NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

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INDEX.

An asterisk (*) indicates that a figure is given.

NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

No. 17. Vol. III. JULY, 1893.

NOTES AND COMMENTS.

The Museums Association.

THE season for annual congresses has arrived, and in Britain the Museums Association meets first among scientific bodies. This year, as we have already announced, the meeting is to be held in London, under the presidency of Sir William Flower, K.C.B., who delivers the opening address on July 3. The Association has already done much towards discussing the methods of Museum arrangement, and agreeing upon concerted action in various matters; but a meeting in the metropolis, where Museums, backed by the resources of the Government, have attained their greatest development, ought to lead to particularly important results.

Truly enough, there is much to be done in reforming many of the provincial Museums, if their efficiency is to be maintained and extended in accordance with modern ideas. Last year we had occasion to remark upon the peculiarities of some of the older foundations; [1] and, notwithstanding all the efforts of the Museums Association, we fear it will be long ere the collections "illustrating the travels of the local gentry" give place to the adequate representation of the Natural History of the neighbourhood in which each Museum is situated. Even newly-founded institutions, such as that at the Brassey Institute, Hastings, which begin well and with good intentions, soon find the difficulty of refusing to accept "curios" from local travellers; and until the governing bodies of the Museums are composed exclusively of scientific men who understand such matters, it is hopeless to expect any noteworthy reform.

We cannot help thinking, however, that the greatest difficulty in attempting reform results from the ignorance of most of the governing bodies of the Museums as to what constitute the qualifica-

[1] Natural Science, vol. i., p. 321.

B

tions and duties of a Curator. If the Museums Association could make its voice heard on this subject in the Town Councils of the land, it would be doing good service. Very frequently a Librarian is supposed to supervise the Museum ; he may have an assistant for the actual work, but even if that assistant be an expert, he is allowed no independent judgment. Only within the last year, the Committee of one great public Museum in Britain refused to take the advice of an expert and appoint a trained Curator, preferring the claims of a respectable tradesman, who happened to have local influence, and who may or may not prove a success in his new duties. The climax was reached in our experience, however, a few months ago, when the following advertisement appeared in a contemporary—not in an ordinary newspaper, but in a journal that comes regularly under the notice of scientific men :—

> WANTED.—A resident Curator, Meteorological Observer, and Caretaker for the Museum and Library Buildings of the Royal Institution of Cornwall, Truro. Salary, £50 per annum, with rooms, coals, and gas. Applications in Candidate's own handwriting, stating Age and Scientific Qualifications, with Testimonials and References, to be forwarded, not later than February 14, to Major Parkyn, Truro.

We have no information as to the result of this munificent offer to a trained naturalist, but if anyone could be found at the price, it would be of great interest to the Museums Association to learn from Major Parkyn the nature of the "scientific qualifications" he obtained so cheaply. Even if the salary were adequate, it would be difficult to find any one man competent to undertake so many duties; and if the funds of the Royal Institution of Cornwall are so low that it cannot afford more than its present expenditure, it might at least be satisfied with a caretaker to keep things free from dust and mildew until better days.

Curators who know anything of their work cannot be had for less pay than a gardener or coachman, whatever the people of Truro and similar towns may think; and when the Museums Association has duly established that fact, it might profitably direct attention next to the question of "honorary curators." In theory it is, no doubt, an excellent idea to have a committee of specialists to assist the curator in the scientific details of the various departments over which his work ranges; and in many instances it is not possible otherwise to ensure accuracy in the labelling of the specimens. In practice, however, so far as our experience goes, the honorary curator is usually an ornamental personage who never looks at the cases from any other point of view than that of a draper arranging his shop window. We know a certain "honorary curator of the department of conchology" in one of our largest provincial museums, who had not even heard of Woodward's "Manual" until long after he had entered upon office. When such nonentities are appointed, they simply meddle in general questions of administration and arrangement, which ought to be solely under the control of a competent curator; and the sooner the system now in vogue is abolished, the better will it be for the development of the museums.

We heartily wish success to the Museums Association in its efforts to benefit the community in these and other directions; and under the brilliant patronage with which the London meeting is favoured, much progress ought to result from the proceedings of this year.

The Antarctic Whaling Expedition.

As many of our readers are doubtless aware, a whaling expedition comprising four ships—the "Balæna," the "Diana," the "Active," and the "Polar Star" left Dundee in September last to try their fortune among the whales and seals of the Antarctic Ocean. Their destination was the region visited by Ross in his third Antarctic voyage in the years 1842–43; and the prime object of search the southern Right Whale (*Balæna australis*). Seeing how rarely the Antarctic regions are visited, and how imperfect is still our knowledge of their geography and natural history, it was arranged that the medical and some of the other officers of the expedition should endeavour to make observations, and collect specimens, so far as the exigencies of their other duties permitted.

The expedition has now returned safe and sound, and one of its members has contributed a preliminary account of its doings to the *Times*, from which we take the following summary.

It appears, indeed, that while much important information has been acquired as to the meteorology of the Antarctic, and some additions made to geographical science, the general scientific results are not so satisfactory as was hoped would have been the case; this being mainly due to the time of all persons on board being fully occupied in capturing and flensing seals, during the period when the vessels were in the most interesting regions. During a short stay in the Falklands, the officers learnt that the Antarctic fox is becoming well-nigh extinct; while Darwin's prophecy that the wild horses and cattle would eventually be replaced by the more remunerative sheep is being rapidly fulfilled. Moreover, through incessant pursuit, the fur-seal, although still found, is comparatively rare.

The first iceberg was sighted in lat. 59° 18′, and the same day (December 16) Cape petrels, and other members of the same order, were met with in shoals, while the seas were found to be absolutely swarming with various species of finner whales. Soon after the first seal was descried and promptly despatched; the species is not identified, although it is stated to be one of those first discovered by Ross. On the 22nd, flocks of sheathbills were met with, while, on the next day, the presence of the lovely white petrel and the olive colour of the water indicated the proximity of the pack-ice.

Although no right whales were sighted, a large number of seals, of various kinds, were taken, and by the middle of February the vessel in which the *Times'* correspondent sailed had secured its full quota. Three different kinds of seals were taken, which are described as the "large seal," the "white Antarctic seal," and the leopard seal. The species of the last is, of course, certain, while the second would appear to be the crab-eating *Lobodon carcinophaga*, and the first is probably Widdel's sea. (*Leptonychotes weddeli*). Besides these, it is stated that the party came across a fourth kind of seal, described as of large size, with a small head, small flippers, thick blubber, and a rather woolly pellage. Possibly this may prove to be the rare *Ommatophoca rossi*, at present known only by two immature examples; but, in any case, it is to be hoped that skulls and skins of all these seals have been brought home in a state fit for scientific examination. The writer describes the Antarctic seals as very foolish creatures. "The present generation have never seen man, and they survey him open-mouthed and fearful, during which process they are laid low with club or bullet. Sometimes they are so lazy with sleep that a man may dig them in the ribs with the muzzle of his gun, and, wondering what is disturbing their slumbers, they raise their head, which quickly falls pierced with a bullet. There may only be one seal on a piece of ice, which is usually the case with the larger kind; but the smaller kinds lie in half-dozens and tens, and as many as 47 were seen on one piece. Seldom do any escape—one cartridge means one seal."

Protoplasm.

At a recent meeting of that very up-to-date society, the Oxford University Junior Scientific Club, Professor Ray Lankester exhibited a fine specimen of a creeping plasmodium. On the general subject of protoplasm he said that a very common error at present was to abuse the word protoplasm. It is *not* the name of any chemical substance, but of the living slimy material which is to be seen round the nucleus constituting the substance of all living cells. To this substance the name was first given some forty years ago, and for this substance the name must be retained. It was quite true that probably a definite chemical substance of very high complexity was present in all protoplasm—a substance to which the name "Proteil" might be applied if that name had not been appropriated for the hypothetical basal form of all matter. This chemical substance might never be isolated and worked out. By its very nature it was extremely unstable—changing its composition, vanishing, dissolving almost as you looked at it—and in living protoplasm besides this substance there must be present always a number of chemical substances, some of them on the way up to the highest point, some on the downward path. The late Professor Moseley always insisted on a similar view. He used to say that the protoplasm probably of every different kind of cell, and certainly of every different plant and animal, was different. There were a great many protoplasms, for protoplasm was simply the name of a physical appearance always found in living cells, and in actual chemical composition varying from cell to cell, from organism to organism, and from moment to moment; but underlying all this flux of living material, no doubt, there was one definite chemical compound identical in all living matter.

Recent Work on the Foraminifera.

The chief activity in the study of Foraminifera has of late years been removed to Italy, and we are indebted to Fornasini, Tellini, Dervieux, Malagoli, and Mariani for a great number of papers, chiefly on the Tertiary forms, of more or less importance. The last one to hand is by Dervieux, who writes upon the Tertiary *Frondiculariæ* of Piedmont (*Bull. Soc. Geol. Ital.*, vol. xi. (2)). There are eleven forms figured, most of which are well-known, and some of which are common both to the Chalk and Tertiary deposits elsewhere. Three are named as new, but surely fig. 17 is *F. medelingensis* of Karrer.

Schlumberger, of Paris, has contributed to the *Mémoires* of the Zoological Society of France (vol. vi.) another of those elaborate researches into the Miliolidæ—*Monographie des Miliolidées du Golfe de Marseille*. The material was supplied to him by M. Marion, and was obtained from the "zone à Bryozoaires," in the gulf of Marseilles, at a depth of 30–40 metres.

Schlumberger picked out the Miliolines, numbering about 20, and has described and figured them with his customary exquisite care. No *Biloculinæ* were found, but there were 3 *Spiroloculinæ*, one receiving a new name (*inæquilateralis*); 1 *Sigmoilina* (*costata*, n. sp.); 4 *Triloculinæ* (*marioni*, n. sp.); 10 *Quinqueloculinæ* (*stelligera*, n. sp.); 2 *Massilinæ* (a new genus "of which the early chambers are arranged in five symmetrical planes, as in *Quinqueloculina*, while the last are placed in two planes or a single common plane of symmetry passing through the axis of the poles as in *Spiroloculina*") the species being *secans* (*Quinqueloculina*, d'Orbigny, no. 43 Tableau Méthodique) and *annectens*, n. sp.; and 3 *Adelosinæ*. The illustrations are given in four plates, photographed from drawings by the author, and the paper is further illlustrated by thirty-seven of those beautiful sections that M. Schlumberger has delighted us with so often. We regret to see our old friend *Q. secans* disappear, but we have long since recognised the jeopardy of all *Miliolinæ*, which now cannot be safely named without the precaution of making sections and studying the internal arrangement of the test.

The most interesting event to be chronicled is the return of the prodigal son, M. E. Van den Broeck, who has fed for long off Japanese husks and now returns to his foraminifera. Van den Broeck has published an "Etude préliminaire" (*Bull. Soc. roy. Malac. Belgique*, vol. xxviii.), and an "Etude sur le Dimorphisme des Foraminifères et des Nummulites en particulier" (*Bull. Soc. Belge Geol.*, vol. vii.). The latter is an elaborate paper of 41 pages, and discusses at length the views of Hantken, De la Harpe, Munier-Chalmas and Schlumberger, Tournouer, Fischer, and others, and states the following conclusions:—*A*. That dimorphism in the Foraminifera is met with in all types where conditions of life are favourable to existence. *B*. Dimorphism is "of absolutely initial origin and in relation with the origin of life itself." *C*. The characteristic of dimorphism in the Foraminifera is, as a general rule, furnished by the contrast existing between the dimensions of a section of the adult of two forms of the same species, and the dimensions *in an inverse sense*, and infinitely more accentuated, of the section of the central or initial chamber. *D*. From the similarity of external characters, etc., it is easy to assure oneself that the two forms *A*. and *B*. not only belong to the same group or zoological subdivision of one genus, but even to the same species. *E*. As to the difference in shape of the two forms in the adult state, the character of the spire, form and number of chambers, they never present but secondary variations. *F*. The difference of the initial stage appears due to the fact that *the Foraminifera reproduce themselves by two distinct processes* (*fission* and *gemmation*) [previously pointed out by Fischer (1870) and Dollfus (1890); see M. Van den Broeck's paper, p. 15]. *G*. Gemmation is well-known among the Foraminifera, Fission among the Freshwater Rhizopoda. The shells of Foraminifera may be a true colony, of which the individual of each successive chamber may, by a kind of fission, be the product of the division of the preceding, remaining attached to the colonial skeleton by a non-liberated test. *H*. Whether the differential characters noticed in the dimorphous tests are connected with the shape and disposition of the chambers or the external characters, or whether these characters are connected with the spire, form, number, or grouping of the chambers in the forms *A*. and *B*. of the same species, or even the respective abundance of *A*. and *B*. in the same habitat, we ought to state, in favour of the principle of initial dimorphism, that these differential characters find, without trouble or argument, their *raison d'être* precisely *in the natural consequences of initial dimorphism*, due to the contrast in the reproductive processes. *I*. The sole objection that may be opposed to this interpretation, consists in the fact that to justify two initial forms in the same species it is necessary to prove—which does not yet appear to have been fully done—the existence of embryonic stages or at least young individuals showing, as in *A*. and *B*., large and adult forms, the divergence into megasphere and microsphere of the initial chambers. *J*. One may be able reasonably to add to the phenomenon of expulsion, setting aside the last external chambers of Nummulites, the fact that never has anyone noticed or figured an adult Nummulite, entire and intact, showing a mouth or opening perfectly preserved. *K*. Looking at the question of the duality of the process of the Foraminifera, as much as at that of the support afforded to the Theory of Fission by the method of individual examination of the contents of each Foraminiferal chamber, the importance which must be attributed to the *question of nucleus among the Foraminifera* demands a complementary system of observation. For further detail and argument, the student must be referred to Van den Broeck's paper, as we have already exceeded the space at disposal; but the communication is so interesting and important that it appears an obvious advantage to quote at this length.

The Distribution of Seeds.

We are glad to observe the tendency of biologists at Cambridge to wander more and more from the long-prevalent exclusive fashion of section-cutting, and to attempt to solve some of the wider problems in the science. Another interesting item of research in this direction was communicated to a recent meeting of the Cambridge Philosophical Society, when Messrs. J. C. Willis and J. H. Burkill recorded their observations on the flora of the pollard willows near the University town.

The plants occurring in the tops of the willows near Cambridge have been recorded during the last few years, and amount to 80 species, occurring 3,951 times altogether in about 4,500 trees. Of these 80, only 18 form more than 1 per cent. of the total number of records. The rest have only a small number of records. As Loew has pointed out in a recent paper, these plants are of interest from the points of view of distribution of seeds and of epiphytism. Classifying them according to means of distribution, we find that 19 species have fleshy fruits; 1,763 records (44·6 per cent.) of these occur. Three species with burrs have 651 records (16·4 per cent.); 34 species with winged or feathered fruit or seed have 996 records (25·1 per cent.); 7, with very light seeds, have 421 (10·6 per cent.); and finally, of plants whose means of distribution is poor or somewhat doubtful, we have 17 species with 120 records (2·9 per cent.). It is thus shown very strikingly how the various distribution-mechanisms succeed, only the better ones showing in the list in any numbers. The bird-distributed plants figure much higher here than in such cases as, for instance, the flora of the churches of Poitiers (Richard).

The observations show clearly the fact that a seed is only carried a short distance by its distribution mechanism. Plants were always found upon the soil, within 250 yards at most, of those found in the trees. An analysis was made as far as possible of the birds' nests found in the trees, and pieces, often with ripe fruits, of many plants in the list were discovered in them, so that probably this means of distribution is of some importance.

With regard to epiphytism, Loew considers these plants as beginning this mode of life. Like true epiphytes, they possess good methods of seed distribution. Their position does not call for any special means of supporting themselves, and the supply of humus is plentiful. *Mycorhiza*, which Loew found on many, was not observed in the few examined. The size of many of the shrubs, *e.g.*, Elder, *Ribes*, Rose, etc., was very remarkable; some elders were 3 inches thick or more, and as much as 12 feet high. Experiments are in progress upon the growth of plants in willow humus.

Expeditious Scientific Research.

Referring to Biology at Cambridge, we cannot refrain from recording that at the meeting of the Zoological Society of London on June 6, the exhibition of an egg of *Ornithorhynchus* was made the occasion for some indignant remarks on the action of a member of the University, in suppressing and rendering useless for the purposes of science, materials collected at the public expense in the haunts of *Ornithorhynchus* and *Ceratodus* in Australia. If Mr. W. H. Caldwell is unable to make use of the specimens he was deputed to collect, it is high time that he reported to that effect to the public institutions that endowed his research; and it would be well if in future the Royal Society added some clause to the conditions on which the Government grant is awarded, preventing any repetition of such circumstances. As biologists, we are glad to learn that one of the German Universities has undertaken the work of supplying us with information concerning the early stages in the life-history of the duck-bills and *Ceratodus*, and will shortly publish the preliminary results; as Britons, we cannot but regret that it should be necessary to depend upon the resources of a foreign nation for the solution of some of the most important problems in the Natural History of one of our own colonies.

Wild-flowers and Drought.

In the *Journal of Botany* for June, Mr. C. B. Clarke gives a few notes on the effect of the warm, dry spring on our wild plants. "A great number of annuals," says the writer, "usually regarded as autumn annuals, have already run their course." On May 6 a quantity of *Valerianella olitoria* was collected, not only in ripe fruit, but with the whole plant whitened as seen in corn-fields after harvest. The fruits contained abundance of perfect seeds ripe. On the same day various banks in Surrey were seen to be golden-yellow with the Mouse-ear Hawkweed (*Hieracium pilosella*). Two other Hawkweeds, (*H. murorum* and *H. sylvaticum*) were gathered in flower, the latter also "off flower" within the next eight days; their normal flowering season, according to the "Students' Flora," is July–September. The Heath *Erica tetralix* was almost as precocious, for a large bundle was picked in full flower at Bournemouth on May 18. Examples in full flower of nearly all the common autumnal weeds were collected, such as *Centaurea scabiosa*, *Erythræa centaurea*, *Jasione montana* and others.

One effect of the drought was the shortening of the stems of many plants, thus the Marsh Thistles were flowering plentifully with stems less than one inch long. A small Pond-weed (*Potamogeton polygonifolius*) had flowered abundantly with stems one to two inches long on nearly dry mud, while the same species, abundant also in the streams, showed no signs of flowering; the warmth of the water causing, on the other hand, a luxuriant growth of stem and leaves. This was also generally found to be the case with water-plants, which have not flowered much earlier than in some other seasons. It is noticeable that in no case was there indication of blossom on the Sundews or *Hypericum elodes*, though all three plants were observed in every state of moisture from saturation to desiccation.

Dr. Forsyth Major on Squirrels.

In criticising a certain book in a previous number of this Journal, we had reason to call attention to the circumstance that, in our opinion, the author had selected too comprehensive a title. We have now to mention a case where an author has done himself injustice by choosing a heading to his paper which errs in the opposite direction. We refer to a paper just published in the *Proceedings of the Zoological Society* (1893, pp. 179–215) by the well-known palæontologist, Dr. Forsyth Major, under the designation "On some Miocene Squirrels, with Remarks on the Dentition and Classification of the Sciurinæ." Any person reading the title would, we submit, imagine that the fossil squirrels were the important part of the paper; whereas, as a matter of fact, they form but an insignificant portion of its contents. The paper is, indeed, not only a re-classification of the squirrels, but also comprises a most important discussion on the question of trituberculism and the origin of the various types of mammalian dentition; the title giving not the slightest inkling of the latter portion of its contents. Although, at present, we have been able to give it only a hurried perusal, while we are by no means sure that we agree with many of its conclusions, the paper appears to us one of the most important that have been published for some years.

In regard to classification, Dr. Major, following the suggestions of some previous writers, utterly scouts the division of the squirrel-family into the Sciurinæ (true squirrels and flying squirrels) and Arctomyinæ (susliks and marmots); and divides it into the Sciurinæ Pteromyinæ, and Nannosciurinæ. The former embraces the grooved-toothed squirrels (*Rhithrosciurus*), the spiny squirrels (*Xerus*), the true squirrels (*Sciurus*)—the latter genus including the chipmunks (*Tamias*)—and the susliks and marmots. The second sub-family is represented by the three genera of flying squirrels; while the third includes the pigmy squirrels (*Nannosciurus*), which have been generally placed in *Sciurus*, from which they are distinguished by their dormouse-like dentition. It will thus be seen how radical are the changes in classification.

Fossil teeth indicate that flying squirrels were abundant in the Miocene; and their occurrence there, with other circumstances, leads the author to conclude that they have originated quite separately from the Sciurinæ, even if they belong to the same family.

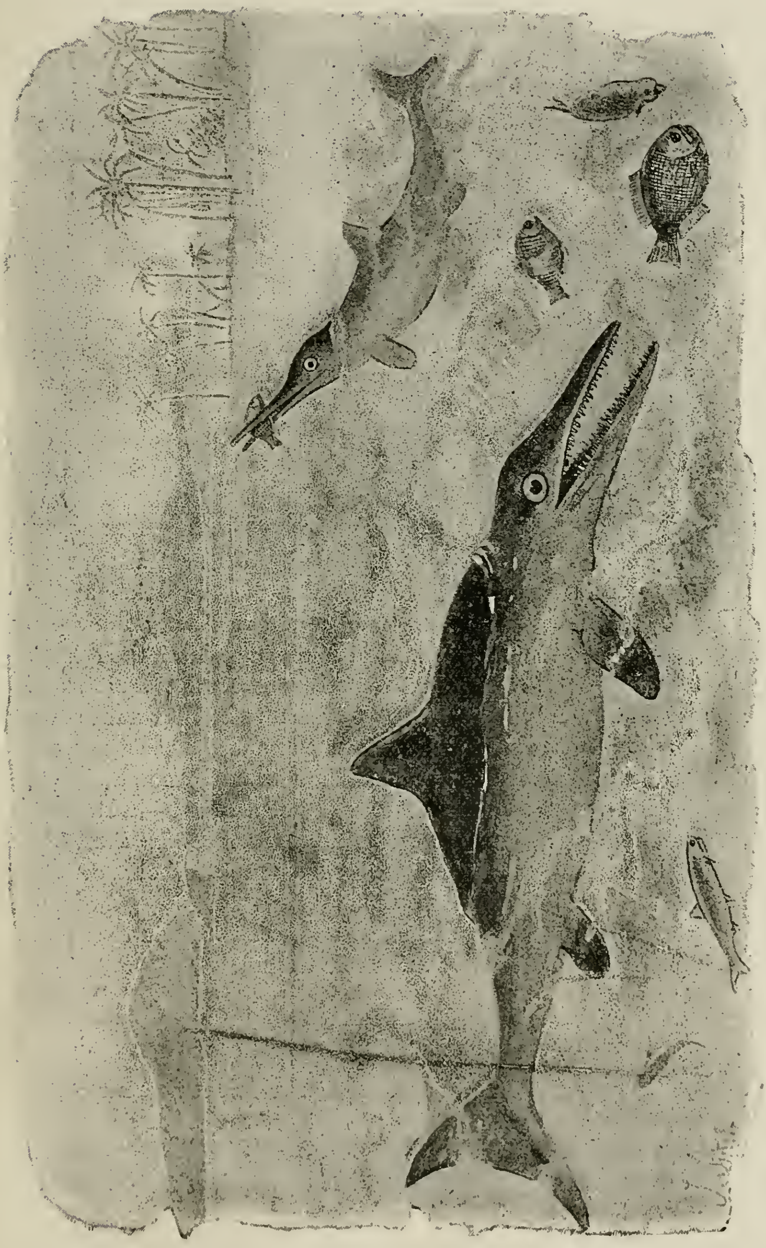
To enter into a discussion of the author's opposition to trituberculism would greatly exceed our limits; but we may mention that he believes that the Eutherian dentition has been derived from a direct modification of the multituberculate type, and that trituberculism is a secondary feature due to reduction. Certainly, one of the figures of a squirrel's molar reminds us very strongly of a molar of the duck-bill, and, as far as it goes, is suggestive of the derivation of both from a type in which the cusps were arranged in parallel longitudinal series. To believe, however, that "*Microlestes* may prove to be a remote ancestor of the Eutheria," if it had incisor teeth at all like those of its ally *Plagiaulax*, is, however, very difficult. We shall await with interest what the Transatlantic palæontologists have to say on the subject.

