAY, MAY 2.

ROYAL SOCIETY, at 8.30.
SOCIETY OF ANTIQUARIES, at 8.30.
LINNEAN SOCIETY, at 8.—On *Alibertia edulis*: Señor Correa de Mello.
CHEMICAL SOCIETY, at 8.
ROYAL INSTITUTION, at 3.—On Heat and Light: Prof. Tyndall, F.R.S.

CONTENTS

ERRATUM.—Mr. J. J. Hall requests us to correct an error in the "Contents" of our last number, whereby he is described as "F.R.S." instead of "F.M.S."