The Bison in the Caucasus.

Hitherto there has been much uncertainty as to the localities where the bison still remains in a wild state in the Caucasus, and naturalists are accordingly much indebted to Dr. Radde for clearing up this point in a brief communication published in the *Proceedings of the Zoological Society* (1893, pp. 175–7). The Doctor tells us that the animal is confined to the district around the sources of the Laba and Bjellaja on the north side of the range, extending eastwards from the former locality to the springs of the Selentschük. It is, however, everywhere scarce, being generally met with only in twos and threes, although occasionally as many as five have been seen together, and once the tracks of a party of seven were detected. The survivors appear to have lost their original settled habits, and to have become wanderers in their last refuge, having, in some cases, crossed the main ridge of the Caucasus and made their appearance on its southern flanks. As there is, unfortunately, but too much reason to fear that the Caucasian bison will, ere long, have ceased to exist, we are glad to hear that Dr. Radde hopes to be able to publish an exhaustive memoir on the anatomy, distribution, and habits of this animal.

A Restoration of Ichthyosaurus.

Some months ago Mr. Lydekker referred in these pages (vol. i., p. 514) to Dr. E. Fraas' discovery of a specimen of the extinct marine reptile, *Ichthyosaurus*, showing the form and proportions of the dorsal and caudal fins. In the second edition of the Rev. H. N. Hutchinson's "Extinct Monsters," to the issue of which we referred



RESTORATION OF ICHTHYOSAURIAN REPTILES. By J. SMIT.
(From H. N. Hutchinson's "Extinct Monsters," second edition.)

last month, Mr. Smit has attempted a new restoration of the animal based upon this discovery; and by the courtesy of the author and publishers (Messrs. Chapman and Hall) we are able to reproduce the sketch on the previous page. The curious downward bend of the vertebral column into the lower lobe of the caudal fin is well indicated; and a life-like drawing of this kind affords a much more vivid conception of the animal than the original outline sketch of Dr. Fraas. We may add that Mr. Smit has executed other new restorations for Mr. Hutchinson's second edition, and criticisms of the first issue have been turned to profitable account.

Cavern Explorations.

Little has been done of late in the exploration of caverns—perhaps there remains now little to be learned concerning the ancient fauna of which they provide evidence. Dr. Eberhard Fraas, however, has been examining the Irpfel cave in the valley of the Brenz, Würtemberg (*Zeitschr. deutsch. geol. Gesell.*, 1893), and Mr. E. Harlé has published, in the *Comptes Rendus* of the Natural History Society of Toulouse, an elaborate summary of the distribution of the Quaternary mammals in the south-west of France.

The cavern in Würtemberg investigated by Dr. Fraas proves to have been a hyæna-den, with some evidence of occupation also by the cave bear and man. The animals found distinctly indicate a Steppe-, not a Forest-fauna; and it is quite evident that the reindeer was contemporaneous with the mammoth, rhinoceros, and hyæna. It is also definitely proved, from the occurrence of flint-flakes, that man lived with these animals in Southern Germany.

The researches of Mr. Harlé also show that the country to the north of the Garonne was a great steppe—not a forest—at the same period. Remains of the Saiga antelope and *Spermophilus* are widely spread, and in four instances they have been found associated. The steppe is believed to have been replaced by forests at the end of what is termed the "Magdalenian" period, when there is reason to suppose that the climate became warmer and more humid.

Our Monthly Selections.

The office boy, or his equivalent so far as scientific matters are concerned, is more conspicuous than ever in the newspapers this month. We must, however, content ourselves with three selections from the Natural History provided for the edification of the general public.

Last month the *Westminster Gazette* tried to discourse learnedly on botanical questions, and provided some amusement for the readers of the June *Journal of Botany;* but this month it has even excelled itself by an attempt at a popular exposition of the Insect House in the London Zoological Gardens. The fact that the journalist failed to count the legs of the stick-insect correctly may be excusable, if he were only a casual visitor; but he might at least have mastered some of the rudiments of zoology before he recorded his observations on the Atlas moth. This, we are told, "is rather bird than insect, and the wings have a depth and softness of plumage which seem to bring it within the animal kingdom." If moths are not animals, and if birds are animals that have evolved from insects, it is a misfortune the *Westminster Gazette* does not expound to us its new philosophy.

For further strange notions of zoology we may turn to the *World* of May 31, where we read:—"I have entitled this story 'Ponsonby and the Pantheress,' because Ponsonby's nocturnal visitor undoubtedly belonged to the genus Carnaria, species *F. pardus*, the *Pardalis* of the ancients." The confusion between *order* and *genus* is delightful, while the meaning of the mystical "*F.*" is apparently left to the intelligent reader.

Finally, in a notice of the skeletons of a man and horse recently mounted side by side in the Natural History Museum, published in the *Graphic* of June 10, we obtain the following information:—"In the *carpus* (wrist-bones) of the horse a further reduction in the number of bones as compared with man takes place, but it is especially in the bones which constitute the hand in man (*metacarpus*) in which extraordinary modifications will be noticed. It will be apparent that what is popularly known as the knee in the horse's foreleg is not the knee, but the middle finger in the human hand, greatly developed and strengthened to sustain the enormous weight of the horse's fore-quarters. It has the same number of parts as the human middle finger, and is terminated by the hoof, which corresponds to the nail of that finger." The uninstructed public are likely to be greatly benefited by the information that the terms metacarpus and hand are synonymous; while when they read that the horse's knee is the equivalent of the human third finger, even their proverbial faith in the so-called popular scientist is likely to receive a rude shock.

The *Daily Chronicle* of June 10 quotes some curious observations recorded by Dr. Hovey on the effect of the pure air of the Mammoth Cave of Kentucky on the senses of smell and hearing. After coming out of the cave, the odours of the outside world, of the trees, grass, and flowers, are so strangely intensified that the different trees can be distinguished by scents inappreciable after breathing the ordinary air for half-an-hour. This effect is, to many sensitive natures, so over-powering that some of the visitors suffer from headache and nausea by a too sudden change from the densely oxygenated atmosphere of the cavern to the more impure one outside it. Hearing is also stimulated. All of the cave animals—which are blind—are so keenly endowed with this sense, that they dart away with rapidity should even a grain of sand fall on the surface of the water. Yet, if perfect silence is preserved, so unconscious are they of the presence of an enemy that they can easily be captured by the hand or net when swimming about.

Early in the century lions were comparatively abundant in India, but at present they appear to be exclusively confined to the Gir of Kathiawar, where they are preserved by the Government. Two fine specimens are reported to have been recently shot by the Governor of Bombay and a party of sportsmen.

We have received from Professor Duns a copy of the abstract of his paper on the early history of some Scottish mammals and birds, appearing in the *Proc. Roy. Soc. Edinburgh*, session 1892–93. It is a plea for the more "adequate description of the environment, physical and vital, of the mammals and birds earliest recorded by Scottish observers." The author enumerates the old publications to which reference may be made.

In reference to the drought, the *Gardeners' Chronicle* of June 10 gives statistics showing the unusually early ripening of the first Pea crops. The record (supplied by Messrs. Hurst & Son) extends over ten years, and is as follows:—1883, June 21; 1884, June 26; 1885, June 24; 1886, June 24; 1887, June 25; 1888, June 26; 1889, June 16; 1890, not given, probably about June 18; 1891, June 27; 1892, June 15; 1893, June 4–6 at the latest.

In the previous week's issue of the same journal the statement is reported that, owing to the drying up of ponds and watercourses, thrushes in the southern counties, especially the dry county of Hampshire, have been forced to build their nests without the usual mud lining. The question is put: How many droughty years would be needed to destroy the habit entirely?

In reference to Mr. Clarence King's recent ingenious estimate of the age of the Earth (Natural Science, vol. ii., p. 81), the Rev. Osmond Fisher contributes an interesting article to the current number of the *American Journal of Science* (ser. 3, vol. xlv., pp. 464–468), pointing out that the rigidity of the Earth cannot be relied upon in estimating its age. He considers the supposed fact that the globe is rigid, still remains to be proved; hence no argument can be founded on it.

The so-called primæval fossil, *Eozoon canadense*, has been subjected to so much destructive criticism that there are now few believers in its organic nature; but until this month no analogous structure had been recorded as found under conditions that could explain the origin of so curious an arrangement of different minerals. At the meeting of the Geological Society of London on June 7, however, Dr. Johnston Lavis exhibited some ejected blocks of metamorphosed limestone from Monte Somma displaying a perfect eozoonal structure. They have been studied most carefully by Dr. J. W. Gregory and himself, and their microscopical characters correspond in all details with those of the original Canadian specimens. In many cases, on account of their freshness, the Monte Somma blocks exhibit some of the pseudo-organic structural details, such as the stolon-tubes, in far greater perfection than does the true so-called *Eozoon canadense.*

In the *Transactions of the Connecticut Academy* (vol. ix., March, 1893), Dr. C. E. Beecher has a paper on the "Revision of the Families of the Loop-bearing Brachiopoda." He arrives at the conclusion that the Terebratellidæ certainly appeared before Jurassic times, because they were well represented by several characteristic genera (*Kingena*, *Ismenia*, *Zellania*, and *Megathyris*); that among recent species, several separate generic and specific names may have been given to stages of growth of a few species; and that species must be based upon surface ornaments, colour, and form, with certain limits, and genera only upon structural features developed through a definite series of changes, the results of which are permanent in individuals evidently fully adult.

A second paper by the same author follows, on "The Development of *Terebratalia obsoleta*, Dall." This is illustrated by three plates, of which the first shows the development of the brachial supports in the Terebratellidæ, dealing chiefly with the *Megathyrinæ*, the *Dallininæ* and the *Magellaniinæ;* while the second and third plates are almost entirely concerned with the same structure in *Terebratalia obsoleta.* Like so much of Dr. Beecher's work, this is thoroughly worked out, and presents points of interest as well to the general reader as to the specialist.

In the *Zeitschr. für romanische Philologie* for April there is an interesting paper on the Italian popular names of the Bats, by Dr. C. J. Forsyth Major. Dr. Major is, fortunately, both a naturalist and a philologist, and one can therefore depend upon his determinations and researches. Not only from Italy itself has the author busied himself in rescuing these interesting words, but also from Corsica, Vaud, Savoy, the Jura, the Vosges, Dauphiné, Provence, etc., etc. Among the words in use that Dr. Major traces to the Latin *Vespertilio*, are:—Tuscany, *vipistrello*, *vispistrello*, *pipistrello*; Florence, *pripistrello*; Figline, *pimpistrello;* Prov. Massa, *spertello;* Prov. Macerata, *spiridillo;* Tenerano, *papastrello*; Premilcuore, *baibastrel*; Imola, *balbastrê*; Padora, *barbastrégio;* Venice (ant.) *barbastregio*, *barbastelo*, *barbastrigo*, *barbastrillo;*

Tyrol (Ital.), *barbustèll*. The reference of several names from the dialects of Corsica and Sardinia to that of the Basques may meet with some opposition. We are glad to read the references to Homer, and to find due recognition of his observations on natural objects. This is not the first of these interesting contributions from Dr. Major's pen, and it is to be hoped that his researches in European Miocene Mammals, on Samos and on Madagascar will yet leave him time to favour us with more of this laborious and interesting philology, the greater part of which is the result of personal investigation in the localities themselves.

In the June issue of the *New Review* the Rev. S. A. Barnett makes some remarks on University Extension Teaching which are of interest as regards Natural Science. The Societies are just now threatened by the demand for popular teaching. Signs are not wanting that the popular lecturer is supplanting the thorough teacher, and while the former fills the largest hall in the neighbourhood, gives his twelve lectures, gets in fees which cover expenses, and satisfies everybody, the latter, " who handles his subject humbly and would make his students humble " and eager to learn, draws small classes and hardly gets employment. University teaching, says the writer, must dare to be unpopular, sending its teachers not to meet but to create a demand. " One of the best teachers in East London, whose mark remains after ten years, never drew a large class and never was popular; but he was thorough in his work, a master of his subject, and by his enthusiasm made others enthusiastic." Of course, this means that University teaching must be endowed.

Another danger is the absence of organisation. " Each society keeps, as it were, a shop, of which its lectures are the stock, and they are labelled with attractive titles. Active local secretaries for country towns or London districts come to the shop to buy, and are often led to the purchase only by appearances." There is thus a want of continuity, and the practice of taking " tit-bits " here and there is far too prevalent.

A concerted plan of action between the three great Societies and the establishment of definite series of courses stretching over two or three years would avoid the serious waste of time and effort which under existing conditions must sometimes occur.

In one respect, the second Romanes' lecture at Oxford, like the first, was disappointing. Professor Huxley, like Mr. Gladstone, has a great reputation for the manner and form as well as for the matter of his addresses, and many of the audience of this second lecture, like many of the audience of the first, came expecting a brilliant display of the arts of oratory; but the Sheldonian theatre is among the worst auditoria in the world, and during the greater part of the lecture those not immediately in front of the lecturer had some difficulty in hearing. We notice elsewhere the matter of the lecture, which has since been printed and published by Macmillan & Co.

The Rev. A. C. Waghorne, late of New Harbor, Newfoundland, has a collection of Newfoundland and Labrador plants for disposal. After 17 years' labour as a missionary, he is obliged by ill health to retire, for a time, from regular clerical work, and, being entirely without means of subsistence, hopes to derive some income by the sale of his plants. He is not himself a botanist, but his plants have been named by Messrs. Macoun, Warnstorf, Vasey, Bailey, Farlow, Hervey, Bebb, Underwood, and Eckfeldt. He proposes to issue complete lists of all he has collected in Newfoundland and on the Labrador, and to distinguish those of which he has material to spare. He hopes to have his list of mosses and lichens, at least, ready this month; the rest will probably not be prepared till the winter. A list of Sphagnaceæ has already been issued. As Mr. Waghorne will spend the summer in Labrador, applications for specimens should be made to the Rev. J. H. Bull, Whitbourne, Newfoundland.

I.

Recent Researches on Plant Nutrition.

IT was well-known before 1771 that a candle, in burning, vitiates the air, and on that account will not continue to burn in a closed air-space; but Priestley, in his "Experiments and Observations on different kinds of Air," records for the first time experiments made about this date which show that the restoration of air thus vitiated can be brought about by the intervention of plants.

Perhaps it was, to some extent, by accident that he was led to try the effect of placing a sprig of mint under a glass jar standing over water. The sprig he observed continued to grow for some months, and after the experiment the air inside the jar was found to be capable of supporting the combustion of a candle, and a mouse placed in it showed no signs of inconvenience. This was considered a remarkable fact, since it was to be expected that as air was known to be necessary for the support both of vegetable and animal life, the effect on it by the life-processes would be the same in both cases—in one case, deterioration was known to take place, and in the other, the same results, it was thought, ought to follow. The fact that a candle would burn in air which had supported plant-life for some time seems to have suggested to him the idea that possibly air injured by processes of combustion may be restored by the action of vegetation, and he accordingly devised experiments to test this hypothesis.

Sprigs of mint and spinach were enclosed in air in which a candle had been allowed to burn itself out, and it was found that the air was restored to its original condition, so far as combustion was concerned, in from two to eight days.

The processes of combustion and respiration were not understood at the time. Priestley seems to treat them as quite distinct, and again goes on to flatter himself that he has "hit upon one of the methods employed by Nature" for the restoration of air vitiated by *animal respiration*.

That some great operation was continually being carried on in Nature for the restoration of air rendered noxious by breathing was obvious, and Priestley, like many others, had tried various experiments in order to elucidate the matter—absorption by the earth, sea, and fresh-water, condensation by pressure, action of various gases, *e.g.*, sulphur dioxide, as well as the influence of light and heat. All these seemed to yield no definite results, but ultimately the action of plants was found to be the chief cause. Mice were allowed to die in a closed glass vessel, and the confined air was then divided into two parts, and enclosed in glass vessels placed upside down over water, so that no air could enter or escape. Into one, sprigs of mint were introduced, and after eight days a mouse placed in it lived perfectly well, whereas, in the other vessel not so treated, mice died almost instantly.

By many experiments extending over two or three years, the reversal of the effects of animal respiration by plants was established and also the discovery made that green aquatic plants placed in sunlight in water containing "fixed air" (carbon dioxide), give off "dephlogisticated air" (oxygen). When the evolution of oxygen stopped, the introduction of fresh-water was followed by a brisk development of more bubbles, thus supporting the view that the oxygen came from the "fixed air" dissolved in the water, and not from the leaf.

Ingenhousz, in 1779, confirmed and extended Priestley's observations. The latter had suggested that the main purifying effects upon the air were due to the mere growth of the plant, but Ingenhousz conclusively proved that the influence of the sun's light upon the plant was an essential factor; in darkness, although kept at the same temperature as during the daytime, no purification took place, but rather the reverse.

Senebier, working independently about the same time as Ingenhousz, went over the same ground and laid stress upon the fact that only the green leaves give off oxygen in sunlight, and also stated more precisely that the amount of oxygen evolved was dependent upon the amount of carbon dioxide present in the water used, distilled water yielding practically no gas at all.

The observations, so far, had been chiefly qualitative in character, and it was not until 1804 that De Saussure's quantitative investigations were published.

By this time Lavoisier's famous researches had given an intelligible account of the processes of combustion and respiration—the composition of carbon dioxide was ascertained and the part played by oxygen in producing it was fully understood. The more extended use of the balance had also asserted the indestructibility of matter, and under the new *régime* progress became possible in all branches of science.

De Saussure, taking advantage of the facts and spirit of this new chemistry, introduced accuracy and terse completeness into his work. Various species of plants were placed in air of known volume and composition, and after exposure to sunlight for thirty-six hours the air was examined and found to have been altered in composition. The volume remained the same, but the carbon dioxide had disappeared and a nearly equal amount of oxygen had been substituted for it.

He also established the absolute necessity of carbon dioxide for plant growth, pointed out its injurious nature when present in more than comparatively small amounts, and, finally, made the most important discovery that, while the decomposition of carbon dioxide and exhalation of oxygen are going on in sunlight, the plant is increasing considerably in dry weight. This increase is, moreover, greater than can be accounted for by the fixation of carbon alone, the excess being due to the simultaneous fixation of the elements of water—hydrogen and oxygen—supplied from the ground through the roots.

With all this accumulated knowledge, it is somewhat difficult to understand why chemists and plant physiologists did not at once begin to inquire into the nature and composition of the product first formed in green leaves as a result of the processes described. The whole question of carbon-assimilation by leaves had been well opened, but the experiments were regarded by many of the chief workers in this field as having not even the remotest connection with nutrition of plants, and were consequently cast aside, or at least neglected and almost forgotten. This was largely due to preconceived notions respecting plant food, and the state of chemical knowledge at the time was such as to check progress. The early Aristotelian teaching—that the sole food-substance of all plants was "humus"—prevailed, and chemists were almost compelled to adopt this view because of their conceptions regarding the formation of organic compounds. Animal, vegetable, and mineral chemistry had been found to be an inconsistent classification, as some compounds belonging to the first two groups were found to be identical; but a more disastrous division into organic and inorganic chemistry took place, the latter dealing with elements under the influence of physical and chemical forces as at present understood, whereas into the former was introduced a "vital force" which helped to get over difficulties otherwise not amenable to treatment, and at the same time made attempt at a reasonable explanation somewhat superfluous. It was dogmatically asserted that substances formed under the influence of the vital principle could not be produced artificially, and, unfortunately, Wöhler's synthesis of urea in 1828 was incomplete, and remained for a long time the only case tending to support an opposite opinion. The existence of such a force, which was even capable of the creation of plant-ash out of nothing, made careful observation unnecessary, and it was not until after the complete destruction of the humus-theory of plant nutrition by Liebig in 1840, and Boussingault a little later, that the mind was free to look at the significance of the work of the early experimenters.

From this period down to the present time the assimilation or preparation by plants of organic compounds from simple substances, such as carbon dioxide, water, and ammonia, supplied to them from the surrounding atmosphere and soil, has been the subject of numerous and elaborate investigations. The recognition of the decomposition of carbon dioxide with elimination of oxygen by the green parts of plants as an assimilatory process, dates from the beginning of the century; the essential importance of light was also recognised by the early workers, but it is to Sachs that we owe the discovery of the connection between this carbon-assimilation and the production of a definite substance—starch—in the chloroplasts of the leaf.

The occurrence of starch granules in the chloroplasts was made known by von Mohl in 1837, but Sachs, in 1862, observed that it was induced by the same conditions as those known to be necessary for the decomposition of carbon dioxide by plants, namely, exposure to light, and his memoirs of 1862–4 deal with the influence of light on starch production in the chloroplasts. He shows that where carbon dioxide is being absorbed there is a constant production of starch when the plant is exposed to sufficiently bright daylight, a subsequent disappearance and removal of this starch from the leaf at night, and for the first time explains the connection between carbon-assimilation and starch production by the leaves. Godlewski, in 1873, strengthened Sachs' conclusions by the demonstration that no starch-grains are formed in the leaf-chloroplasts when carbon dioxide is missing from the surrounding atmosphere, even when the plant is exposed to bright light.

When decomposition of carbon dioxide and exhalation of oxygen is going on, starch in nearly all cases makes its appearance in the green chloroplasts of the leaf, yet Sachs' work does not show that starch is the immediate and direct product of this assimilation, although the fact that the volume of oxygen given off equals the volume of carbon dioxide absorbed was at first taken as a support for this hypothesis. That a complicated compound, such as starch, should be actually formed directly from carbon dioxide and water was highly improbable, and since Sachs' first papers there has accumulated a large amount of evidence pointing to the formation of some much simpler carbohydrates, which are transformed into starch by the chloroplast, probably only when they are in excess of the immediate requirements of the cell for its own nourishment and respiration—that starch is, in fact, always a reserve material and is only the first visible product in a large series beginning with carbon dioxide and water.

Böhm (1877) showed that starch can be formed in the chloroplasts from materials which have been elaborated elsewhere than in the cell in which starch-production is going on, and Schimper (1880) paved the way for further research in this direction by his discovery of the starch-forming corpuscles or amyloplasts in parts of plants not green (tubers, stems, &c.) and their morphological identity with chloroplasts. These bodies are colourless and are concerned with the formation of starch out of ready-formed sugars supplied to them, and can be changed into chloroplasts by exposure to light in many cases.

Schimper's conclusion that the chloroplasts of the leaf parenchyma have not the power to produce starch except by assimilation from the atmosphere was shown to be incorrect by Meyer (1885-6), who found that starch is readily formed by the mesophyll chloroplasts when the leaves are floated on solutions of dextrose, levulose, cane-sugar, galactose, and even mannite and glycerine.

Baeyer's suggestion that the first formed product of the carbon assimilation is formaldehyde—a compound which undergoes easy condensation into sugar-like substances—has been considerably supported by recent work on the sugars, notably by Emil Fischer's syntheses of dextrose and levulose, although attempts to feed chloroplasts with aldehyde or its derivatives have met with only partial success.

The most recent and at the same time one of the most important contributions ever made to the study of plant-metabolism, is that of Messrs. Brown and Morris, communicated to the Chemical Society in April last. In their paper they deal with the occurrence of starch, diastase, and sugars in leaves, and the work will undoubtedly take high rank in the annals of plant physiology.

Sachs, in 1884, endeavoured to determine the weight of starch produced during the day by estimating the dry weight of two definite equal areas of the leaf, taken symmetrically from opposite sides of the midrib, one in the morning and the other in the evening after the day's exposure to sunshine. The assumption was made that the differences in dry weight were due to gain in *starch*, and the results showed in the case of a sunflower an increase of 9·14 grains per square metre in ten hours. Similar experiments carried out on plucked leaves with their petioles immersed in water yielded greater increase in weight, owing to the dissolution products of starch not being able to flow into the stem under these circumstances.

Messrs. Brown and Morris point out that this method merely indicates the *total assimilated products* which are manufactured under the given circumstances, and not necessarily the amount of *starch* produced. Sachs thought otherwise, as he assumed that all the assimilated products of the leaf must at one time or other pass through the form of starch in the chloroplasts, an assumption for which there is no evidence. These observers bear testimony to the applicability and accuracy of Sachs' dry weight method of estimating the rate of assimilation, and go on to determine the amount of starch which is present at any one time in the leaf, and the proportion which it bears to the total products of assimilation.

Leaves were rapidly dried at 75-80° C., or killed by the action of chloroform vapour, and then finely powdered and the fats and chlorophyll extracted with ether. Ten grams of the powdered leaves were then taken and extracted with alcohol; the starch in the powder was then gelatinised by hot water, and finally determined by estimation of the maltose and dextrin produced after the action of diastase upon it for about two hours.

Two different batches of Tropæolum leaves picked after the same amount of isolation were found to contain 6·408 and 6·545 per cent. of starch respectively, results which are representative of the accuracy which can be attained by their method.

Estimation of the starch in the leaves of sunflower while attached to the stem, at five a.m. and again at five p.m. on a fairly bright day, showed an increase of 1·4 grams of starch per square metre during this period, while the total assimilation-products actually formed during this time, estimated from the dry weight of similar leaves separated from the stem and their petioles immersed in water, was over twelve grams per square metre. It is here seen that if Sachs' view is correct—that all the assimilated materials pass through the form of starch—the formation of the latter substance and its dissolution must be carried on at an exceedingly quick rate. It is, however, far more probable that much of the assimilated material passes out of the leaf into the stem of the plant without ever being organised into starch by the chloroplasts.

Messrs. Brown and Morris go on to discuss the occurrence and periodic variation in the amount of diastase present in leaves, and in this direction they have achieved much success. Incidentally, it may be observed that the whole of their work arose out of the discovery of diastase in dry hops when they were prosecuting an enquiry into the cause of secondary or "cask fermentation" in beer.

The function of the ferment diastase in the breaking down of starch into the soluble substances sugar and dextrin when seeds are allowed to germinate, has been recognised almost from the time of its discovery and isolation by Kirchoff, Payen and Persoz, and Dubrunfaut at the beginning of this century, and a study of this dissolution of starch led to the generally expressed belief that the same or a similar ferment is concerned with the disappearance of starch from the leaf chloroplasts.

Attempts, however, to isolate diastase from green leaves were not made until about 1877, when Kosmann and Baranetzky took up the work. Small traces were found in the leaves of various species of plants by both observers, and Baranetzky expressed the opinion that the smallness of the amount present at any one time was to be attributed to its being used up as fast as it is produced.

Brasse, in 1884, also conclusively proved its presence in leaves, though Sachs in the same year, in his paper on assimilation, was unable to decide whether the dissolution of starch in the leaf is due to some special power of the chloroplast, or to soluble ferments.

Wortmann, in 1880, from experiments of the most questionable character, concluded that diastase is missing from practically all leaves, and even when present plays no part in starch dissolution, the process being attributed by him to the action of the living contents of the cell.

Messrs. Brown and Morris find, however, that, instead of leaves possessing little or no diastase, there is always present more of this ferment than is sufficient to transform all the starch they can ever contain at one time, and in some cases the amount is such as would transform a quantity of starch more than equal to the whole dry weight of the leaf itself.

The conflicting results previously obtained respecting this subject are almost entirely due to the fact that although diastase is soluble in water, it cannot readily be extracted from the leaf, on account of the tenacity with which it is held by the protoplasm; the presence of considerable amounts of tannin also renders it insoluble. However, by specially drying the leaf, and using the powdered tissue itself, it is possible to demonstrate the presence of leaf diastase in all cases, and these experiments give proof of its identity with the malt ferment; the production of the sugar maltose and dextrin from soluble starch is brought about by both alike.

The diastatic activity of the leaves of various species of plants was compared, the results being very remarkable. The leaves of leguminous plants stand far ahead of all others in this respect, the garden pea heading the list. The relative diastatic activities are stated in the amount in grams of maltose, which 10 grams of the air-dried leaf will produce from soluble starch in 48 hours at 30° C. Twenty-six species were examined, and the following are typical results:—

Pea (*Pisum sativum*)	240·30
Scarlet Runner (*Phaseolus multiflorus*)	110·49
Clover (*Trifolium pratense*)	89·66
Vetch (*Vicia sativa*)	79·55
Potato (*Solanum tuberosum*)	8·16
Sunflower (*Helianthus annuus*)	3·94
Onion (*Allium cepa*)	3·76
Cotyledon umbilicus	4·61
Funkia sinensis	5·91
Hymenophyllum demissum	4·20

The work points to a connection between starch-forming power and the diastatic power of leaves, but must be extended before any general statement can be given.

Changes in diastatic power of different portions of the same leaf at different times of the day prove that the ferment increases as fast as the leaf is being depleted of its starch and other assimilated products. Considerable accumulation takes place in the dark. They believe that the secretion of diastase is only carried on by the protoplasm when the soluble nutriment of the cell has been used up or carried out into the stem. At this starvation point diastase is secreted in order to bring about the supply of soluble food-materia from the reserve starch in the chloroplast.

The last part of their memoir is concerned with the sugars of the leaf, and this part of the work indirectly supports the unexpected view that cane-sugar is the first sugar to be synthesised in the assimilatory process, and at the same time establishes almost beyond doubt that the starch dissolution in the leaf is mainly brought about by the action of the diastase which they have proved is so generally present.

It is in the determination of the nature and variation in the amount of the different sugars present in the leaves of plants that Messrs. Brown and Morris have laid the foundation of a more complete and extended knowledge of the complex chemical changes occurring in plant carbon-assimilation, and their work may be characterised as the first which has placed the subject on more than a hypothetical basis.

When diastase acts upon starch the result is the formation of a sugar—maltose—and dextrins, which latter on further action are also changed into maltose. If, therefore, this ferment is concerned in the dissolution of starch in leaves it ought to be possible to prove the existence of maltose in these organs. This they have satisfactorily accomplished by processes which are too technical to be introduced here, but about which there can be no doubt. The only other sugars found to be present in Tropæolum leaves were cane-sugar, dextrose, and levulose. Simpler pentoses were looked for but none discovered. Experiments were afterwards directed towards the quantitative determination of these sugars in the leaves at different times of the day and night, in the hope that some light would be thrown on their function, translocation, and genetic relation to each other, and to the carbohydrate starch, and to ascertain something about the sugars which stand between the first products of assimilation and starch and those which are to be looked upon as derived from the latter by chemical transformation.

A considerable number of leaves of Tropæolum were picked at 5 a.m. and separated into two batches. One set (*a*) was dried at once and the carbohydrates estimated, and the leaves of the other set (*b*) were placed with their petioles in water and exposed to sunlight until 5 p.m., and then a similar series of leaves (*c*) was picked after exposure to sunlight during the same period, but still attached to the plant. The starch and sugars of these three samples were determined with the following results :—

	a	*b*	*c*
Starch	1·23	3·91	4·59
Sugars—			
Cane-sugar	4·65	8·85	3·86
Dextrose	0·97	1·20	0·00
Levulose	2·99	6·44	0·39
Maltose	1·18	0·69	5·33
Total sugar percentage on dry leaf	9·79	17·18	9·58

From (*b*) and (*c*) it is seen that the production of starch in cut leaves does not go on so rapidly as in leaves which are allowed to remain attached to the plant.

Sachs first noticed that the dry weight of a plucked leaf increases considerably when isolated and its petiole placed in water, and this increase is seen to be due mainly to the accumulation of sugars, not starch. The total sugars in (*b*) have been nearly doubled, and this is mainly due to the increase in cane-sugar, a result which militates against the generally received opinion that a glucose is the principal "up-grade" sugar first formed by the leaf.

The changes which the sugars undergo when assimilation is prevented are well brought out by the following experiment. A considerable number of leaves were plucked in the afternoon of a fairly sunny day, and dried immediately (*a*). Another similar set (*b*) were taken and placed in the dark for twenty-four hours, their stems being dipped in water. The result of the analysis gave :—

	(*a*)	(*b*)
Starch	3·693	2·980
Sugars—		
Cane sugar	9·98	3·49
Dextrose	0·00	0·58
Levulose	1·41	3·46
Maltose	2·25	1·86
Total sugar percentage in dry leaf	13·64	9·39

Total loss of sugars and starch in (*b*) = 4·96 per cent.

The loss or destruction of starch and cane-sugar is due to respiration. The disappearance of the cane-sugar has been accompanied by increase in levulose. Seeing that the conditions of the experiment preclude the possibility of formation of levulose by assimilation, this fact points to the conclusion that the cane-sugar is first inverted, and the resulting dextrose and levulose are used up at uneven rates by the respiratory processes, the former, together with a little maltose, contributing chiefly for this purpose. The experiment, at the same time, renders it almost certain that the increase of levulose in (*b*), previously described, is derived from cane-sugar inversion, and not from direct assimilation.

Looked at from all points of view, Messrs. Brown and Morris's results undoubtedly point to the conclusion, so far as Tropæolum is concerned, that cane-sugar is the first sugar to be synthesised as a result of the carbon-assimilation by the leaf, and that only after a certain concentration of this substance in the cell-sap is starch formed as a more stable reserve material. In the leaf, cane-sugar suffers inversion, and ultimately passes from cell to cell and out into the stem as dextrose and levulose, the starch being at the same time translocated as maltose.

The whole work is full of suggestion, and is a model of consummate skill and accuracy. Plant physiologists have reason to be grateful for the improved methods of research devised by these two workers, and the foundation which has been laid for further investigation, as well as for the results obtained. Taken in conjunction with the extraordinary haziness manifested in the chemistry of even modern plant physiology, the work tends to emphasise the necessity that those who would further progress in this department of biology must first be chemists.

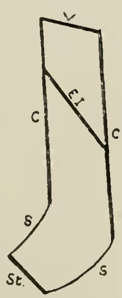
Considering the great labour and patience which these researches must have involved, it may, perhaps, seem ungrateful to express regret that the function of chlorophyll meets with no consideration. Notwithstanding the researches of Engelmann, Pringsheim, Timiriazeff, and many others, this part of the subject is little understood, and now that the morphological and physiological identity of the coloured chloroplast and colourless amyloplast is completely established so far as starch-production from already assimilated materials is concerned, the quite separate function of constructing a proto-carbohydrate from carbon dioxide and water by the former, due to its possession of chlorophyll, is of especial interest.

J. Percival.

II.

The Respiration of Birds.

JOHN HUNTER, I believe, held that birds did not draw breath during flight, but merely used the air stored in the sacs. Extraordinary as this view may seem, he was led to adopt it by what is a very real difficulty, namely, that the movement of the breast in breathing would seriously derange the machinery of flight. Mr. Edmond Alix, in his *Essai sur l'appareil locomoteur des Oiseaux*, gives what I believe is the right solution of the problem, viz., that the bird during flight moves its back, not its breast, up and down in the act of breathing; but he does not explain how this is effected. My own investigations have led me to the conclusion that the muscular movements necessary to flight themselves to a great extent bring about the enlargement and contraction of the thoracic cavity without which respiration cannot take place. The muscle called the latissimus dorsi, which arises from the vertebræ, and, after passing over the scapula, is inserted in the humerus, raises the backbone during the down stroke of the wing, bringing the bird's body into the position which is required for horizontal flight; and not only is the backbone raised and brought to, or nearly to, the horizontal, but at the same time the ribs are straightened, and this causes an expansion of the air-sacs. The effect of this raising of the back may be seen if a bird is suspended by its backbone, when the weight of the sternum and the breast muscles hanging upon the ribs causes them to straighten out at the junction of their costal and sternal portions. The muscles, also, which pass downwards to the ribs from the posterior ends of the scapulæ, will help towards the same result, and, besides that, will broaden the roof of the thoracic cage, the upper part of the rib tending to become more horizontal when the lower part is raised. The action of these last-named muscles is assisted by the movement of the scapula. It will be found that during the down stroke, when the front margin of the wing is lower than the hind margin, so that the undersurface of the wing looks downwards and backwards, the anterior end of the scapula is depressed very slightly, and this causes a considerable raising of the posterior end. Thus in birds, as in many crustaceans, progression itself aids greatly the process of respiration. Another point worth noticing is that in this the great pectoral plays an important part, for it is due to the great strength of this muscle that the contraction of the latissimus dorsi causes a movement of the body and not of the wing. All possible work is put upon the pectoral muscles, so that the centre of gravity may be low down, a point of great importance in a flying-machine; but though the flight muscles help so largely towards respiration, of course much is done by what are commonly spoken of as the respiratory muscles. The levatores costarum, which I have found very highly developed in the domestic pigeon, arising from the vertebræ, passing backwards and attaching to the ribs some way down, tend to make the upper part of the rib horizontal, thus broadening the chamber beneath. The triangularis sterni, which arises from the inside of the sternum, from its anterior lateral end, and attaches to the sternal ribs, works towards the same object from below. The action of the external intercostals is not so easy to understand. In man they raise both the ribs, which they connect, and at the same time raise the breastbone; but in birds the pectoral muscles render the sternum heavy, and, what is far more important than this, the weight of the body hanging upon the wings during flight, and the pressure of the wings inwards upon the coracoid bones and clavicles, must make the breastbone practically immovable. What, then, will happen when the external intercostals contract? It must be remembered that the contraction of a muscle tends to shorten the distance between its two ends. The muscular band, passing obliquely back and downwards, will lower the front rib to which it is attached, and raise the hinder; the loose muscular connection of the vertebral column with the shoulder blades allowing some depression of its anterior and a corresponding raising of its posterior part. Moreover, the backbone of most birds that I have examined bends downward easily, and through a considerable arc just anterior to the ilium. The raising of the hindmost ribs which articulate with the vertebræ behind the point where the bend takes place, will aid the vertebral muscles in straigthening the back. The accompanying diagram illustrates the working of the external intercostals.



c. = Costal rib.
e.i. = External intercostal muscle.
s. = Sternal rib.
st. = Sternum.
v. = Vertebral column.

The contraction of the muscle will lower the anterior and raise the posterior rib.

Wishing to test these conclusions by experiment, I suspended a freshly-killed pigeon by its wings, and inflated the air-sacs by means of a blowing tube. The backbone, a little in front of the thigh joint, moved rather more than half-an-inch—the movement of the sternum being almost too slight to measure. I do not wish to represent this experiment as one of much value. However, the conditions of flight were so far reproduced that the weight of the body was hanging upon the wings, and so hindering the movement of the breast, while leaving the back free ; but there was none of the pressure—which during flight must be very great—of the wings upon the coracoid bones and clavicles. But would not the only effect of this pressure be to render the sternum and the bones united with it still less ready to move ?

It will be observed that I have said nothing upon the vexed question of the action of the internal intercostals. They are so weak that the result of their action, whatever it may be, cannot be a large factor in the problem of respiration. When a bird is standing or running, it is obvious that the back is fixed to the legs, so that it cannot rise and fall. Clearly, then, the process of respiration must be different, and if a bird be watched when it is filling and emptying its air-chambers in order to utter a loud note, the moving forward of the breast may be easily seen. As I have said, the main obstacle to its motion during flight is, not its weight, but the tension and pressure at the shoulder joint. With regard to the abdominal air-sacs, the raising of the hinder-quarters by the legs, when the bird is standing, will itself expand them. The air will be expelled by the contraction of the abdominal muscles, which will draw the sternum inwards. In connection with this, I may mention that Mr. F. E. Beddard has found the oblique septum muscular in the puffin, and I myself have confirmed the observation. When a bird is sleeping with its breast resting upon the perch, or lying upon the ground with nearly all its weight upon its breast, it must adopt a method of breathing similar to that employed in flight. Often geese and other birds, while lying on their breasts, will exert a great deal of voice-power, and then the rising of the hinder part of the back is distinctly visible. The fore part may appear to rise a little, but this appearance is due, I think, to the raising of the ribs by the levatores costarum. The legs, apparently, have no weight upon them, so that the back is not held down as in standing. The muscles called into play must be the external intercostals and the triangularis sterni.

F. W. Headley.

III.

Observations on Certain Marine Animals.

I.—A New Protective Device of *Maia squinado.*

I HAD occasion recently to watch closely the habits of the large Spider Crab, *Maia squinado*, while in captivity in the Aquarium tanks of the Jersey Biological Station. The first individual obtained, a female, was temporarily placed in a large bare tank, unfurnished with either boulders or sea-weeds. A thin layer of fine shingle about an inch in depth covered the bottom. *Maia* for a while was very restless, seeking, apparently, for a suitable place of concealment. Finding none, she at length took up a position in the centre of the tank, scratching a hollow in the shingle wherein to lie. After a short interval of rest she began an admirable plan of concealment. First with one chela and then with the other the crab selected and lifted carefully pebble after pebble and deliberately placed them, closely fitted together, upon the surface of the carapace, the rough spinous nature of this being admirably suited to hold them in position. Rarely a pebble would fall off, so carefully did *Maia* poise them and so steady did she hold her body. At the end of a quarter of an hour the red spiny back was converted into a little mound of pebbles, and the crab absolutely hidden.

Now *Maia*, in common with several allied species, such as *Pisa tetraodon*, *Acheus cranchii* (Spence Bate *Ann. and Mag. Nat. Hist.*, 1866), and *Stenorhynchus*, etc., has the habit of decorating and trimming the carapace and limbs with scraps of weed hooked often quite artistically on to the recurved bristles that beset these parts. This with obvious protective intention. But *Maia* at home seems never to frequent pebbly localities. Its habitat is in the sand and under the weedy, root-matted edges of the *Zostera* banks. Hence it is reasonably to be inferred that the protective device described above was independently thought out to meet the special exigencies of the occasion. Several other *Maia* subsequently placed in the same tank performed precisely the like operation.

It is noteworthy that all these were either females or immature males; the adult males are normally very large, usually at least twice the size of females, and are provided with long and powerful chelæ, which make them independent of the subterfuges adopted by the weak small females. An interesting point, too, is that the weak chelæ of these mimics are quite smooth, without the recurved bristles of the other limbs, and hence are unlike them, never having any weedy trimmings.

II.—Colour Assimilation Among Fishes.

We already know a good deal about this, especially among the flat-fishes, so the following instances but go to swell an already long list. Still, these are so striking as to be worthy of permanent record.

Two tanks were used for experiment, one with dark background and bottom, well shadowed; the other bright, with a white mottled sand bottom. Several of the marine stickleback (*Gasterosteus spinachia*) were placed in each. To sum up the result briefly, those in the dark shadowed tank remained practically unchanged in colour, but those in the light-coloured tank had in greater or less degree lost their brightness and intensity of colouring. The beautiful gold bronze lustre so characteristic of these sticklebacks was lost, and the backs were mottled black and white, contrasting strangely with the nearly unbroken yellowish black of the dorsal surface of their friends in the dark tank.

In the dark tank had also been placed a number of Wrasses (Labridæ), and these showed fading all round, most marked in the bright greens and scarlets. As these colours are usually in combination with brownish marking, the fading of the bright hues meant a close approximation to the brown appearance of the bare conglomerate forming the rockwork of the tank. One fish especially beautiful at first—of a most brilliant scarlet and brown—faded to a dirty combination of pale olive green and brown, scarcely recognisable had the fish not been marked in a distinctive manner at the beginning of the experiment. The whole of these colour changes were effected within the remarkably short period of a week.

It may be that these instances of colour assimilation carry the key to the problem of colour variation or rather mimicry in the prawn *Hippolyte* (*Virbius*) *varians*.

Plaice (*Pleuronectes platessa*) have also shown rapidity of colour-change much more marked than I was prepared for. Some that were placed in a large shallow tidal pond where the colour of the bottom varies considerably and where a portion is often in deep shadow, show change from a uniform grey to a well-marked and intensely dark blotched appearance within a few seconds. Indeed, it is quite chameleon-like, so quickly is the transformation effected. In ordinary tanks where the light and the colour of the sand are stable, the plaice soon take the exact colouring requisite, and retain it without alteration so long as they remain in the particular tank.

III.—A Fighting Stratagem of *Palinurus*.

The Common Crawfish (*Palinurus vulgaris*) has many points of interest, and not the least curious is his plan of combat when matched with a powerful antagonist. Without chelate limbs, he seems weak and defenceless. One is at first inclined to commiserate this apparent want of means alike of offence or defence, especially in comparison with his kindred, the lobsters, armed so well with powerful seizing chelæ. That he had means of defence seemed probable; but it is only within the last few days that this was satisfactorily demonstrated. Without any particular intention in view, we had dropped a medium-sized lobster into the tank containing two large *Palinurus*. At first no sign was given, but in a little while we were attracted by a loud noise as of a skirmish, and had an inimitable object-lesson in Crawfish warfare. The larger of the two Crawfish apparently resented the intrusion of the lobster, and was determined upon ejection. There was a good deal of preliminary sparring, but the fight which promised to be protracted ended suddenly in a most unexpected manner. Making a sudden twist, the Crawfish got above the lobster crosswise, and suddenly snapping his powerful tail, jammed the body of his antagonist in the fold, thus impaling him on the sharp downward spikes of the pleura that are such conspicuous objects in a side view of *Palinurus*. The lobster was put quite *hors de combat*, for his body was terribly mutilated by the sharp spines, which had pierced his armour as though it were tissue-paper. Besides this instance, cases are known where persons, incautiously handling the Crawfish, have received wounds on the arm inflicted by similar sudden flap of the tail.

IV.—Abnormalities in *Haliclystus* (*Lucernaria*) *octoradiatus*.
(Preliminary note.)

This year, having occasion to examine a very large number of the Lucernarian most common in these seas, viz., *H. octoradiatus*, I was surprised to find a very large proportion to possess more or less strongly-marked abnormal features. These were divisible into two groups, the first where the colleto-cystophores were malformed, the second where these organs or the grouped tentacles were above or below the normal number eight.

The specimens gathered were in two lots, the one taken in February and March comprising large adult individuals $\frac{3}{4}$ to 1 in. in diameter, the other collected in the beginning of June, and made up of half-grown individuals, averaging half-an-inch across, but in which the ova, etc., were well advanced.

The malformed colleto-cystophores showed several interesting points, but the only one I wish to dwell upon now was a common case, where the summit of the organ showed very obvious homology with the ordinary capitate tentacles. In many the resemblance was but faint—a slight rounded swelling at the apex—with presence of a few of the ordinary nematocysts of the tentacles. From this stage there were all gradations up to a few rare instances where the apex of the c.-cystophore bore a perfect capitate tentacle precisely the same in structure to the normal tentacle, saving that the stalk was extremely short, just equal in length to the diameter of the head. All the examples of this variation were furnished by the lot gathered in June. The proportion was surprising. Out of 118 specimens examined but 40 were without thi abnormality in the c.-cystophores.

14	showed	one	colleto-cystophore	with	tentacle-crowned	apex.
15	,,	two	,,	,,	,,	,,
15	,,	three	,,	,,	,,	,,
9	,,	four	,,	,,	,,	,,
8	,,	five	,,	,,	,,	,,
5	,,	six	,,	,,	,,	,,
4	,,	seven	,,	,,	,,	,,
8	,,	all the	,,	,,	,,	,,

Otherwise these specimens showed but little mutability. Two colleto-cystophores were doubled and one specimen had an extra group of tentacles, while another lacked one.

In the larger specimens taken earlier in the year, the tentacle-crowned c.-cystophore phase was not observed, but there was extreme want of symmetry. Quite 33 per cent. showed variation in the number of colleto-cystophores or in the groups of tentacles. The greater number (more than half) were cases where the marginal organ was wanting in the space where normally it would be found; the others were instances of excess number of tentacle groups (accompanied usually by the presence of an equal number of excess colleto-cystophores) or of duplication—"twinning"—of one or two of the last-named bodies.

The conclusions to be drawn from the last mentioned abnormalities are obscure, but those where the colleto-cystophores bear the papillate appendix at the summit are certainly atavistic, explainable by the original derivation of these organs from ordinary papillæ.

James Hornell.

IV.

Recent Progress in Conchology.

SOME recently-published conchological treatises seem deserving of somewhat more than a mere passing notice in these pages, since they contain matter which deals with either the whole, or a large section, of the molluscan phylum.

To begin with, there is an interesting paper from the pen of M. Moynier de Villepoix (1) that furnishes an admirable summary of what is known concerning the formation and growth of the molluscan shell. It is not, however, confined to a summary, and among the new points which its author claims to have established is the mode of formation of the ligament in bivalves. This, it appears, is secreted by the epithelium of the dorsal suture, each of its constituent elements being formed by cells specialised for the purpose. With regard to his observations on the growth of the shell in *Helix*, we greatly fear that those relating to the special areas of secretion of the mantle, with the description of their cells, will have to rank as confirmatory only and not as discoveries, for if we mistake not these structures have already been dealt with some years ago by MM. Longe and Mer (2), the title of whose paper we miss from the valuable bibliography at the end of the memoir. Two other references seem also to have escaped the vigilance of the writer, namely Gray's principal and important paper on shell-structure (3), and the source of that naturalist's inspiration (albeit unacknowledged), Count Bournon's work on Calcite (4).

The next memoir to which we wish to allude is that by M. H. Fischer (5), a coming writer on molluscan anatomy and already noted for the careful nature of his work, who deals in a comprehensive manner with the morphology of the liver in the Gastropoda.

His principal results may be briefly summarised as follows:—In the Scutibranchs (*e.g.*, *Neritina*) the hepatic lobes of the embryo are equal and symmetrical. One genus only of the Prosobranchs, *Valvata*, which also resembles the Scutibranchs in possessing a bipectinate gill, displays the same regularity. In all other Gastropods the two lobes are unequal, either *ab initio* or soon after their formation, the left lobe being the larger. It sometimes happens in the adult (*Elysia*, *Arion*, *Buccinum*) that the balance is restored, but this acquired symmetry of a later stage has not the same morphological value that attaches to a like equality in the initial stages of the development of that organ. The liver in Prosobranchs undergoes a process of evolution comparable to that of the other organs; but the subdivisions that could be established for the Pectinibranchiata on its variations would not entirely correspond with those founded on other characters, such as the radula, nervous system, kidney, or shell. Hence the difficulty of founding a classification on the sum of the characters, and the desirability—M. Fischer considers, since the liver is not suitable for the purpose—of basing a systematic arrangement on some single feature, such, for example, as the radula.

The classification of the Mollusca, especially from a phylogenetic aspect, has received of late very much attention, and quite a forest of genealogical trees have been constructed for this sub-kingdom, or portions thereof, by Von Jhering and others, to the great bewilderment of the unlucky student who attempts to thread their maze.

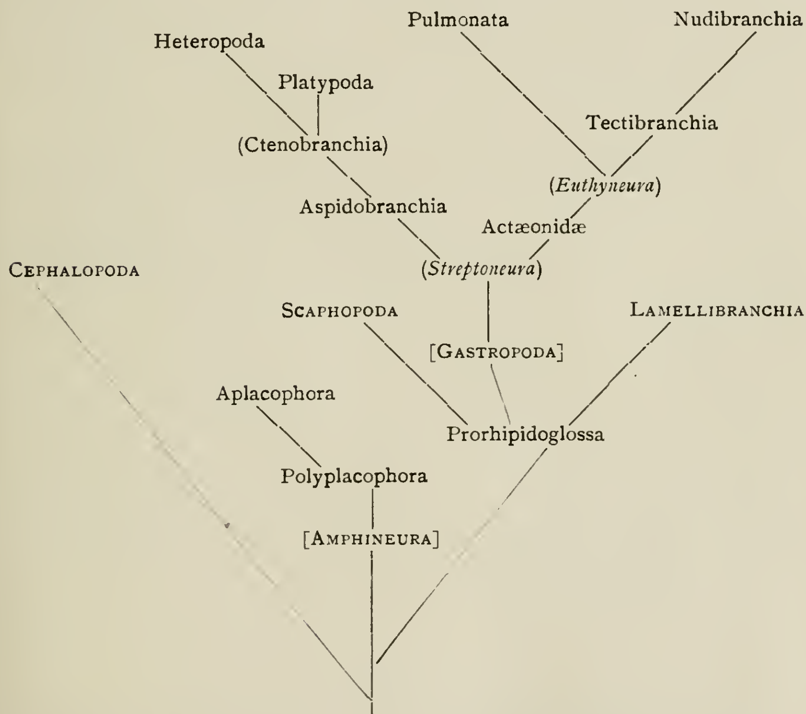
The most recent complete scheme is probably (there may be a later by the time this appears in print) that by Dr. P. Pelseneer (6), and it is a matter for regret, that while the privileged few have had author's copies for some months past, so that his table has been even incorporated in Simroth's new edition of the molluscan portion of Bronn's great work (7), the volume from which it was supposed to be taken has but just been made available for the many.

On looking at the scheme here reproduced in both its tabular and phylogenetic form (the latter modified in accordance with a later paper to be presently mentioned), certain points at once engage the attention: most of them, it is true, have been already set forth in minor communications, but the arrangement, as a whole, has not been presented before, and therefore it appears a fitting opportunity on which to call attention to them:—

Amphineura		Polyplacophora	
		Aplacophora	
Gastropoda	Streptoneura	Aspidobranchia	Rhipidoglossa
			Docoglossa
		Ctenobranchia	Platypoda
			Heteropoda
	Euthyneura	Opisthobranchia	Tectibranchia
			Nudibranchia
		Pulmonata	Stylommatophora
			Basommatophora
Scaphopoda		Solenoconcha	
Lamellibranchia		Protobranchia	
		Filibranchia	
		Pseudolamellibranchia	
		Eulamellibranchia	
		Septibranchia	
Cephalopoda		Dibranchia	
		Tetrabranchia	

In the first place, the Pteropoda as a class have disappeared, being merged with the Opisthobranchiata. They were originally classed as allies of this last-named order by De Blainville (8 and 9), who then used the term "Pteropoda" for certain of the Nucleobranchs and *Argonauta*, whose shell in those days was not thought to be the product of the animal that occupied it. This arrangement, however, was never really adopted by conchologists, and it was not until 1886 that Boas (10) pointed out, on anatomical grounds, that the Pteropoda could not be separated from the Opisthobranchiata.

As one of the results of his researches in connection with the preparation of the "Challenger" Reports on the Pteropoda (11) Pelseneer was led to acknowledge the correctness of this conclusion, and it has been subsequently accepted by Professor Lankester (12) and adopted by Lang in his Lehrbuch (13). The latter authority, also, follows Pelseneer in his classification of the Pelecypoda (14), or Lamellibranchiata, to use a later and less preferable term, to which we note with



regret that our author reverts. These last he derives, as before, with the Gastropoda from a common stock—the hypothetical Prorhipidoglossa, but adds now as another branch from the same—the Scaphopoda. In this he is in accord with Plate (15), who, however, places their branch a little more directly on the main stem, between the off-shooting of those of the Pelecypoda and *Patella*. In a subsequent note on the genus *Actæon* (16), Pelseneer, agreeing with Bouvier (17), slightly modifies his scheme, and considers that *Actæon* is not only the common stock whence are derived the Pteropoda, Tectibranchiata, and Pulmonata, but also the link between the Streptoneura (= Prosobranchiata) and the Euthyneura (= Opisthobranchiata and Pulmonata); for *Actæon* is streptoneurous, that is to say, possesses a nervous system in which the loop-like visceral commissure is crossed over in the form of a figure 8, and hence shows that the Euthyneura, in which the visceral commissure forms a simple loop, were in the first instance streptoneurous like the Prosobranchiata, and that their euthyneurity, if the word may be permitted, is the result of secondary displacement of the visceral hump from left to right. This retrograde displacement may possibly have been occasioned through the hermaphroditism of the animal, since the Streptoneura are all unisexual. Among the Streptoneura the Trochoid Rhipidoglossa are nearest to *Actæon*.

Another point of importance is the direct derivation of the Cephalopoda from the primitive stock before the other branches lead off. In thus opposing the Cephalopoda to the rest of the molluscan phylum, Pelseneer agrees, as he himself is careful to point out, with Giard, who advanced this view in 1876 (18). The grounds for this theory are to be found, not only in the anatomy, but also in the high antiquity of the class.

As to the source whence the Mollusca arose, this, Dr. Pelseneer thinks, must be sought in the Polychæta Errantia, or that group of the Worms to which, among others, the gaudy sea-mouse (*Aphrodite*) belongs; and, moreover, he is of opinion that the family Eunicidæ are, among the living representatives of that sub-order, the nearest, in the *ensemble* of their organisation, to the Mollusca.

Granting this conclusion, what then of the shell? We are tempted to speculate whether the power of specially secreting a calcareous covering may not be the revival of a dormant faculty (seeing that it is present in a collateral sub-order, the Tubicolæ, or Tube-building Worms), just as the individual offspring of higher creatures will at times present characters or display capabilities that have lain dormant in their immediate progenitors—an occurrence to which the term atavism has been applied.

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B. B. Woodward.

V.

The Museums of Public Schools.

I.—CHARTERHOUSE.

IT is an encouraging sign that the "Museum Question" has penetrated into the majority of our Public Schools, and that a desire is felt that the "old-curiosity-shop" type of museum should give place to an organised institution of real educational value. It will be granted on all sides that education is the primary object of all and particularly of School Museums.

The problem then presents itself—How ought a Public School Museum to be arranged to the best advantage of Public School teaching? To this question a cut-and-dried answer applicable to all is impossible, for, within limits, each Museum requires to be adapted to the special characters of its locality, to the class of persons for whose instruction it exists, and to the special teaching of various subjects in the school curriculum.

Probably in all cases the objects possessed by any Museum can be arranged so as to bear reference to one of the following sciences: Ethnology, Zoology, Botany, Geology, and Mineralogy. It is almost superfluous nowadays to insist that each of these subjects should be kept distinct from one another. It is best that a separate room should be devoted to each, but since this must often be impossible in schools, at any rate let the cases contain specimens illustrative of one only of these "ologies," and let the object and purpose of each case be conspicuously apparent.

In any large school there are at least two sets of individuals whose needs must be considered in the arrangement of the Museum. Firstly, the average boy who has a healthy love of Nature, but does not know or wish to know overmuch about the details of structure, principles of classification, theoretical considerations, and so on, but is content with external form and interesting anecdotes. This variety of boy is destined to become a "country gentleman," and, as such, he has a sportsman's desire to be able to identify his captures and to know something of their habits. Such persons, be they young or old, deserve all encouragement; among their number are found a few ardent collectors willing to enrich the Museum and a few—alas! a very few—careful observers keeping an amateur naturalist's diary and amassing a quantity of facts the record of which, though known for years by the student of the "literature of the subject," is of inestimable value in the training both of mind and eye. Secondly, the "scientific variety," *i.e.*, the boy who has deliberately chosen for himself a scientific profession and desires an elementary training in those subjects to which his after life is to be devoted. It is a lamentable fact that, in my experience, with one or two exceptions, the boy field-naturalist is not also the boy scientific. Granted, then, the existence of these two types, it is evident that what is necessary, helpful, and instructive to the latter, will not be palatable to the country gentleman in embryo ; technical knowledge of any subject is to him "stinks," and as such not only is abhorrent to his nostrils, but may even disgust him to so great a degree as to make him fear contamination even in the open fields ! Many a boy, aye and man too, is much interested in butterflies and moths, but has an extraordinary aversion to Lepidoptera.

It becomes, therefore, inevitable that a double arrangement of the Museum should be contrived, (1) a general natural history collection, by the aid of which boys may identify their victims ; this should be arranged in such a way as to elucidate the main structure of the various groups in the animal and vegetable kingdom, and to stimulate collectors to cease to be merely such, and advance to the condition of accurate observers of the special group which they have made their hobby. This may be done by the introduction of exhibits illustrating the development and life-history of some common species, so that anyone with but little apparatus can verify each phenomenon for himself when once pointed out.

(2) A small collection of types as fully described, labelled, and explained as space will permit. In this series there should be no hesitation as to the use of technical terms, but let each term be fully explained and its derivation given, in order that the convenience and necessity of its use may be apparent. It is advisable that this series should conform to the course of class or special teaching adopted in the school, and it is undoubtedly desirable that it should, to a certain extent, follow the arrangement of some good practical text-book on the subject, so that a boy with book in hand can work through the entire series, or each section of it, prior to attempting the same section with scalpel and forceps in the laboratories.

We have not hitherto considered the needs of the general public in School Museums, nor should they be considered apart from the school with regard to arrangement. Nevertheless, it is a wise and generous regulation to admit visitors freely, nor will the concession be without return, for by this means the Museum is advertised, and residents of all classes will readily present specimens of great local interest, which ought to find their resting-place in some public institution and not remain hidden in private halls or on cottage mantel-shelves.

To pass on to the limits which should be assigned to the collections. Undoubtedly, in the Botanical and Zoological sections, especial prominence should be given to the local flora and fauna, but it is not advisable to limit the scope quite so narrowly, for boys come from all parts of the kingdom, and it is, for the most part, at their homes and during the holidays that most of their unaided work as naturalists is carried on. During term there are so many rival attractions, and such a love of athletic distinction, that many good naturalists forsake woods and meadows for the cricket field. It is therefore better, in these subjects, to extend the collections to all British animals and plants, and *to draw the line unflinchingly at this point.* If a Museum is to educate at all, the faculty of observation claims its attention before

Fig. 1—External View of Museum of Charterhouse School, Godalming.
From a Photograph by] [*Messrs. W. & A. H. Fry, Brighton.*

all others, and if the boy is not taught how and what to observe in the small world immediately around him, the man assuredly will not observe Nature truly in the enlarged world of after life. It is a difficult and often an ungracious task to say "no" to some generous offer of exotic lepidoptera, foreign birds, &c., to say nothing of heads of big game, stuffed crocodiles, and other "white elephants," but the refusal is absolutely necessary if any reasonably representative collection is to be set forth in the limited space at the disposal of any school. Let it, however, be at once stated that there is no objection to receiving foreign specimens of species which do occur in Britain, but let their locality be clearly stated on the labels, and also the fact that they are of British occurrence. Exception may, perhaps, be made in the series of special types where British species are inconveniently small; but even so, foreign species should only be exhibited in company with their British congeners and should not entirely oust them.

With regard to the Geological and Mineralogical sections, the features of the immediate neighbourhood should be clearly displayed, but for teaching purposes it is best to follow pretty closely in arrangement some good text-book, of which a copy should be kept near the

FIG. 2.—INTERIOR VIEW OF NATURAL HISTORY MUSEUM, CHARTERHOUSE SCHOOL.

cases or in the room specially set aside for these subjects. A series of well-described models of crystals ought to be placed in a conspicuous position in the collection of minerals, with actual examples, as far as possible, accompanying each model.

In addition to the actual exhibition rooms of a Museum, well-lighted work-rooms ought to be within easy access, if possible under the same roof; one work-room, at least, should be for the use of pupils, and one should be for the private use of the Curator. The care of the Museum is most satisfactorily carried on by an official whose main work should be the arrangement, setting up, and labelling of specimens, while actual class teaching ought not to occupy any great portion of his time. Where this is impossible and one of the

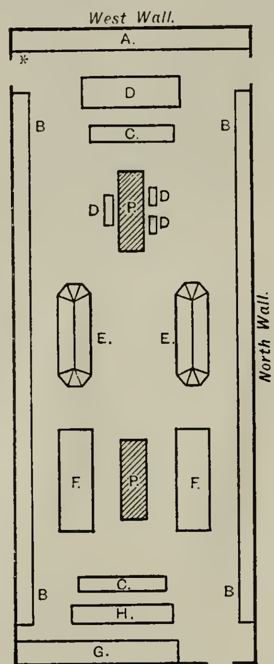

FIG. 3.—GROUND-PLAN OF NATURAL HISTORY MUSEUM, CHARTERHOUSE SCHOOL.

A.—Wall-case for Mammals.
B.B.B.B.—Side shelves for Birds.
C.C.—Central stands for ditto.
D.—Large entomological cabinet, surmounted by table case and double glass shelf.
D.D.D.—Small entomological cabinets.
E.E.—Show cases of types of Animal Kingdom.
F.F.—Table cases with Fossils.
G.—Wall case for Geological Model of Neighbourhood, etc.
H.—Botanical cabinet, surmounted by table case and double glass shelf, the two latter contain Mineralogical Collections.
P.—Stacks of hot-water pipes—tops are used for large fossils, etc.
* —point of view of photograph.

ordinary Masters of School becomes Curator, even though he be assisted by colleagues in various sections, it is most essential that there should be a thoroughly competent skilled assistant, capable of setting up specimens, making dissections, and conducting the general preparatory work. This particularly applies to the first few years after the opening of the Museum, for when once the permanent preparations are in position, the work of keeping all in good order is comparatively slight.

Turning now to the Museum at Charterhouse, the general features of the building can be seen from the accompanying figures. The whole block consists of two centrally situated halls 75 feet by 27 feet 6 inches, running parallel to each other, these are flanked on the north and south by class rooms; to the west is a low colonnade, while the east end is formed by a large lecture hall capable of seating some 250 persons. The two central halls form the Museum proper; they are lighted by windows placed continuously on each side, immediately beneath the roof, and also by a pair of large windows in their western walls.

The south hall is devoted to Antiquities, Ethnology, Art, etc. In it are also contained many Carthusian relics, together with weapons of the chase, utensils, and so on of local interest, which furnish valuable mementoes of the simple country life of Surrey, a mode of life which is all too rapidly vanishing before the advance of railways and speculative builders. The two side walls are occupied chiefly by collections of flint weapons and of ancient pottery. The cases in which the latter is arranged are backed with continuous mirrors, so as to enable both sides of the specimens to be seen without opening the case fronts. Standing in the body of the hall are show-cases illustrating the various processes by which engravings, etchings, etc., are produced, or containing specimens obtained from the Swiss Lake Dwellings; others exhibit collections of ancient coins, Greek sculpture, and so on. In short, this portion of the Museum appeals to the anthropologist and antiquarian rather than to the student of Natural History in its ordinary acceptation.

To the north of this hall is the Museum of Natural History. The main portion of this is by force of circumstances devoted to birds. The accompanying ground plan will show at a glance the general arrangement. The whole length of the west wall is occupied by a large case with four glass doors which is to be devoted to mammals. Down both the north and south walls run two shelves at the height of about 3 ft. 6 ins. and 6 ft. 6 ins., on which are arranged the cases of birds. On the front of the lower shelf is a continuous glass-topped tray in which are placed the eggs, and where possible the nest of the species standing behind and above on the lower and upper shelf respectively. This collection of birds is of great local value; the major part of it was brought together during a period of many years by the late Mr. W. Stafford, of Godalming, by whom most of the specimens were both shot and set up. On the death of the collector a considerable sum of money was subscribed from various sources in order to prevent the collection from leaving the locality in which it had been made or being dispersed, and the whole collection, thus purchased, was handed over to the Charterhouse Museum. In addition to this, a notebook exists giving many details of the capture, locality, etc., of the specimens, and notices of other authenticated occurrences in the neighbourhood. It has thus been possible to affix to many of the cases the exact date and spot whence obtained. The wall space is insufficient to accommodate the whole collection, and accordingly the geese and ducks are placed on brass-supported frames, running transversely to the length of the hall, near the west and east ends. The collection is strictly limited to British species according to the "Ibis" list, which has also been followed in the classification.

A few hybrids and professed "foreigners" have been removed from the collection and placed together in a separate part of the hall; eventually it is to be hoped they will be banished. The arrangement of the birds was necessarily the first thing to be attended to when the Museum was being equipped last year, thus many other sections of this half of the Museum are in a backward condition. Next to the birds, the insects, and especially Lepidoptera, attract most attention. Here the arrangements are good; there is a large entomological cabinet in which are being arranged all the British Macro-lepidoptera. It is intended that the full life-history of every species shall be shown with larvæ on artificial food-plants, pupæ both in and out of cocoon where such exists, and imagines in resting attitudes, as well as the conventional "setting." This, of course, will be a work of years; but it has begun and is progressing. Above the cabinet is a table-case, in which will be placed diagrams and described specimens to show the chief anatomical features of the various orders of insects; and above this again double glass shelves for miscellaneous objects of interest connected with the habits and economy of insects. In addition, there has been presented a fine collection of Lepidoptera (imagines only), which is kept apart. Hymenoptera have claimed a few votaries, but other orders of insects fail to attract at present. There is accommodation for a collection of Mollusca and material, chiefly foreign, where much weeding will be necessary.

Two show-cases standing in the room are devoted to special types, one including Protozoa to Mollusca, the other Chordata. These are intended for specialists in Biology, and will ultimately be arranged so as to follow, with a few additions of extinct forms, the book of Practical Zoology in use in the school. There are two table cases of fossils arranged stratigraphically. A case of Minerals arranged after an elementary text-book stands near the east end of the room, and beneath this case a collection of dried plants mounted on cards and placed on sliding trays. Portfolios of dried plants, comprising collections made in the neighbourhood of Godalming, complete the Botanical section at present. At the east end of the hall is a large wall case, destined to contain a Geological model of the neighbourhood and diagrams to illustrate sections. The fossils characteristic of the strata, and, as far as possible, the peculiarities of fauna and flora, will be pointed out. Beneath the shelves on which the birds are placed is a vacant space which eventually can be occupied by low cabinets, storecases for duplicates and specimens, for class work.

The care of the Museum is entrusted to two of the Masters of the school, who are unfortunately not assisted by a regularly trained skilled assistant. It may seem ungracious to criticise what is in many respects excellent, but a few possible improvements may be mentioned for the warning and benefit of others who have not yet "builded new barns" for Museum purposes. (1) There are no work-rooms in connection with the Museum, nor any rooms situated conveniently near in which the work of preparation can be carried on. The inconvenience of this want to the Curators will be only too obvious to all who have ever had charge of any portion of a Museum. Moreover, the absence of a work-room for students greatly limits the usefulness of the collections. (2) The position of the windows, though giving a good light, does not give so good a light as continuous skylights would have afforded. (3) The supports of the roof descending on to the main walls of halls curtails the available wall-space to a great extent. In the present instance, had the roof been supported in some other way, a rail-gallery would have been possible round both halls, which would have doubled the wall-space. Under the present circumstances, nearly half the wall-space is too high for *instructive* exhibition purposes, though it can be made serviceable for the reception of various gifts which have no proper place in a School Museum, and can therefore without loss be "skied" into positions sufficiently remote as not to disturb the visible arrangements, and sufficiently prominent to gratify the well-meaning donors.

OSWALD H. LATTER.

VI.

The Surface of the Moon.[1]

IN the January number of NATURAL SCIENCE, attention was directed to two short articles on the subject of lunar volcanoes and lunar glaciation. In the *Bulletin of the Philosophical Society of Washington* (vol. xii., April, 1893, pp. 241–292) will be found a paper on "The Moon's Face, a Study of the Origin of its Features," being the retiring address of the President, Professor G. K. Gilbert. This is an admirable summary of the history of the study of the moon's surface, while, at the same time, it abounds with suggestive and thoughtful ideas. Professor Gilbert commences by remarking that the topography of the moon is perhaps better mapped than that of North America, and mentions the various features which have been observed and named. The "craters" are described, and figures are given, comparisons being drawn between the "wreath" of a lunar "crater" and the landslip terraces of the margin of a basaltic plateau. After this descriptive matter, the various "theories" are taken into consideration. Regarding the "volcanic theory," Professor Gilbert compares unfavourably the relative abundance of the craters on the moon with any equal area on the surface of the earth, those on the earth being as one-tenth to those on the moon. The same disparity exists with respect to size, the ten largest terrestrial craters recorded having a mean diameter of eleven miles. These are, Lake Bourbon (Luzon), 16 by 14 miles; Asosan, 15 miles; Kamschatka (Scrope, 2nd ed., p. 457), 15 miles; Mauritius (Darwin), 15 by 11 miles; Lake Bolerna (Italy), 11 by 9 miles; Lake Maninju (Sumatra), 15 by 7 miles; Pepandayan, 15 by 6 miles; Teneriffe, 7 by 8–10 miles; Deception Island, 8 by 7 miles; Monte Cavo, 7 miles; Mount Marindin (Mindanao), 9 miles; Mount Astria (Iceland) 10 miles. The mean diameter for the ten largest lunar craters visible to us is 275 miles. These are, Apennines, Serenitatis, Crisium, Humorum, Humboldtianum, Bailly, Iridium, Clavius, Otto Struve, and Grimaldi. The actual largest terrestrial crater has a diameter of about fifteen miles, the largest lunar crater, that whose rim is partially preserved in the Carpathian-Apennine-Caucasus chain, has a

[1] Précis of Professor G. K. Gilbert's address to the Philosophical Society of Washington, December 10, 1892. By C. Davies Sherborn, F.G.S.

diameter of 800 miles. There is no important discrepancy in the vertical dimensions. Lunar craters of the first rank range from 8,000 to 15,000 feet in depth ; terrestrial craters, probably from 2,000 to 4,000 feet. The contrasts as to form are of greater importance, and are all fully set forth in the paper. Professor Gilbert concludes by observing that "the volcanic theory, as a whole, is therefore rejected, but a limited use may be found for the maar phase of volcanic action [no eruption of lava, but merely an explosion of steam] in case no other theory proves broad enough for all the phenomena."

Passing next to the tidal theory, Professor Gilbert puts aside the views of Rozet, Hooke, Bergeson, and Humphreys, and considers those of Faye, Ebert, and Hannay. These postulate a time when the moon was liquid, with the exception of a thin crust. The moon then rotated more rapidly than now, and great tides, excited by the earth's attraction, racked and cracked its crust, and here and there squeezed out a portion of the liquid nucleus, which flowed back again when the tidal wave had passed ; but congelation caught the flood at its edges, so as to mark its limit by a solid ridge. By each successive tide the operation was repeated, with the result that the wall was given a circular form, and was gradually built up. The process was finally closed by the congelation of lava in the orifice, and while congelation was in progress the last feeble eruption sometimes produced a central hill. Professor Gilbert asks, in criticising this theory, for an explanation of the multitude of small craters overlying the larger, and why, if the crust were divided by fissures, would not the tensile strains wrought by the crest of the tidal wave cause the fissures to gape, instead of forcing out the liquid through apertures here and there ? Or, if there were no fissures, would not the strains suffice to produce them ? He further points out that there are numerous craters, of small or medium size, occupying slopes of the greater crater rims, and the initiation of these by tidal process seems impossible. Whatever lava escaped from an orifice on a slope would, he remarks, flow down the slope, instead of being drawn back.

The views of Ericsson and Peal, that the site of each crater was once occupied by a pool of water which, by heat from below, was vapourised, converted into snow, and from which was eventually accumulated an annular ridge, he criticises as follows:—If the rim were built up by the quiet fall of an infinitude of ice particles or snow flakes, its configuration should be smooth and regular instead of exhibiting the rugosity actually observed. The postulated heat of the central area might render the inner slope steep, and even produce the inner cliff and terraces, but the theory affords no explanation of the wreath nor of the central hill; it fails also to account for the small craters formed on the rims and slopes of the larger, for the bottoms of these are far above the assumed rock plain of the moon, through which the theory supposes the internal heat to have been communicated. Professor Gilbert admits that he is unable fully to judge Peal's views, which have been circulated in a pamphlet, of which 100 copies were privately printed, and therefore practically unpublished and inaccessible.

All other theories which have been put forward appeal in some way or other to the collision of other bodies with the moon's surface, and Professor Gilbert groups them together under the heading "Meteoric." Proctor apparently was the first to definitely suggest a "meteoric" theory in 1873 and 1878. "Asterios" and Meydenbauer have also advanced similar views. The "meteoric" theory is thus defined in Gilbert's paper:—If a pebble be dropped into a pool of pasty mud, if a rain-drop falls upon the slimy surface of a sea marsh when the tide is low, or if any projectile be made to strike any plastic body with suitable velocity, the scar produced by the impact has the form of a crater. This crater has a raised rim, suggestive of the wreath of the lunar craters. With proper adjustment of material, size of projectile, and velocity of impact, such a crater scar may be made to have a central hill, and it is Professor Gilbert's belief that all features of the typical lunar crater and of its varieties may be explained as the result of impact. His arguments may be summed up in the following sketch. The shooting star records by its brief coruscation the collision with our atmosphere of a particle of star dust; and although they are generally very minute in size, a few survive after passing through an atmosphere and reach the earth as aerolites weighing ounces, pounds, or occasionally tons. It is also an ascertained fact that these bodies are spreading in countless myriads through space in all directions. As the moon is either without atmosphere, or has one of extreme tenuity, the impact of one of these bodies may have an important effect on the surface. As it is incredible that even the largest meteors of which we have direct knowledge should produce scars comparable in magnitude with even the smallest of the visible lunar craters, advocates of meteoric theories have assumed that at some earlier period the meteors encountered by our solar system were of greater size than now, and as no evidence has been found that the earth was subjected to a similar attack, there is assigned to the lunar bombardment an epoch more remote than are the periods of geological history, any similar scars produced on the earth having been obliterated.

Professor Gilbert does not assume that the surface of the moon was necessarily soft. Rigidity and plasticity are not absolute terms, but relative. But he suggests that the heat developed by the sudden arrest of a fragment of matter travelling at the rate of 45 miles per second might serve not only to melt the fragment itself, but also to liquefy a considerable tract of the rock mass by which its motion was arrested. The difficulty of the relation of the volume of the rim to the capacity of the hole is carefully considered, and experiment proved that when target and projectile were of uniform consistency throughout, there was no defect of rim; but when the general mass of the target was softer than the portion at the surface, the uplift consequent on the production of the hollow was only partially localised about its periphery, the remaining part being widely distributed through the floor of the softer material below. The last difficulty is connected with the circular contours of the craters. The predominant direction of the swiftly moving meteoric bodies approaching from all directions would be about 45°, and the scars produced by collision would be predominantly oval instead of predominantly circular. Proctor suggested that immediately after the shock of the collision there might be an elastic return to a circular form ("The Moon," London, 1873, p. 346); but to account for this peculiarity of form of the lunar craters, Gilbert advances a "Moonlet Theory," assuming that before our moon came into existence the earth was surrounded by a ring of meteors similar to that possessed by Saturn, and that the small bodies constituting this ring afterward gradually coalesced, gathering first around a large number of nuclei, and finally all uniting in a single sphere, the moon. Under this hypothesis the lunar craters are the scars produced by the collision of these minor aggregations or moonlets, which last surrendered their individuality. This theory is fully explained and worked out in the address, to which the reader must be referred for details.

Professor Gilbert then discusses the "arched floors" of some of the larger craters, and the distribution and overlap and interference of the craters themselves; and then turns to the question of lunar sculpture. The sculpturing of the Mare Imbrium has particularly engaged his attention, and he points out that the trend of the sculpture lines and a peculiar softening of the minute surface configuration, as though a layer of semi-liquid matter had been overspread (which he believes to be the fact, the deposit having obliterated the smaller craters and partially filled some of the larger), indicate that a collision of exceptional importance occurred in the area of the Mare Imbrium, and that one of the results was the violent dispersion in all directions of a deluge of material—solid, pasty, and liquid. The effect of this "splash" has been traced by Gilbert 900–1,000 miles in one direction, and he estimates the volume of the projectile to have equalled a sphere 80–100 miles in diameter. A sketch-map is given, showing the trend of the lines of sculpture, and the broken and notched rim of the Mare Imbrium.

Referring to the "furrows" seen on the moon's surface, so remarkably straight in general, one of which is calculated to be 187 miles long, 10–25 miles wide, and 11,000 feet deep, it is suggested that these are the tracks left by solid moonlets, whose orbits at the instant of collision were nearly tangent to the surface of the moon. It is curious that when plotted on the chart of the moon's face, Professor Gilbert found that more than half of them accorded in direction with the trend lines of the Imbrium outrush, and he is led to suggest a connection between them and the scattered fragments dispersed by the collision in that district. The anomalous plateau of Wargentin is considered, but no definite suggestion is made, although clues are not wanting, for the whole neighbourhood abounds in evidence of flooding.

With regard to the "white streaks," which are quite independent of any surface configuration, and which usually radiate from some crater itself lined with a similar white matter, Professor Gilbert quotes an unpublished suggestion of William Würdemann of Washington, which is as ingenious as it is simple. Würdemann writes to Dr. B. A. Gould as follows:—"The most remarkable appearance on the moon, for which nothing on earth furnishes an example, is presented by those immense radiations from a few of the larger craters—perfectly straight lines, as though marked with chalk along a ruler—starting from the centre of the crater and extending to great distances over every obstruction. My explanation is that a meteorite, striking the moon with great force, spattered some whitish matter in various directions. Since gravitation is much feebler on the moon than with us, and atmospheric obstruction of consequence does not exist, the great distance to which the matter flew is easily accounted for." Gilbert remarks that this theory accounts for the straightness of the rays, for their vanishing edges and ends, for their independence of topography, for their relation to craters, for the whiteness of the associated craters, and for the nimbus in which the rays sometimes unite close to the crater. It further explains the white crests of many grey craters, for peaks would intercept more than their share of the horizontal shower. The difficulties and suggestions of this theory are fully dealt with by Professor Gilbert, and he is inclined to believe the white matter to be a readily fusible solid.

Summing up the various theories in his retrospect, Professor Gilbert says:—the impact theory applies a single process to the formation of craters (excepting only the rill pits), correlating size variation with form variation in a rational way. Specialised by the assumption of an antecedent ring of moonlets, it accounts also for the great size of many craters. It brings to light the history of a great cataclysm, whose results include the remodelling of vast areas, the flooding of crater cups, the formation of irregular "maria," and the conversion of mere cracks to rills with flat bottoms. It explains the straight valleys and the white streaks. In fine, it unites and organises as a rational and coherent whole the varied and strange appearances whose assemblage on our neighbour's face cannot have been fortuitous.

The address concludes with a brief discussion of the growth and age of the moon. Professor Gilbert believes that during the whole period of growth the surface lost heat by radiation, but the process of growth cannot have been slow enough to permit the concurrent dissipation of all the impact heat. Liquefaction was a local and temporary surface phenomenon, and there is little evidence of the wrinkling, which, theoretically, should result from the adjustment of a cold crust to a cooling nucleus. The topography is due to sculpture, not to wrinkling; the smooth floors of the maria have anticlinal and monoclinal forms, but of so gentle a slope that they are seen very near the terminator, and can represent but a minute amount of arc shortening. It is therefore probable that the final shrinkage of nucleus was small, and the antecedent storage of heat correspondingly small. During the whole period of growth, the body of the moon was cold. With regard to the age of the moon, Professor Gilbert asks two questions. (1.) Does the earth exhibit impact craters? If not, then erosion and sedimentation have destroyed them, and the Cenozoic era did not witness the building of the moon. (2.) Is any horizon of stratified rocks generally or widely characterised by molten disjecta? If not, then the moon was already a finished planet in Palæozoic times.

VII.

Antarctica: a Supposed Former Southern Continent.

IN the current number of the *Geographical Journal* there appears a paper by Mr. H. O. Forbes, on "The Chatham Islands: their relation to a former Southern Continent," read before the Royal Geographical Society on the 13th March last. He describes his discovery there in the previous year of remains of a large extinct ocydromine Rail, which he has identified with *Aphanapteryx*, a genus till then known only from Mauritius, and of a large *Fulica* closely related to *F. newtoni* of the same region. Taking these fresh facts into consideration, he marshals all the data which he considers prove a strong case for the probability of the existence of a former southern continent, and he sketches on a map of the Southern Hemisphere what he believes was the configuration of *Antarctica*, as he has named that vanished continent. He believes that it followed nearly what is the 2,000-fathom line, and extended northward from a circumpolar area, by broad extensions, one to join an old New Zealand continental island (including the Antipodes, the Maquarries, New Zealand, the Chatham, Lord Howe, Norfolk, the Kermadec, and the Fiji Islands); another to East Australia with Tasmania; another to the Mascarene and surrounding islands (the *Lemuria* of Sclater); *perhaps* one to South Africa, and, lastly, one to South America. The form of this continent would not interfere with the opinions expressed by many authorities in the permanence of the great ocean basins.

The chief evidence adduced by Mr. Forbes in support of the former existence of this continent, shortly stated, rests on the distribution of forms confined to or extending but little beyond the Southern Hemisphere.

I.—Among Aves.

1. Of the Struthious birds:—*Dinornis* and *Apteryx*, in New Zealand; *Dromornis* and *Dromæus* in Australia; *Casuarius* in the Papuan sub-region; *Æpyornis* in Madagascar; *Brontornis* in Patagonia.
2. Of the Trogonidæ in South America, Tropical and South Africa, and in the Indian region.

3. Of the Spheniscidæ, which are confined to the Southern Hemisphere, and appear fossil in New Zealand and in Patagonia.
4. Of the Chionidæ, a family of the Charadriidæ, confined to the Antarctic Islands.
5. Of the Psittacomorphæ, confined chiefly to the Southern Hemisphere, with a few stragglers in North America and a few groups in India.
6. Of the *Peristeropodes* (of the Gallinaceæ) in Australia and in South America.
7. Of the Ocydromine Rallidæ (*Aphanapteryx*), of *Fulica* and *Notornis*, of *Frigelupus* and *Heterolocha*, in the New Zealand and the Mascarene regions.
8. Of Solitaire, Dodo, and Didunculus, in the Mascarene Islands and Samoa.
9. Of the Avocets and the Stilts, in New Zealand, Australia, Africa, and South America.
10. Of the embryological affinities of the Dendrocolaptine birds of the Neotropical region with the Gymnorhine crows of Australia; of *Artamus*, with *Pitta* and *Grallaria*; of *Petræca*, with the Mniotiltidæ; of the New Zealand Xenicidæ, with the Australian and Oriental Pittidæ, the Mascarene Philepittidæ, and the Neotropical Pipridæ, Cotingidæ, Tyrannidæ—evidence of great weight.

II.—Among other Vertebrata.

1. Of Marsupialia—*Nototherium*, *Diprotodon*, *Thylacoleo*, *Thylacinus* in Australia; *Prothylacinus*, *Amphiproviverra* in Patagonia.
2. Of Edentata—In South America, South Africa (Orycteropidæ) and in India and the Malay Archipelago (Manidæ).

   This distribution of the *Edentata* may, however, indicate a connection of "Antarctica" with Africa and South America when it was not in connection with Australasia, but when there was a bridge between East Africa and the Oriental Region.
3. Of Amphibia—Cystognathidæ in Australia, Tasmania, and South America; and giant tortoises in Madagascar and the Galapagos Islands.
4. Of Fresh-water Fishes—the great alliance between those of Australia, New Zealand, Chili, Patagonia, and the Falkland Islands; Haplochitonidæ and Galaxiidæ and the Dipnoan and Osteoglossid types in South Africa and in South America.

III.—Among Invertebrata.

1. Of *Peripatus* in South Africa, Australia, South America, with the West Indies.

2. Of *Cercophonius* (a genus of Scorpions) in South East Australia and South America.
3. Of Buprestidæ and *Longicornia* in South America, Australia, and New Zealand.

IV.—Among Plants.

1. Saxafrageæ—Escalonieæ (17 genera) and Cunonieæ (18 genera) in New Caledonia, Australia, Tasmania, New Zealand, Mascarene Islands, South Africa, South America. Only two of these 35 genera cross the Equator.
2. Proteaceæ (49 genera, 950 species)
3. Monimiaceæ (22 genera and 250 species)

(2 and 3:) in South America, South Africa, Madagascar, Tasmania, New Zealand, New Caledonia.

4. Of the Cupressinæ (Coniferæ), *Callitris* in Africa, Madagascar, Australia; *Fitzroya* in Chili and Tasmania.
5. Of the Verbenaceæ, *Petræa* typically South American, in Java and Timor, and *Petræovitex* nearly related to *Petræa* in Buru and Amboina.
6. Of *Senecio appendiculatus*; *Fernelia buxifolia*; *Chasalia capitata*; *Pouzolzia lævigata*, *Cyperus scoparius* in Timor and in the Mascarene Islands.
7. Of *Ranunculus trullifolius*, *R. Mosleyi* (allied), *Lyallia*, in Kerguelen and in Fuegia.
8. Of *Polypodium vulgare* in Kerguelen, South Africa, South America, and Marion Islands. *Cotula plumosa*, *Uncinia compacta* in Kerguelen and Australasian region. *Pringlea* in Kerguelen and Marion Islands.
9. From the remains of buried forests, and the occurrence of lignite beds, and of enormous deposits of Peat in the Chatham Islands, in Kerguelen and in other Antarctic Islands, indicating a former forest-clad region.

The development of the Southern fauna and flora occurred, Mr. Forbes believes, during the Ice Age of the Northern Hemisphere when "Antarctica" rejoiced in a semi-tropical climate. Their dispersion occurred during the slow oncome of the partial glaciation of the Southern Hemisphere, which exists now.

The occurrence of many southern forms of life in the Miocene of France and other parts of Europe is explained, according to the author, by their having been driven north during a cold, or perhaps glacial, period in the Southern Hemisphere in that age, of which there are evidences in various parts of South Africa, Australia, and South America, corresponding to the warm epoch in the Miocene age in northern latitudes.

Mr. Forbes has independently arrived at views very similar to those expressed by Professor L. Rütimeyer in 1867 in his work, "Ueber die Herkunft unserer Thierwelt: eine zoogeographische Skizze"; and several geologists, notably Dr. W. T. Blanford, have discussed some of the more important problems involved. We have, however, compiled a concise synopsis of Mr. Forbes' latest arguments in favour of the theory of "Antarctica" (based upon the known distribution of certain animals and plants), because the sum-total of the evidence is now so much greater than it has been heretofore; and such marked progress has been made in investigating the geology of the Southern Hemisphere during recent years, that it seems profitable to re-open the question from the geologist's point of view once more. Zoologists and botanists will welcome the expression of the latest views of those who are acquainted with the physical considerations bearing upon the subject.

VIII.

On the Work of Glaciers.[1]

AN invitation from the Editor of NATURAL SCIENCE to write a short critical review of Professor Bonney's article on Glaciers in the current number of the *Geographical Journal*, is as refreshing as a whiff of cool, pure air from the "Gletcherwelt" in these sultry June days. To be brief in the present instance is not difficult, since I have nothing of importance to add to what I have already put into print, while I have nothing, on the other hand, to unsay. One has not time in these days to go on repeating, *ad nauseam*, the same arguments. All that my friend Dr. Blanford has urged now has been met and answered, as I venture to think, by anticipation in my papers, and in the criticisms which I have published of the writings of others during the last ten years, that is to say, since my papers (1) "On the Mechanics of Glaciers," (2) "On the Origin of Valley Lakes," were published side by side in the February number of the *Quarterly Journal of the Geological Society* in 1883. On the other hand, after reading the article of Professor Bonney's, which I have been asked to notice, I can only find one criticism to offer, and that is that he does not make enough of the physical impossibility of the work of excavation being done by ice, or of local earth movements, which must occur as details of the "warping" of the crust, in districts such as must be subjected to considerable squeezing between two great stable masses of the Archæan crust, as these undergo circumferential approximation towards one another by secular contraction. All that has been written in more recent years by Professor Spencer (and others) on the negative results observed by him under some of the great Norwegian glaciers, and the positive results obtained by himself and his colleagues as to the recent changes of level in the region of the great lakes of North America, co-operating with the damming-up of old Eocene lines of drainage by glacial detritus during the Quaternary period, goes to confirm the suggestion (*inter alia*) that I threw out ten years

[1] "Do Glaciers Excavate?" By Professor T. G. Bonney, D.Sc., F.R.S. With observations by Dr. W. T. Blanford, Mr. Douglas Freshfield, Sir Henry H. Howorth, and Mr. W. M. Conway. *Geographical Journal*, vol. i., pp. 481–504 (June, 1893).

ago as to the origination of the basin of the Lake of Geneva by the pinching up of the valley of the Rhone, as an incident in the comparatively recent movements that have given to the Alps and the Jura their present positions.

There are some pertinent remarks to the same effect in Heim's well-known "Mechanismus der Gebirgsbildung"; and that writer once for all disposes of the case of the Vierwaldstättersee (Lucerne Lake) by the results obtained by a series of soundings carried out by himself and one of the engineers of the Federal Government; since these show conclusively that the bed of the lake is *an alluvial plain* of an ancient valley, which has been converted into the present gem of central Alpine lakes by a partial closure of the valley at the outflow of the Limmat. The deep Achensee, again, is completely explained by the facts which I gave ten years ago, as a case of a fault-originated gorge, the drainage having been reversed as the great gorge of Jenbach was dammed by the moraine stuff from a side-valley at Maurach.[2] The Lake of Constance, again, is now well known to be a hollow formed by the damming-up of an ancient valley by glacial detritus; the present "Falls of the Rhine" at Schaffhausen having originated from a similar set of surface-changes to those which have produced the Falls of Niagara, by diversion of drainage due to glacial obstruction of the ancient valleys. Instance after instance of those quoted by Ramsay and his school (even Ramsay's pet child, the Lake of Llanberis) is being disposed of, as larger views of physiography are made to bear upon the study of lakes.

To go in detail into Dr. Blanford's criticisms would be but to attempt to combat a *scientific belief*, which has been, to my mind, rendered untenable by counter-arguments, based on known physical and mechanical laws and observed facts. Until "geologists" will take the trouble to go into the physical laboratory and master those laws, and then show that my experimental work of ten years ago in the Wellington College Laboratory upon the properties of ice, and the conclusions to which it leads, as put forth in my paper on the "Mechanics of Glaciers" (*supra cit.*); in the paper supplementary thereto on "Solar Radiation and Glacier Motion";[3] in some criticisms of a paper by another author which I published under the title of the "Motion of Land Ice"[4]; and in the remarks which I made two years ago under the head of "Dr. Nansen on Glaciation,"[5] I feel that it is rather a "dealing of blows into the air" to enter into controversy on these points.

The phenomena of the recession of glaciers within the last century or two, leaving a plain strewn with rolled detritus, and the draining

[2] See my paper on the "Triassic Deposits of the Alps," *Geol. Mag.*, 1882, p. 504.

[3] *Nature*, vol. xxvii., 1883.

[4] *Geol. Mag.* for 1891, pp. 141–142.

[5] *Nature*, vol. xliii., pp. 541–542.

of a still more ancient lake by the sawing through of a moraine-dam by the valley-stream, can scarcely be better illustrated than in the case of the Rosegg-glacier. There, as in many another Alpine example, the recession of the glacier leaves no trace of excavation of the valley floor, but the contrary. The Muir Glacier of Alaska is also a very strong case. I gratefully avail myself of the paper on this which Professor Cushing, of Cleveland, Ohio, was good enough to send me.[6] His concluding remarks are—"Those who hold the power of glaciers to vigorously erode hard rocks under most circumstances, take (it seems to me) an indefensible position. At the Muir Glacier, in just the position where the greatest erosion would naturally be expected, soft gravels have been undisturbed by the ice. The key setting forth when glaciers will erode and when not, is certainly lacking at present. It is very desirable that a prolonged and detailed study of the Muir Glacier should be undertaken. It is a comparatively large glacier, rapidly dying out, and presents an admirable opportunity for studying the behaviour of a large glacier under such circumstances. Such work could not fail to be of great value."

On one minor point I cannot follow Professor Bonney. When referring to Ramsay's classical paper, he says (p. 482): "It was proved that the rock-basins of the Alpine lakes could not have been produced by any local subsidence, or by fissures in the Earth's Crust." In reasons given in my paper (*supra cit.*) on the "Origin of Valley Lakes," I have given the grounds of this dissent. I believe that, in certain instances, these have severally operated as *important factors*, though not as sole cause, and my belief of ten years ago has been confirmed by the observations I made in 1883, through the courtesy of F. Ritter von Hauer, of models of saliferous and gypsiferous strata, beneath upland valleys in the Alpine Trias, which are to be seen in the Geologische Reichsanstalt of Vienna. Mr. Jukes Browne has referred to these in his "Handbook of Physical Geology" (2nd edition, p. 627). Let underground erosion by the solution and removal of such beds or of calcareous strata proceed through a long period of time, and another great glacier filling such a valley would make, by its dead weight, short work of the hollow floor. Such a crushing-in of the pie-crust is, however, a very different thing from what Ruskin has graphically described as a "custard scooping out its own dish." I suggested this ten years ago: its *rationale* is self-evident, but it has I suppose been ignored by subsequent writers because it was not "orthodox," though it commended itself to the late Professor John Morris, when I suggested it to him. But subsidences can and do occur without the crushing weight of glaciers.

The old and trite argument based on the association of lakes with glaciated regions may be put another way. It may, I think, be fairly asked, whether it is not the association of lake-basins in great numbers

[6] *American Geologist*, October, 1891.

(as in Scandinavia, Finland, and North America) with great districts, in which the terrain is for the most part made up of Archæan crystalline rocks, in which therefore the traces of glaciation in high latitudes are better preserved, and surface changes (including differential movements) have been longest in progress, that has led to some confusion of thought on this matter.

A. Irving.

IX.

Professor Huxley on Evolution and Ethics.[1]

PROFESSOR HUXLEY'S zoological work is great, and it has left an enduring mark on his science. He has lived in times when a new idea has been a solvent corroding the incrustations of tradition and habit, and those who knew the jewel of truth only by its setting and tarnished ornament, hated and feared him. Acute minds, orthodox or unorthodox, respected and admired him as a master in the craft of controversy, honest and resolute, apt at keen lunge and nimble recovery. But the great crowd who make reputations that iconoclastic historians of the future may have material, see in him a constructive thinker—a prophet of the new era. And so this delivery on "Evolution and Ethics" will be taken as coming "with emphatic warrant," and must be subjected to a more critical appreciation than the author himself may think fitting.

Let me state the Professor's argument as clearly as I can. Among the observed phenomena of the cosmos the most typical is cyclical change. Thus, from the inert bean there grows up the bean-plant, which, in its turn, produces other beans, and the wonder of leaves and stem fades away. Such recurring cycles are the salient feature in the shifting impermanence of the cosmos. In man, to whom the stream of life has apparently led, we find that pain, present everywhere, reaches its acutest point, and we find that his progress has been such that the old ape and tiger habits of ruthless and cruel aggression are only virtues belated in time, and become sins by the accident of advance. Thus consideration of the cosmic process of evolution brings man up against the mystery of evil.

With the philosophers of Hindustan and Ionia, centuries before our era, the changefulness of the phenomenal world, as well as the suffering in it, became apparent. When culture of the intellect and of the pleasures followed material prosperity, there came exhaustion and ennui and that "beneficent demon, doubt, whose name is legion and who dwells amongst the tombs of old faiths." Originally the conception Justice arose from some beginning like the compact of a wolf-pack, the understanding not to attack one another while on the chase. From this

[1] Evolution and Ethics. By Thomas H. Huxley, F.R.S. The Romanes Lecture for 1893. London: Macmillan & Co., 1893. 57 pp. Price 2s. net.

came the conception of justice-in-itself, of merit as divorced from the effect of action on others, the abstract idea of goodness.

The old world philosophers, turning from this new conception to the cosmos, found the cosmos incompatible with goodness. Suffering and sorrow, sunshine and rain, were distributed independently of merit. With Greek and Semite and Indian the conscience of man revolted against the moral indifference of nature. Instead of bringing in a verdict of guilty, they attempted reconciliation in various ways. Indian speculation invented or elaborated the theory of transmigration, in which the Karma or soul-character passed from individual to individual, and in the whole chain the algebraic sums of happiness and sorrow were proportional to desert. Attempts at metaphysic were more potent in shaping ethics. Then, as now, a permanent "substance" was supposed to underlie the shifting series of phenomena of mind and matter. The Indians called the "substance" of the cosmos "Brahma," of the individual "Atman." The "Atman" had its individual existence only through its casing of phenomenal desires and passions, and the aim of wise living was not merely to destroy the body, but, by a rigorous and disciplined asceticism, to suppress all individual desires, until "Atman" in negation of self became merged in "Brahma."

Gantama accepted most of these premises, but going beyond them, he disbelieved in the substance of mind as of matter, and reduced all to a shifting series of phenomena.

Karma was handed on by a process of induction from phenomenon to phenomenon; transmigration was abolished and Nirvana, the cessation of all became the ultimate good. But the means for this was not asceticism, but culture of moral qualities, and a direct attack on the baser passions as desires.

The Stoics also were metaphysicians, and imagined an immanent, omnipotent, and infinitely beneficent cause. Evil was incompatible with this, and so they held against experience that either it did not exist, or it was inflicted for our benefit or due to our fault. These theories, although they remain on evidence, do not explain "why the immense multitude of irresponsible sentient beings, which cannot profit by such discipline, should suffer." The practical conclusion of the Stoics was to live according to nature; not nature in the sense of the crude individualism of later days, but the higher nature of moral man as opposed to the lower nature of the world in general. The ethical system of the Stoics was divorced from this theory of the cosmos; it was purely intuitional, and they came to see that the ideal wise man was incompatible with the nature of things; that the cosmos works through the lower nature of man not for righteousness, but against it. So their "perfect way" came to be "apatheia," in which desire must do nothing but execute the judgment of pure reason.

Thus Professor Huxley sees the same course in the evolution of ethics among Indians and Greeks; and the end, when the "malady of thought" brings the conception of good against the evil of the cosmic process, is Nirvana or Apatheia—salvation sought in absolute renunciation.

In the modern world, the scientific optimism of a few years ago is falling before a frank pessimism. Cosmic evolution is accountable for both good and evil, but knowledge of it provides no better reason than earlier speculation for choosing the good. The survival of the fittest does not mean the survival of the best, but of the best adapted to the conditions. The cosmic process is not only non-moral, but immoral; goodness does not lead to success in it, and laws and moral precepts are directed to the curbing of it. Still, Professor Huxley sees a way out of absolute pessimism. Society remains subject to the cosmic process, but the less as civilisation advances and ethical man can combat it. The history of civilisation shows that we have some hope of this, for when physiology, psychology, ethics, and political science, now befogged by crude anticipations and futile analogies, have emerged from their childhood, they may work as much change on human affairs as the earlier-ripened physical sciences wrought on material progress. And so, remembering that the evil cosmic nature in us has the foothold of millions of years, and never hoping to abandon sorrow and pain, we may yet, in the manhood of our race, accept our destiny, and, with clear and steady eyes, address ourselves to the task of living, that we and others may live better.

It would be absurd to imagine that Professor Huxley considered this eloquent address a reasoned exposition, and indeed many of his own notes supply precisely those remarks which a hostile critic would have been glad to make. The sole occasion of criticism is that the address will have a positive scientific value assigned it. The dominant note in the address is that the process of life presents the "appearance of cyclical evolution." The "value of a strong intellectual grasp of the nature of this process lies in the circumstance that what is true of the bean is true of living things in general." This is really an argument from analogy, and has no value in a constructive system. The politician addressing those who agree with him, or the preacher strengthening his hearers in the faith which they share with him, use analogies with right and reason. But they are essentially rhetorical appeals. They become arguments only when by reason of some definite identity in the subjects compared they cease to be analogies. The Professor in his first note (on the word "appearance" in the sentence just quoted) shows that he introduces the example of the bean-plant as a mere analogy. The actual process true of all living things is not a cyclical but a linear evolution. A continuous chain stretches from every living being to the distant and prodigious first appearance of life. The successive individuals are pendants of the chain, and, dropping off, leave it unbroken.

Professor Huxley makes too much of the shifting impermanence of all things, both on his own account and in his interpretation of the old world philosophers. In the physical world impermanence means no more than constant movement; that energy rather than inert matter is the conspicuous phenomenon. In the mental world, it is true "that no man can, with exactness, affirm of anything in the sensible world that it is." But that is an ultimate metaphysical difficulty in a manner distinctive of all thinking, in a manner distinctive of none. The human mind is conditioned by its own limitations, but these limitations are conditions to be held in the background of all argument, an understood preamble in all thinking, and by no means to form an "in-and-out clause" in questions at issue.

Nor is it indisputable that Professor Huxley has assigned their proper value to pain and suffering in the world of life. Even if it were true that the consummation of pain is reached only "in man, the member of an organised polity," this were no isolated fact of far-reaching ethical import; but a fact in dependence on the increased intelligence of civilised man, an intelligence equally susceptible of increased pleasure as of increased pain. For suffering in the animal world, let Wallace and Darwin answer: "On the whole, then, we conclude that the popular idea of the struggle for existence entailing misery and pain on the animal world is the very reverse of the truth. What it really brings about is the maximum of life and of the enjoyment of life with the minimum of suffering and pain. Given the necessity of death and reproduction—and without these there could have been no progressive development of the organic world—and it is difficult even to imagine a system by which a greater balance of happiness could have been secured. And this view was evidently that of Darwin himself, who thus concludes his chapter on the struggle for existence: 'When we reflect on this struggle, we may console ourselves with the full belief that the war of nature is not incessant, that no fear is felt, that death is generally prompt, and that the vigorous, the healthy, and the happy survive and multiply.'"[2]

It is not, then, inevitable to regard the cosmic process as evil. Even when man, in various ages, had elaborated the conception of abstract goodness, and had endeavoured to make his justice a doling out of reward and punishment according to merit, it was not necessary to bring in a verdict of guilty against the cosmos. It is quite true that man has seen in all ages the sun shine on the just and the unjust. But it is an easy reflection that the world could not turn round on individual merit, and that if few are so guilty as to deserve the agonies of grief that may come to all, still fewer deserve some of the simpler and most common joys of life. Behind the "self-hypnotised catalepsy" of the devotee of Brahma, behind the aspirations towards Nirvana, behind the "apatheia" of the Stoics, there was a recognition of this worthlessness of the individual:

[2] Wallace's "Darwinism," p. 39.

an equable acceptation of oneself as part of a process: a triumph of intelligence over selfishness.

When we come to Professor Huxley's account of modern thought with its "tendency to move along the old lines to the same results"—that is, to the results of Indian and Greek philosophy—an extraordinary omission becomes obvious. Modern thought pays at least as strong a regard as did Indian or Greek thought to the position of the individual, and the inequality of individual lots strikes at least as harshly as ever against abstract conceptions of justice. But there is a new factor, the consolations of the ideals of conduct expressed in the New Testament. These bring happiness within the reach of the most unhappy, and have altered in the most fundamental way the principles of ethics. If they come with the authenticity assigned them by dogmatists, there is an end of the matter. If they are the climax of an inner spiritual law, which man himself is constantly shaping and perfecting for his own guidance, there is also an end of the matter. For then they would form as integral a part of the general cosmic process as any of the ape and tiger methods, and it would be as absurd to speak of the battle between ethical man and the immoral cosmos as is the well-worn argument of those defending miracles, that a law of nature (gravity) is interfered with when a man lifts his hand.

I can sum up my criticisms of this address very shortly:—

Evolution does not move in cycles, but in a chain.

Man's capacity for pain is a fact in dependence on other facts.

The "immorality" of the cosmic process is at least a matter of doubt.

If the events on this world are part of the plan of a divine creator, speculation on the immorality of the little we understand is futile.

On the other supposition the highest modern ideals and the brightest results to be hoped for in the future are a direct outcome of the chain of evolution, and it is rhetoric with no philosophic basis to write of the struggle between ethical and cosmic nature.

Our consciousness of the process of ethical amelioration, just as the part our consciousness plays in it, is a part of the cosmic process.

P. C. M.

SOME NEW BOOKS.

The Pursuit of Happiness: A Book of Studies and Strowings. By Daniel G. Brinton, A.M., M.D., D.Sc. 8vo. Pp. 280. Philadelphia: David McKay, 1893.

In his new book Professor Brinton, philologist, ethnologist, and Americanist—gifted with "the pen of a ready writer"—discourses as a physiologist, moralist, and philosopher. He takes for his subject the pursuit of happiness—to the Pessimist a chimera, to the Puritan a crime. Happiness he defines as not pleasure but built on it; its pursuit as the incessant prompting to a higher form of existence. Biologists have discovered, he states, that the avoidance of painful and the search for pleasurable sensations are the first principles of organic animal life, and those which have developed the amœba into the man, in whom it has blossomed into that self-consciousness to which he owes all the growth of his higher nature, his essentially human powers. "The yearning for joy is a cry of Nature." "It is the secret of evolution." Happiness is the reward of effort, but the truth of the Spencerian concept that "the greatest efficiency is the greatest happiness" is denied on the ground that it is historically false, for men most famous by their enormous personal capacity have certainly not been the happiest of their kind. Nor is childhood the happiest age, nor old age the wisest; there is a rabbinical saying, "He who teacheth the old is like one who writeth on blotted paper."

The fact that women are less happy than men is attributed first to inherent physiological differences and their consequences; secondly, to the errors of a false system of education. The author then proceeds to discuss the effects that our bodily and mental constitution, the laws of heredity, environment, and climatic influences produce on our happiness in general. Dr. Brinton does not, like Ibsen, make heredity accountable for everything, but shows how its sinister conditions can be ameliorated, if not actually annulled. He attributes his individual incapacity for the appreciation of music to the fact that musical instruments were excluded from the houses of his Quaker ancestors for five generations. The author then considers how far our happiness depends on ourselves and how much on others; reviews the pleasures of the senses, the emotions, the intellect; advocates the cultivation of Individuality as opposed to Egotism, the promotion of social happiness, and, in conclusion, recapitulates the consolations of the afflicted. In fact, Dr. Brinton's *Studies* might teach us all "how to be healthy and wealthy and wise." The *Strowings* consist of interspersed "Whitmanese" in the form of aphorisms more or less original and quaintly expressed, such as "Distrust the current estimates of great men. They alone are not tried by their peers." "Self distrust is nowhere more appropriate than in discussing difficult questions, and nowhere less displayed."

Dr. Brinton's latest contributions to physiology, phychology, and

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ethics, give evidence of thoughtfulness, wide reading, and that intimate acquaintance with human nature characteristic of the wise physician. He has produced a philosophical, up-to-date manual of the "Pursuit of Happiness," which, according to the American Declaration of Independence, is one of the inalienable rights of man. If there is no infallible guide-book to happiness, we may at least learn from this one how to avoid that morbid physiological condition which has been termed the disease of the century—*Ennui.*

In Dr. Brinton's opinion, "Our happiest moments are those in which we believe we can realise our ideals," but it has been well-said by his fellow-countryman, Lowell, "Woe to that man, or that nation, to whom mediocrity has become an ideal."

Agnes Crane.

The Evolution of Decorative Art. By Henry Balfour, M.A., F.Z.S., Curator of the Ethnographical Department (Pitt Rivers Collection), University Museum, Oxford. London: Percival & Co., 1893. Price 4s. 6d.

It is appropriate that this interesting little volume should come from the Pitt Rivers Curator, for, until General Pitt Rivers designedly studied the art of modern primitive races of mankind, any connected theory of the beginnings of design and decoration was an impossibility. Mr. Balfour has kept closely to General Pitt Rivers' original idea—that of studying the decorative art of the most primitive people now in existence, believing that the methods employed by man in all ages in reaching at conventional design from simple imitation of natural objects have been very much the same. Mr. Balfour's close study of the Pitt Rivers collection since it came to Oxford has enabled him to get much more definite and exact evidence of this process. Shortly speaking, there are two leading methods in the process. Everyone who was not born since the age of typewriters has dropped a blot of ink on his paper, and has found himself elaborating the stain into some fantastic form. So primitive man, finding a knot in the blade of his paddle, has stained and shaped it into a pattern, and has reproduced the pattern more or less rudely along the face of the blade; or, having trimmed the bamboo shaft of his spear, his eye has been caught by the regular pattern formed where the leaves are cut off at the nodes, and he has coloured the markings and the fringe-like scratches made where they cut off the blades. The transition from this to pure ornament comes when the decorative knots or node markings are reproduced upon a blade or stick where their natural origin does not exist. The second method is also familiar. We have all heard of the game called "Russian Scandal," where a simple statement or story, whispered through a chain of persons, appears in utterly unrecognisable shape when the version of the last person is compared with the original. The author has given two most interesting and amusing illustrations in which this little game was played by successive persons, each copying his predecessor's version of an original sketch. So among savage people, some unclad Raphael imitates the human figure on a weapon or a gourd, and future weapon-makers copy his representation of the natural object, until, from a possibly successful picture, a pattern apparently purely geometrical is produced. We have chosen two typical illustrations of the way in which the author deals with his materials, but the book is so short and so readable that it would be unfair, by prolonging this account, to give any reader the slightest excuse for refraining from the book itself.

Zoology of the Invertebrata. A Text-book for Students. By Arthur E. Shipley, M.A. Pp. 458, with many illustrations. London: Adam & Charles Black, 1893. Price 18s. net.

Although zoological text-books are strewn thickly as the leaves in Vallambrosa, it cannot be said that there is no room for Mr. Shipley's work. The smaller text-books are elementary and insufficient; the lapse of time and a quickly advancing science have left behind the once invaluable Claus. Others, English and foreign, are too much affected by the lamentable doctrine of selected types—a method that, however it be a necessary concession to the conditions of time and space in laboratory teaching, is pernicious in a text-book. Mr. Shipley makes a wise parade of the multiplicity of Nature. He does not send the zoological world spinning round on the earth-worm because there is great store of it in the back garden, nor regard the cray-fish as a prop of creation because it can be ordered from the fishmonger.

At the same time, in each larger group a fuller account is given of some important example in order that the most interesting modifications presented by other members may be set forth. Thus, to take an example, in the chapter on Nemertea, *Tetrastemma flavida* is most fully described, but in every point its characters are compared with the diverging characters of other members, and the diagrams and illustrations given are of general application, not special representations of the anatomy of the particular species. Or, again, in the section on Ascidians, the features of *Ciona intestinalis* are made the text, but particular description is avoided by constant reference to other Ascidians.

It may be said that a clear and definite idea of the groups is given, and that a student who reads this book along with the recognised courses of higher laboratory work will have a very good knowledge of the Invertebrata.

Following a time-worn custom, the author places at the beginning of each chapter a few lines in italics entitled "characteristics." These are given in the disjointed, unfinished phrases that invariable custom supposes tenable by the memory. Possibly they are necessary, but they seem a compromise between intelligence and the examination system. Mr. Shipley, it is true, calls them "characteristics," but they will be taken as definitions. A definition of a group of animals would not exist if the personal equation were allowed for, and it is more important that the mind of the student should be fixed on the connections than on the distinctions between the groups. His concept of the group must always be his knowledge of the animals in it. Curiously enough, the author, while giving "characteristics" for the Protozoa, Metazoa, and the groups like the Coelenterata, Porifera, and so forth, has not attempted them for the Coelomata and Acoelomata, but has remained content with the much more scientific method of writing a little chapter or section on them.

In classification, Mr. Shipley is convincing and ingenious.

The book is well illustrated, well indexed, and conveniently arranged.

The Lepidoptera of the British Islands, By Charles G. Barrett, F.E.S. Vol. I., Rhopalocera. Pp. viii., 313, pls. xl. London: L. Reeve & Co., 1893 Price £1. 10s. (with coloured plates, £2. 10s.).

This volume on British Butterflies is the first of a work on our native Lepidoptera, uniform with Fowler's "Coleoptera" and Saunders'

"Hemiptera." No work on the most popular of insect-orders, on such a scale as the present, has appeared for many years. The great increase of collectors and observers, while it has added vastly to the facts available for such a book, renders the author's task of sifting and arranging his material more onerous than in former times.

The introduction on the Lepidoptera generally occupies only ten pages, and anatomical statements are made very loosely. We are told that "insects of this order are never provided with jaws," that "they take food . . . by means of a flexible proboscis or trunk which is really a long, hollow, double tongue," and that they are "also provided with a pair of palpi attached to the labium or lower lip." Not a word informs the student that the "tongue" and the labium are each formed by the union of parts of a pair of jaws.

The old arrangement of the families is followed in the main, the Papilionidæ coming first and the Hesperidæ last. It seems a pity that the issue of this work was not used to familiarise British lepidopterists with Bates' order, now generally accepted, in which the Danaidæ stand at the head of the series, and the Papilionidæ are relegated to the place required by their affinities, just before the Hesperidæ.

In the systematic and faunistic portion of the work, however, which is in such publications the principal consideration, Mr. Barrett's long practical experience ensures a valuable result. After a good and original description of each butterfly, and its larva and pupa when known, we are furnished with notes on the variation shown by the insect, its localities, and habits. The attention paid to varieties is the feature of the work, and many remarkable local or aberrant forms are figured in the plates. But there is comparatively little suggestion on the vexed question of the causes of variation. Possibly this may be furnished in later volumes.

Naturalists will read with considerable interest the excellent notes on habits and localities given with each species, and will note with regret how many of our finest butterflies are on the road to extinction, if not extinct already. The generation of entomologists who saw *Chrysophanus dispar* alive is now passing away, and it seems that the present generation will see the last of *Aporia crataegi*, *Lycæna arion*, and *L. acis* as British insects. On the other hand, we have several additions recorded. *Danais archippus*, *Lycæna bætica*, *L. argiades*, and *Hesperia lineola* are now considered as entitled to a place in our fauna, though, owing to the absence of the food-plant of its larva, the first-named butterfly can never be more than a casual visitor to our shores.

The figures of the butterflies are generally good, but the caterpillars are often drawn in an attitude very suggestive of "blown" specimens.

The Hawks and Owls of the United States in their Relation to Agriculture. By A. K. Fisher, M.D., U.S. Department of Agriculture, Division of Ornithology and Mammalogy; Bulletin No. 3. Pp. 210, pls. 26. Washington, 1893.

Illustrated by well-executed coloured plates of twenty-six of the species described, this excellent little monograph of the diurnal and nocturnal birds of prey inhabiting the United States should prove valuable alike to the field naturalist, the agriculturist, and the game-preserver. It may be hoped, indeed, that its issue will do something to check the lamentable slaughter of these birds, which, if not stayed in time, appears only too likely to reduce many parts of the United States to the same condition as the British Islands in regard to birds of prey.

In the introductory chapter, the author enters into general considerations regarding the feeding-habits of hawks and owls, and comes to the conclusion that while a few of the former group are harmful to the poultry-keeper and game-preserver, in the greater number of cases the benefits conferred by the members of both groups far outweigh their depredations. A few species, moreover, like the swallow-tailed kite and the Mississippi kite, are absolutely beneficial, feeding entirely on reptiles and other noxious creatures. While the owls are chiefly instrumental in keeping in check the various species of rodents which are so harmful to the agriculturist, the smaller Accipitrines do equally good service in ridding the fields of the swarms of grasshoppers and kindred insects by which they are too often devastated. With this unmistakable verdict in their favour, we trust that we may ere long hear the last of the offer of bounties for the wholesale destruction of these interesting birds.

The total number of species of birds of prey recognised in this volume as inhabiting the United States is upwards of fifty, among which seventeen are owls, while the remainder, inclusive of the osprey, are Accipitrines. Many of these, it need scarcely be observed, are solely American, while others, like the duck-hawk, are merely varieties of Old World species, and others again, such as the golden eagle, differ in no respect from their representatives on this side of the Atlantic. The chief diagnostic features of each species are briefly but clearly given ; while especial attention is directed to the areas of distribution. The especial feature of the book is, however, the care which has been bestowed in ascertaining the precise nature of the food of each species ; for which purpose elaborate tables are given, describing the contents of the stomach of a large series of specimens, reaching in some cases to over a couple of hundred. Such researches must have entailed enormous labour on the part of the author and his assistants, who, with the Department, deserve the thanks of all ornithologists.

R. L.

Handbook of British Guiana. By James Rodway. 8vo. Pp. 93. Georgetown British Guiana, 1893.

Mr. Rodway is well known to the readers of Natural Science ; in a series of articles full of life and interest he has made us familiar with the forests of British Guiana, and now he gives us a general description of the country, its inhabitants, climate, geology, fauna flora, industries, its past history, and its resources and capabilities for future progress. Besides a map and a chart showing the steamer routes, there are five-and-twenty excellent plates reproduced from photographs, bringing vividly before us, now the splendid falls of the Masaruni or the Kaieteur, now the solitary grandeur of Roraima, towering upwards like a gigantic castle from a slope six thousand feet above sea-level, an immense sandstone rock, with an area of 32 square miles ; or we are looking down on the walls and roofs of Georgetown, or New Amsterdam, standing out sharp and clear in a setting of tropical vegetation, or it is the vegetation itself which forms the subject of the picture, the dense tangle of the forest, a clump of palms, or the *Victoria regia*, with its broad tray-like leaves quite hiding the surface of the water.

Along the coast is a fringe of alluvium so fertile that alternation of crops is unknown, sugar-canes growing year after year in the same soil with hardly any deterioration. Plantains, Indian corn, yams, sweet potatoes, and a host of fruits ripen all the year round; at present hardly a tenth of this is under cultivation. Behind lie swamps choked with tall sedges, with here and there an island of sand on which a few trees and bushes manage to exist, or wherever the land begins to rise, grand clumps of the magnificent Eta palm (*Mauritia flexuosa*). Then come the sand-reefs, the white beach of ages long ago. Miles and miles of pure sand, washed clean as driven snow, dazzle the eyes in the glare of the noonday sun. Behind the reefs the flatness of the coast gives place to an undulating country, gradually rising to hills, and then mountains, and clothed for its greater portion with primeval forest—part of the wilderness of teeming vegetation which covers most of tropical South America. This wilderness abounds with riches. There is gold in the river beds, the working of which increases every year; but of incalculably greater value is the timber in the forest. For piles, wharves, and other structures more or less immersed in water, greenheart timber, says the writer, is perhaps the very best in the world. It is also classed with teak and oak by "Lloyd's" as the most suitable for ship-building. Mora is another first-rate timber for house-frames and ship-building, and although not so lasting as greenheart, "better even than oak for all purposes to which that excellent wood is applied." Wallaba is a very useful wood, as it is easily split into shingles and paling staves, and is far more durable than oak for the large vats necessary for water storage. Among the hundreds of varieties of woods found in the colony are some of the hardest and heaviest in the world, as well as others that are light. They vary, too, in colour from nearly black through the various shades of brown to almost pure white. Where the forests are broken are the savannahs, a paradise to the botanists, especially in the mountain region. Among foaming rivulets, running through banks of ferns and mosses, grow some of the most beautiful flowering plants—orchids, utricularias, droseras, and a hundred others—in the wildest profusion.

"The climate of the colony is on the whole a very pleasant one." The variations of the thermometer are but slight, generally from 76°—86° F., while on the coast the sea breeze tempers the greatest heat, and the heavy rains cool the earth and cause so luxuriant a vegetation that an arid tropical appearance is hardly ever present. Sometimes, however, a protracted dry season brings a drought. Destructive earthquakes and hurricanes are not experienced, while accidents from lightning are said to be almost unknown. Besides large areas on the coast, thousands of square miles of fertile river bottoms and valleys in the interior await cultivation. The great difficulty is the ever-present labour question. The emancipation of the slaves caused the ruin of many estate owners, and led to the plantations being abandoned, the negroes refusing to work at all except for wages which, with the low price of sugar, the staple product, no one could afford. Labour is now obtained by immigration from East India and China, but the great want of the colony is settlers, energetic and industrious, with some capital, and prepared to rough it a little at first.

For further information on the colony, its beauties and resources, we must refer the reader to Mr. Rodway's very readable little handbook.

MODERN METEOROLOGY. By Frank Waldo, Ph.D. [Contemporary Science Series.] London: Walter Scott, 1893.

ON taking up this book, which is one of the Contemporary Science Series, the attention is at once fixed upon the title. Why should the author have chosen the exact title of a well-known existing work? This feeling of surprise is intensified by further acquaintance with the contents of the book. It is not an account of the present state of the Science of Meteorology. It neither completely supplements nor replaces Scott's and Abercromby's work, neither is it a picturesque and popular version. What is it then? It is a series of fragmentary essays dealing with certain branches of Meteorology. There is much detail given concerning the internal arrangements and routine of observatories, and there is a long account of the theories of Von Bezold, Hann, and Ferrel upon the circulation of the atmosphere. Indeed, the value of the book lies chiefly in this latter portion, which will be very useful to those English and American meteorologists who cannot read the works of the German authors themselves. Whole sections of the science are either omitted or referred to in a most sketchy manner. From the preface it is clear that Dr. Waldo is fully aware of the true character of his work, inasmuch as he remarks: "As I have been mainly a student of what may be termed the German School of Meteorology, I probably have not brought into sufficient notice the names and work of French, English, and Italian meteorologists." In other words, such advances as have been made by English, Scottish, French, and Italian meteorologists are scarcely referred to in a work called "Modern Meteorology"! Let the author change the title to "Some Chapters in Meteorology," or "Meteorological Essays," or some similar description of his work, and there will be less reason to quarrel with him; though, even then, it will be a matter of astonishment that anyone can reprint the absurd picture on page 193. In many ways the work is good and useful, but the general result is disappointing.

OBITUARY.

KARL SEMPER.

Born July 6, 1832. Died May 29, 1893.

We regret to have to record the death of the eminent Professor of Zoology in the University of Würzburg. Born at Altona, near Hamburg, sixty-one years ago, Karl Semper was the son of a distinguished architect of that city. At an early age he entered the University of Würzburg, devoting himself especially to the study of Zoology. During the years 1859-61 he made a scientific exploration of the Philippines, and in the following year pursued zoological researches in the Pelew Islands. In 1864 he proceeded again to Mindanao, in the Philippine Islands, and in 1866 returned to Würzburg as Privat-docent in the University. Two years later Semper became extraordinary Professor of Zoology in the same University, and in 1869 he succeeded to the ordinary chair. A new Zoological-Zootomical Institute was subsequently erected, and of this the Professor became director in 1872. His most important work is the series of five volumes detailing the scientific results of his travels in the Philippine Islands, published in instalments between 1867 and 1886, partly in co-operation with other zoologists in special departments. In 1877 the Professor was invited to deliver a course of lectures at the Lowell Institute, Boston, and on this was based the work by which he is best known to readers in this country, "The Natural Conditions of Existence as they affect Animal Life." Apart from his own researches, Professor Semper made great contributions to the progress of Zoology by directing the work of his pupils into profitable channels; and the series of volumes of "Arbeiten," published by the Zool.-zootomical Institute of Würzburg, is filled with scientific results of great value.

The death is also announced of Carl Friedrich Nyman, of Stockholm, author of the well-known *Conspectus Floræ Europeæ*; of Henry Eliason Seaton, the young and promising Assistant Curator of the Gray Herbarium, Harvard University; and of William Reid, of Nairn, a keen observer in ornithology and ichthyology, to whom many authors own much indebtedness.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Professor Baldwin Spencer, of Melbourne, is spending his vacation this year in Europe, leave of absence having been extended until December.

Dr. W. Deecke, the well-known palæontologist of Greifswald, has been appointed extraordinary Professor of Palæontology in the University of that city.

Mr. H. Yule Oldham, Lecturer in Geography in the Owens College, Manchester, succeeds Mr. J. Y. Buchanan as Lecturer at Cambridge.

Mr. E. B. Poulton, F.R.S., has been appointed Hope Professor and Curator of Entomology in the University of Oxford, in succession to the late Professor Westwood.

Mr. W. Fisher, late of the Department of Forestry in India, has been appointed Assistant Professor of Forestry in the Royal College, Cooper's Hill.

The honour of knighthood has been conferred upon Dr. B. W. Richardson, F.R.S. The University of Durham has conferred the Honorary Degree of D.C.L. upon Sir Henry H. Howorth, F.R.S.

The University of Pennsylvania conferred, on May 10, the honorary degree of "Doctor of Science" on Daniel Garrison Brinton, M.D., LL.D., of Philadelphia, Professor of Ethnology in the Academy of Natural Sciences of that city and of American Archæology in the University of Pennsylvania, in recognition of his numerous contributions to linguistics, ethnology and American archæology.

The University of Dorpat, now Jurjeff, is beginning to issue its publications, according to the Imperial decree, in the Russian language. We have just received what appears to be an important thesis on the development of the carpus and tarsus in mammals, illustrated by three beautiful plates; but the work is not accompanied even by an abstract in French or German, and it will thus be of little service to the majority of anatomists interested in the subject.

According to *Indian Engineering*, a Committee for the establishment of a Pasteur Institute in the Punjab Himalaya has held its first meeting at Lahore, when a working scheme was decided upon, and it was determined, in the first instance, to ascertain from the Government of India the conditions under which a Pasteur Institute, supported by the public, could be amalgamated with the Imperial Bacteriological Laboratory which is to be transferred from Poona to the Punjab Himalaya.

The Rothschild Museum, Tring, has just received from Madagascar some limb-bones of the extinct struthious bird, *Æpyornis*. In size they exceed the largest known bones of *Dinornis* from New Zealand. They seem to indicate that the tibia of the Madagascar bird was relatively shorter than that in the New Zealand genera.

We regret to learn that the fund collected by the Owen Memorial Committee is still insufficient to carry out adequately the resolutions adopted at the meeting of subscribers held early in the year. It is to be hoped that the proposal to place a marble statue of the late Sir Richard Owen in the great hall of the Natural History Museum at South Kensington will not lapse from want of pecuniary means; and we would urge all who have not yet subscribed but who desire to honour the memory of the deceased Naturalist, to forward their donations without delay to Sir William Flower, Treasurer of the Committee.

According to a recently-issued circular, the total amount of the contributions received is only £935, and in the list of subscribers many well-known names are conspicuous by their absence. We are surprised to notice how small a proportion of Sir Richard Owen's former colleagues in the British Museum and other scientific departments of the Government have responded to the appeal. The name even of the present Hunterian Professor at the Royal College of Surgeons does not appear in the list.

Mr. R. W. Atkinson, for six years Honorary Secretary of the Cardiff Naturalists' Society, has resigned office, and Mr. Walter Cook succeeds him.

At the anniversary meeting of the Linnean Society on May 24, the gold medal was conferred on Professor Daniel Oliver, F.R.S. On presenting the medal, the President, Professor Stewart, referred to the wide character of Professor Oliver's work, including besides his extensive contributions to systematic and geographical botany, papers on the morphology and anatomy of plants. The "Lessons in Botany" was mentioned as the most useful elementary book we have, and the President also referred to the thirty years' tenure of the chair of botany at University College.

From the Annual Report of the Essex Field Club for 1892 (*Essex Naturalist*, April–May, 1893), we learn the final arrangements for the amalgamation of the Essex and Chelmsford Museum with the Essex Field Club. The collections of the two societies are now in process of arrangement in the old Museum at Chelmsford pending the erection of new buildings. The subscribers to the old Essex and Chelmsford Museum have been admitted as Members of the Essex Field Club, which now increases its ranks by nearly a hundred persons. The headquarters of the Club will shortly be removed to Chelmsford.

At the Annual Meeting of the Haslemere Natural History Society, held on May 29, Mr. Jonathan Hutchinson, F.R.S., was elected President for the ensuing year. The Society proposes in future to publish "Records of Lectures and Addresses" delivered at its meetings. A Museum is also in course of formation, but the Society cannot hope for much success in this direction until it is able to decline the offers of those well-meaning but mistaken donors, who are likely to convert it into a "curiosity shop."

Dr. R. Blanchard, Secretary of the International Zoological Congress, announces that the subject for the first award of the prize offered by Czarewitch of Russia is "A Study of the Fauna of one of the great Regions of the Globe and its Relations to the neighbouring Faunas." Manuscripts must be received before May 1, 1895, and they may relate either to a Fauna in general or to one particular class of animals. The award will be made at the Leyden Meeting in 1895, on the report of a Committee consisting of Messrs. Milne Edwards, Blanchard, Bogdanov, Zograf, Jentink, Studer, and Bowdler Sharpe. Detailed particulars may be obtained from the office of the Congress, 7 Rue des Grands-Augustins, Paris.

The following letters have been circulated this month among the Fellows of the Royal Geographical Society of London :—

"Savile Row, W.
"1st June, 1893.

"Dear Sir,—At the recent Anniversary Meeting of the Royal Geographical Society, in the course of a discussion on the admission of ladies to its Fellowship it was suggested, and the suggestion met with the approval of the meeting and was accepted by the President, that the best course would be to obtain the opinion of the body of the Fellows throughout the country. General R. Strachey—acting in the absence of Mr. Clements Markham—has consequently directed me to send you the accompanying note and postcard. A general vote, thus taken, is not formally binding on the Society, but its result will, there is every reason to believe, be acquiesced in and confirmed by a General Meeting, as a conclusion to the recent controversy.

"The admission of ladies to the Society is no new idea. It was accepted in principle as a measure both of justice and expediency by the Council in 1887. Its application was deferred to a convenient occasion, and that occasion arose when last year an eminent lady traveller, Mrs. Bishop (Miss Bird), declined to furnish a paper to a Society that would not acknowledge her as a geographer. I need not catalogue the many other ladies, from our Gold Medallists, the late Mrs. Somerville and Lady Franklin to Miss Edwards and Miss North, whose names are well-known to all interested in the literature and results of travel.

"The ladies the Council particularly desire to see admitted as Fellows are, in the first place, travellers who contribute to geographical knowledge, and also ladies who have so warm an interest in geographical progress that they desire regularly to attend our meetings and to use our library. Up to the present time, such ladies, if widows or unmarried, have only been able to be present at meetings by begging a ticket on each occasion from a Fellow. The Society has refused their subscriptions, and they have been debarred from the privileges accorded to members. Is not such a state of things an anomaly at a date when ladies are already admitted as members of a large proportion of the more important Scientific Societies of the metropolis, and of, I believe, all the other Geographical Societies in the United Kingdom and the Colonies? It appears in this light, not only to the Council, but also to Fellows of such experience as Lord Northbrook, Lord Brassey, Sir John Lubbock, Sir Alfred Lyall, Sir Richard Temple, and Sir William Flower, P.Z.S., and Director of the Natural History Museum, not to mention many others. In my own twelve years' experience as one of the Honorary Secretaries of our Society, I may venture to subscribe to their opinion and to express my belief that the admission of qualified ladies as Fellows will add to the prosperity and reputation of the Society, and greatly conduce to its main object—the growth of geographical science—and to geographical knowledge.

"Some of our Fellows have recently found a legitimate subject of complaint in the fact that, owing to the extensive use by Fellows of their privilege of admitting a companion, the best seats at the most interesting lectures have sometimes been half filled by ladies. It must be pointed out that the Council have recently taken a step that will remedy this grievance by modifying a bye-law, so as to empower the officers, on occasions of exceptional interest, to seat all Fellows before any guests. It would obviously be unjust to exclude qualified ladies from the Fellowship because unqualified ladies, admitted as visitors, have occasionally occupied an unfair proportion of our benches!

"The Council, deeply impressed by the importance of geographical knowledge to the generation of the English people, and noticing every day how its absence is felt in questions of politics, of commerce, or of emigration, trust you will be able to agree with their view, and to answer the question submitted to you in the affirmative. "I am, yours faithfully,

"Douglas W. Freshfield, Hon. Sec."

"Royal Geographical Society,
"1 Savile Row,
"Burlington Gardens, W.,
"5th June, 1893.

"Sir,—I am desired by the Council of the Royal Geographical Society to inform you that Mr. Freshfield's letter of June 1st was written and issued without their authority or knowledge, and that his action in so writing has been disavowed by them. The Council as a body have no wish to influence the judgment of any Fellow in the matter.

"I have the honour to be,
"Your obedient servant,
"R. Strachey, Vice-President."

We need not add to our remarks last month. So far as we can judge, all the Royal Geographical Society's difficulties have arisen from the usurpation by the Council of powers exclusively belonging to the Fellows assembled in a Special General Meeting; and the time for granting the Fellowship to ladies has thus been unfortunately postponed.

During the absence of Mr. Theodore Hughes in England, Mr. T. D. La Touche undertakes the duties of Superintendent of the Geological Survey of India. We briefly referred last month to the new orders under which the work of the Survey will for the future be directed more especially to subjects of economic interest, and we are now able to add some further particulars. It is ordered that two-thirds of the officers shall always be primarily engaged on the explorations necessary to complete the geological survey, and the remaining third on the practical investigation of mineral fields. In the latter case, the exploration will be confined to such preliminary researches as may be necessary to supply general information regarding their character and extent to capitalists and promoters, to whom will be left the responsibility for more detailed prospecting. According to the recently-issued annual report, the most important scientific work of the past year was that carried out in the Central Himalaya by two of the survey officers in co-operation with Dr. Diener, the eminent geologist of Vienna, and their joint researches are said to be certain to prove of high scientific interest. There were experimental borings for oil in Beloochistan and on the Indus, investigations into the Nerbudda coal-fields, the coal resources of Tenasserim, jade in Burmah, coal and rubies in the regions bordering on Yunnan, and much else of a similar character. The advance in the knowledge of Indian coal-fields has been great. The centres of production, which a few years ago were almost confined to Bengal, have now been extended to Assam, Punjab, Central Provinces, the Nizam's territory, and Burma. The survey has also done much to determine the character of the oil resources of the country, and soon the question will be set at rest whether any important or permanent supply of oil can be secured in Northern India.

CORRESPONDENCE.

The Recrudescence of Leprosy and its Causation.

As the readers of Natural Science would scarcely comprehend the full scope and purport of my recently-published volume, "The Recrudescence of Leprosy and its Causation," from the brief notice in the April number, I shall be glad to be allowed space for a summary of my conclusions, the result of personal investigation in many countries where this terrible disease is endemic. I can prefer this request with the more urgency from the fact that many of the sufferers from leprous vaccination are absolutely friendless, while most of them are without political representation and are unable to plead their own cause. In the pursuit of my inquiries I have been repeatedly entreated to bring their intolerable grievances before the English-speaking public through the Press, with a view of reaching the ears of Parliament and other legislative bodies.

These conclusions are as follows :—

(1.) That leprosy has greatly increased during the last half-century, and that it is prevalent in many places where it was formerly unknown.

(2.) That whilst the opinion of medical authorities and experts varies considerably on the subject of the contagiousness of leprosy, the preponderance of authority is in favour of the theory that it is not contagious in the ordinary sense of the term, but is communicable by means of a cut, sore, or abraded surface ; and this view is confirmed by my own personal investigations.

(3.) That other alleged factors such as malaria, a fish diet, syphilitic cachexia, heredity, and insanitation are admittedly unequal to explain the rapid growth of the disease in certain of our Crown colonies and dependencies, as well as in other countries.

(4.) That on one point there is much agreement, and hardly any dissent, namely, the inoculability of leprosy ; and that the view of leprosy as an inoculable disease, while it is most clear to those who take the malady to be due to a bacillus, is older than the bacteriological evidence, and is not dependent thereon.

(5.) That the most frequent opportunities of inoculating the virus of leprosy are afforded in the practice of vaccine inoculation, which is the only inoculation that is habitual and imposed by law ; and that the evidence here adduced is calculated to show that vaccination is a true cause of the diffusion of leprosy.

(6.) That the official information, collected by interrogatories and otherwise, has not been hitherto of a kind to show how far vaccination has determined the amount of leprosy in recent times ; and that any interrogatories that may be sent out in future should not be limited to ascertaining the effects, as regards leprosy, of hypothetically "pure lymph." When, on very rare occasions, interrogatories have been submitted, they have been framed to ascertain the results of a purely hypothetical system of vaccination, which is not anywhere discoverable in practice (*i.e.*, with pure lymph, and free from hereditary taint), and the replies are therefore futile and misleading.

(7.) That with the exception of two groups of cases—those adduced by Dr. Roger S. Chew, of Calcutta, and Dr. S. P. Impey, of Robben Island—those reported in this volume have not been the result of special investigations, but have cropped up accidentally in the course of medical practice, and in some instances have been published by practitioners, with apologies to the profession for presenting such unwelcome disclosures.

(8.) That the increase of leprosy in the Sandwich Islands, the West Indies, the United States of Columbia, British Columbia, British Guiana, South Africa, and New Caledonia has followed *pari passu* with the introduction and extension of vaccination, which in nearly all these places, without previous inquiry or demand from the inhabitants, has been made compulsory.

(9.) That as leprosy is a disease of slow incubation, often taking years to declare itself, and in its incipient stages can be detected only by practitioners of large experience, it follows that, in countries where leprosy exists, there is great danger of extending the disease by arm-to-arm vaccination.

(10.) Leprosy being one of the most loathsome diseases to which the human race is subject, and being practically incurable, it behoves all interested in the public well-being to do their best to *prevent* its diffusion, and as a means thereto, to discourage the practice of vaccination on that ground, if on no other.

Devonshire Club, St. James's, S.W. William Tebb.
24th May, 1893.

Mr. Hick and *Calamostachys Binneyana*, Schimp.

In the letter which appears in the last issue of Natural Science (vol. ii., p. 480), Professor Williamson announces that the "true answer" to my article on "The Fruit-Spike of Calamites" (Natural Science, vol. ii., pp. 354–359) will be given in a memoir which he and Dr. Scott hope to present to the Royal Society before the close of the present year. May I, on my part, refer those who are interested in the matter to the forthcoming volume of the *Proceedings of the Yorkshire Geological and Polytechnic Society*? There, a paper will be found in which I have given a full description of the beautifully-preserved specimens on which the article is based, so far as it deals with *Calamostachys Binneyana*, Schimp., as the fruit of some form of *Calamites*.

Thomas Hick.

TO CORRESPONDENTS.

All communications for the Editor *to be addressed to the* Editorial Offices, *now removed to* 5 John Street, Bedford Row, London, W.C.

All communications for the Publishers *to be addressed to* Macmillan & Co., 29 *Bedford Street, Strand, London, W.C.*

All Advertisements *to be forwarded to the sole agents,* John Haddon & Co., *Bouverie House, Salisbury Square, Fleet Street, London, E.C.*

ANSWERS TO CORRESPONDENTS.

Henry Johnson.—Laurence Oken's library was sold by auction on 17 May, 1853, at Zürich. It consisted of 5,881 items, 600 of which were Historical, Classical, and Theological books, the rest dealing with the Natural Sciences.

C. W. K. (Leeds).—You will find that Hagen's *Bibliotheca Entomologica* (Leipzig, 1862) gives the majority of writings on Insects previous to the date of its publication. There is a good index, and you can find out the papers on the special group you want. After Hagen, the volumes of the *Zoological Record* should be consulted.

NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

No. 18. Vol. III. AUGUST, 1893.

NOTES AND COMMENTS.

The Exploration of East Africa.

AMONG the latest news of interest that reaches us this month is a communication from Dr. J. W. Gregory, giving a brief account of his expedition in East Africa up to the date of writing, May 8. It appears that he left Mombasa on April 23, taking with him a small force of only forty men, thus hoping the more easily and quickly to traverse any territory where food was scarce. He passed Kilwezi, the Nyika plateau, Nzoi, Kilungu, Zuni, Machakos, the Athi river, Lanjoro, and through the Kikuyu country to Fort Smith. His present letter reports very satisfactory, and is dated from Longenot, 450 miles from the coast, and to the north of Lake Naiwasha, Masai-land.

Dr. Gregory has had little trouble with his men; there have not been any desertions; and food, though not abundant, was to be had; indeed, he says that on the banks of the Malewa he lived in luxury, game and birds being abundant, and a whole sheep costing less than a 2lb. tin of corned beef. There are no vegetables, except Indian corn and dry beans, of which he bought a ton at Fort Smith. His progress has been much delayed by heavy and persistent rains.

Dr. Gregory states that he has found *Senecio johnstoni* (a remarkable composite tree which attains a height of from 20 to 25 feet, previously recorded three times from Kilima-njaro, at heights over 8,500 feet) in considerable abundance near Naiwasha. His traverse of, and occasional branch excursions into, the Ulu Mountains have yielded considerable evidence with regard to the earth-movements of that district. He has found traces of the former existence of a vast lake to the south of Naiwasha, which was probably destroyed by the east and west line of elevation to which the volcanoes of Longenot and Kenia were due.

The hostility of the Masai, who happened to be on the war-path some 9,000 strong at Naiwasha, at the time of his visit there, compelled Dr. Gregory to leave that district more quickly than he wished, and this prevented him from making any examination of the lake fauna. He reports, however, that he found the site of a factory of stone implements used by some prehistoric tribe, and has traced one of their trade routes across a pass still used by some caravans. He expected to leave Lake Barengo (his farthest point) by the end of May, and to spend June and July exploring in the Aberdare Mountains, and on and around Mount Kenia. No further news can be expected of him until he reaches Brindisi on his return journey, about the end of September, for it is more than likely that he will miss the outward mail from Mombasa at the end of August. Save for a chill, caught while crossing a river, Dr. Gregory's health since he left the coast has been excellent.

"Borderland."

Mr. W. T. Stead has sent us a circular containing information about his new Quarterly, *Borderland*, and a letter asking our views on the subject, as we are among those "whose opinion weighs much with the public," and from whom may be sought "such wise words of guidance in this new enterprise as he can collect from the most eminent of his fellow men."

It were easy to treat Mr. Stead's adventure with mere chaff or with ridicule. But Mr. Stead already has exploited the credulity of the public in the matter of red and blue electricity, and so competent a journalist doubtless will succeed in exploiting the superstitions of the class for which he caters. In this process it is "even money" that many will believe in the utility of his methods, while the odds are not long against his own belief in them. So he shall have our "wise words." He proposes to form a series of circles of students to study occult phenomena, and anyone may join a circle on payment of ten shillings per annum. The occult phenomena to be studied by this "veritable college of the occult sciences" are, we gather from the letter and circular, "the powers of the unconscious mind, multiple personality, invisible intelligences with whom we may profitably enter into communication, and the persistence of the individual after death."

These are questions of great interest, and the collection of data concerning them is of first-rate importance. But the evidence of those not trained to observe is misleading about the plainest occurrences: it will be absolutely worthless about phenomena so complicated by subjective and objective deception as these phenomena notoriously are. More than one expert in some science has been attracted to the investigation of spiritualism and has abandoned it, because of the ensnaring and degrading fraud associated with its votaries. And a college chosen on Mr. Stead's plan will be a mixture of confiding simplicity and designing impudence. For the serious treatment of the obscurer questions of the mind there are in abundance laboratories of physiological psychology, trained specialists in the laboratories, and technical journals for the publication of their researches. The idea that science "has contemptuously relegated to superstition" such problems exists only in the minds of busybodies who have been snubbed. There are competent experts at work, and were the geologist to lay down his fossil and the anatomist his scalpel to follow Mr. Stead into the wilderness, they would aid only the wind to shake the reeds until such time as they turned to the stoning of the prophet who led them out. As for the "dangers" surrounding the study of occult science, let no fluttered neophyte be deceived. The dangers are real; though he will not be thrilled by any devil of his own raising, if the germs of imbecility lie in him they may well be scared into activity by the fraud of his associates.

Sierra Leone.

The Botany and Geology of Sierra Leone are the subject of a recently-issued Colonial report compiled by Mr. G. F. Scott Elliot, and embodying the results of his observations as botanist to the late Anglo-French Boundary Commission. Mr. Elliot has already laid before the Linnean Society the purely botanical results of the expedition, and in the present report, dealing with the economic aspect, has brought together a good deal of interesting and valuable information on the native or cultivated plants of the colony. A number of geological specimens have been named and described by Miss C. A. Raisin, of University College. The only mineral of importance is iron, of which the country apparently contains a very large amount. A specimen of titaniferous ore from a rich belt in the hills behind Sierra Leone, proved on analysis to contain 52 per cent. of the metal.

In speaking of the fertile alluvial soil along the coast and rivers, the effect of the mangroves in reclaiming land from the sea is described: "the trees seem, in fact, to have been designed by nature to change any bay or indentation of the coast-line into fertile soil." They require brackish water, and wherever a mudbank is in process of formation, there they grow, gradually advancing seawards as the silt accumulates. The trunk divides at the base into six or seven curved buttress-like roots, each of which by repeated sub-division covers a wide area with grasping supports. Moreover, from every branch long hanging roots descend vertically, dividing about the level of high tide into grasping fingers, which grow down into the water and take root so firmly in the silt that they cannot be torn up by any ordinary force of current. This meshwork of roots and rootlets catches and holds the fallen leaves of the mangroves, and all the silt and soil in the water, and the accumulation rapidly advances. Gradually the level of the soil thus formed rises above high tide, and then the mangroves, which require a constant supply of brackish water, die off, and the whole grove advances seaward, leaving behind it a mass of rich vegetable alluvial mud, better suited for rice than probably any other soil in existence. Tobacco, sugar, and cotton would, doubtless, also do well. The part bordering the sea is adapted for the oil palm, and, presumably, also the coco-nut.

The rest of the country consists chiefly of low rolling hills or downs, covered by almost impenetrably thick bush 20 to 30 feet high, or tall grasses from 8 to 10 or even 15 feet. The upland plateaux, above 2,000 feet, would probably form excellent grazing ground.

Mr. Elliot concludes that, from a botanical point of view, the country is more suited to most valuable tropical plants than either Jamaica or Mauritius, but the climate is in most parts dangerous for Europeans, and neither energy nor physique can be kept up sufficiently to make proper use of its natural advantages.

Discovery of an American Type of Extinct Mammal in the Balkans.

As our palæontological readers are probably aware, those remarkable gigantic extinct Perissodactyle mammals known as the Titanotheriidæ (together with the Uintatheriidæ) have hitherto been regarded as confined to the Tertiaries of North America. Herr Vacek had, however, suggested, as far back as 1877, that a species from the Lower Tertiary of Transylvania, described under the neat and concise name of *Brachydiastematherium transilvanicum*, which has been generally regarded as a member of the Chalicotheriidæ, might really belong to the American family.

In a recently-published paper (*Sitzb. Akad. Wien*, 1892, pp. 608–615) Dr. F. Toula describes two mammalian teeth from Miocene beds at Kalina, near Sofia, in the Balkan Peninsula, which seem to leave no doubt that the family in question was really represented in the Tertiaries of Europe. The specimens consist of two very large lower molar teeth, which pertained, as shown by their different degrees of attrition, to as many individuals; one being the last and the other the penultimate of the series. That they belong to Perissodactyles there can be no question; and the last tooth differs from the corresponding molar of all the large Perissodactyles of Europe in possessing a third lobe, whereby it is at once distinguished from *Chalicotherium*. Dr. Toula refers the specimens provisionally to the American genus *Menodus*, under the name *M. rumelicus*,—a reference which we believe to be perfectly correct, save that the name ought to be *Titanotherium rumelicum*. He also supports the view that *Brachydiastematherium* is likewise a member of the same family.

We had been inclined to flatter ourselves that we were fairly well acquainted with the mammalian palæontology of Europe, and that there was not much hope of the discovery of any strikingly new types. However true (or false) this may be in regard to the mammals of Western Europe, it is quite certain that it will not hold good for those of Eastern Europe, where, doubtless, a large field awaits the investigator. That it is in this half of the Continent that American types would naturally be expected to occur, is self-evident, if the view that the migration from the one hemisphere to the other took place by way of Behring Strait be correct. The occurrence in Central Asia of deer closely allied to the Wapiti, and of an alligator whose nearest living ally is to be found in the Mississippi, are both strong evidences in favour of this view, which receives additional support from this new and most interesting discovery. We trust that ere long upper molars of the new fossil will be forthcoming, whereby its affinities may be more thoroughly indicated.

Plant Pitchers.

The recently-issued number of the *Annals of Botany* (June) contains two papers dealing with the pitchers of *Dischidia rafflesiana*, a twining epiphyte of the family Asclepiadaceæ, growing on trees in the forests of the Malay Archipelago. Mr. Groon has studied the live plants at Singapore, and also worked with spirit material in this country. Dr. Scott and Miss Sargant, the authors of the second paper, have had the advantage of living specimens now growing at Kew, sent two years ago from Java by Dr. Treub.

Study of the anatomical structure completely confirms Treub's view that the pitcher is a modified leaf, the upper surface of which grows more rapidly than the lower, so that the outer surface of the pitcher represents the upper surface of the leaf, and the inner surface of the pitcher the lower surface of the leaf. The mouth is small, with deeply incurved lips, and directed sometimes upwards, sometimes downwards. The stalk gives off roots which enter and ramify within the cavity, which contains a quantity of earthy particles, small stones, fragments of leaves, etc., and frequently water. Dead and decaying insects were also found in small quantity, and often ants, the latter sometimes abundantly. The first author thinks that the solids are mainly brought by ants, a view with which Mr. H. N. Ridley agrees, regarding them as the remains of ants' nests; having observed also that the earth in the pitchers resembles that at the foot of the host tree. As regards the function of these structures, Delpino's view—that the plant is carnivorous, the pitchers serving to catch insects, which are drowned, and then help to nourish the plant—meets with no support; as Treub has already objected, the roots afford a most convenient way of escape to any insects which might find their way into the cavity. There is, indeed, no provision either for attracting or retaining insects, except, perhaps, the allurement of a shady resting-place; and Mr. Groom is inclined to lay stress on the last point, suggesting that the existence of species with merely concave leaves indicates the evolution in the pitcher of an organ more perfectly adapted to provide shelter for the ants on the one hand, and on the other to secure for the use of the plant the materials collected by them. At the same time, he recognises the function of storing up rain-water and the substances washed down therewith. Dr. Scott and Miss Sargant also attach considerable importance to the detritus in the pitchers, which is proved to serve as a food-supply. They, however, find no evidence of a myrmecophilous (ant-loving) habit, and agree with Treub that the detritus is washed in by the rain, the pitchers serving to collect rain-water, and in a less degree to economise the water of transpiration. The upright pitchers, they remark—those, namely, with the mouth at the base—"can have no other function than to store up the water given off as vapour in transpiration. We have shown, on anatomical evidence, that the inner surface of the pitcher is the chief transpiring surface of the plant. The condensed water of transpiration is undoubtedly re-absorbed by the roots." It is difficult to follow this last observation. A pitcher turned topsy-turvy could only store water in the reflexed rim, and as moisture clinging round the roots, which latter would doubtless be soon re-absorbed; but why should the vapour transpired condense to any appreciable amount within the pitcher? What plays the part of condenser? Surely the economy in water of transpiration is effected merely by the restriction of the area into which it is transpired, which, by becoming a still atmosphere saturated with water vapour, will retard further transpiration.

The same authors give an interesting account of the purple colouration on the inner surface of the pitcher, which contrasts strikingly with the pale green of the outer surface. Spectroscopic examination of the purple layer, also of an alcoholic extract of the pigment, showed a considerable absorption of the yellow and green, and almost complete absorption of the blue and violet rays. Hence the green and purple layers between them cut off almost all light from the interior of the pitcher, which thus forms a dark chamber, into which the negatively heliotropic root is likely to be attracted.

Fungus Parasitic on a Beetle.

Professor Alfred Giard contributes to the *Bulletin Scientifique* (vol. xxiv.) a study of *Isaria densa*, a fungus parasitic on *Melolontha vulgaris*, the cockchafer. He published two years ago in the *Comptes Rendus* a notice of this fungus, and the present essay is partly an amplification of it and partly a continuation of his experiences. This research has been inspired with the hope of waging a successful war, in the interests of agriculture, on the cockchafer by wholesale infection of the larvæ with this fungus. Such methods have been tried, as everyone knows, by Continental mycologists and bacteriologists in certain cases, from the Australian rabbit downwards. In this country very little has been said or done, biologists being mindful, no doubt, of the luminous idea evolved by an eminent member of their body, and gravely proposed to his French colleagues, that they should exterminate the Phylloxera by means of the yeast fungus! No wonder if there has been a dearth of ideas on the subject in this country since then. M. Giard, however, has made a truly scientific study of the fungus and its artificial culture, and of the possibility of employing it in the manner indicated. It certainly deserves to be rewarded with a practical success. The great difficulty in the way of most cases of the kind is, of course, that which has at all times prevented the proper testing of the celebrated method of catching birds by putting salt on their tails. Every mycologist knows, however, the extraordinary powers of multiplication possessed by fungi, and the extraordinary diffusion of their spores. By assisting nature with artificial cultivations of the fungus, M. Giard has obtained a gratifying success so far as his proposal has been tested, and his method has plainly numerous advantages, as a natural and otherwise harmless one, over the usually futile one of sprinkling chemical insecticides. Such methods as M. Giard's deserve not only success but further developments, and it is to be hoped that they will not be hindered by vain talk of the dangers of "letting loose the germs of disease" generally indulged in by newspaper writers, who are willing to believe that one bacillus differs from another only in degree of rapacity in attacking the human body. To assist nature in such affairs is surely better than to squirt petroleum and other chemical (patent) insecticides over our gardens and fields. Some of these have a mischievous effect on the soil, and most have no measurable effect on the parasite; all are "quack medicines." For example, it was once proposed to cure the potato disease by the application of a substance costing about £17 per acre, as if this were the difference to the farmer between a good crop and a bad one of potatoes; moreover, the substance in question was useless for the purpose. Agriculturists should, therefore, welcome the development of all such rational proposals as this one of M. Giard, the fruit of much observation and ingenuity.

Algæ.

The second part of the *Phycological Memoirs* has just been published and amply fulfils the promise of the first number—indeed this part seems to us to be the more interesting of the two, dealing, as it does, with many of the vexed questions in the study of Algæ, which can only be settled satisfactorily in a place of such vast resource as the

British Museum. It is interesting to note that a letter confirming the results of the investigation of *Splachnidium* has been received by the authors of the new natural order, published in the last part. Mr. Laing writes from New Zealand describing the escape of the zoospores from the Splachnidian sporangia, which he has himself witnessed. The first paper in Part II. is entitled "Notes on the Morphology of the Fucaceæ" (*Coccophora* and *Seirococcus* by Miss A. L. Smith, *Notheia* by Miss Mitchell, *Xiphophora* by Miss Barton and *Sarcophycus* by Miss Whitting), and goes far to clear up points left undecided in this large and important order. It is prefaced by the editor, who explains that the authors of these notes have investigated material of genera unexamined or left uncertain by Oltmanns in his account of the Fucaceæ, and the result is in some ways confirmatory of Oltmanns' expectations. We are glad to see that the two genera *Ecklonia* and *Scaberia* are also to be examined, since the fact of finding oogonia on branched hairs in the female conceptacles of *Sarcophycus* would lead us to expect interesting results from the investigation of a genus so closely allied as *Ecklonia*. *Notheia* is also to receive more attention in a future number, since we have here an example of a degraded Fucaceous alga, if not a parasite, at least suspiciously attached to its host by a wedge of tissue like a haustorium.

A contribution by Miss Whitting on a new species of *Chlorocystis* shows careful investigation. The occurrence of parasites on Algæ is becoming a somewhat favourite field of research, and the new species here described is the only one of this genus which has hitherto been found to inflict injury on its host. It is a pity that this clever worker has not cultivated a more impersonal style of expressing herself. Possibly Mr. Bracebridge Wilson, who supplied the material to the British Museum, may be able eventually to report more completely on the life-history of this new parasite.

The editor (Mr. George Murray) has three papers—one throwing light on the much-disputed question of "fasergrübchen," or, as they are called here, cryptostomata. This, taken together with Miss Mitchell's paper on *Hydroclathrus*, places the character of these bodies in a clearer light than has hitherto been the case, and we hope Mr. Murray will not allow the matter to rest here, but will pursue the investigation still further and place the question of the real nature of cryptostomata beyond all dispute. His two remaining papers are, "On Halicystis and Valonia," and "A Comparison of the Marine Floras of the Warm Atlantic, the Indian Ocean, and the Cape of Good Hope." The former treats of *Halicystis ovalis*, Aresch.—a generic type new to Britain—and compares the morphology of this Siphoneous genus with that of *Valonia*. The remarkable reproductive organs of *Valonia*, noted in the living state some years ago by the author in the West Indies, are here described for the first time. Dr. Schmitz's remarks on the position of the genus *Halicystis* are incorporated in this paper.

In the comparison of the three Marine Floras, Mr. Murray is dealing with a subject on which he has already published several papers; and this one, embodying the results of a large amount of work, is a valuable contribution to a subject which has been very little studied.

The illustrations to this part of the *Phycological Memoirs* are equal to the last, and in both cases excellent.

We look forward to welcoming before very long the third part of this interesting book.

Popular Geology.

A slander action of some interest has recently been tried in the Court of Session in Edinburgh. A County Council lecturer, Mr. Alexander Johnstone, has been explaining the geology of soils to the "blameless Hyperboreans," and in the course of this, according to Sir Arthur Grant of Monymusk (defendant), he said certain wild and whirling things about the mistake of Providence in not putting more phosphates into the soil of Aberdeen, and made certain criticisms of the Biblical account of the Creation. The plaintiff denied having said such things, though he did say in reply to a question that he was not there to defend the first chapter of Genesis, and possibly he remarked that the account he would give of the creation of the world as proved by science might be more interesting than that given in the chapter referred to. Sir Arthur Grant had anyhow taken the occasion to reprove clergymen for quarrelling among themselves instead of uniting against men like plaintiff, and the Court seems to have agreed with him, and given him the verdict. We should greatly like to hear Mr. Johnstone's, or anyone else's account of the creation of the world "as proved by science." Sir Arthur Grant might then be shown as good sport by scientific men as any of the churchmen now militant in the north could exhibit. Such a match (Dr. Kinns on "Moses and Geology" barred, and Sir Henry Howorth, F.R.S., closured on the "Mammoth and the Flood") may be commended to the organising committees of sections in search of an exciting discussion for the British Association. It would be as seemly as a recent discussion on the Hygiene of Stays. So long as public lecturers regard the book of Genesis as the lamented Mr. Dick regarded King Charles I.'s head, they may expect a return innings, and it is to be deplored in any case that such matters should find their way into the Courts of Justice.

A correspondent sends us a newspaper-cutting containing a condensed report of this trial, with the remark that "if such bigotry and ignorance of geology prevails in Scotland, it is time some geological missionaries should enlighten the people." Our correspondent forgets that the countrymen of Hutton, Lyell, Murchison, the Geikies and others have already sent missionaries south in this matter, and they might do so again!

Dr. Herdman contributes to the *Conchologist* for June 24 an interesting confirmatory note as to the views of Giard with respect to the mimicry of the mollusc, *Lamellaria perspicua*. "The Mollusc," he says, "was on a colony of *Leptoclinum maculatum*, in which it had eaten a large hole. It lay in this cavity so as to be flush with the general surface; and its dorsal integument was not only whitish with small darker marks which exactly reproduced the appearance of the *Leptoclinum* surface with the ascidiozooids scattered over it, but there were also two large elliptical clear marks which looked like the large common cloacal apertures of the Ascidian colony." Professor Herdman did not notice the *Lamellaria* until he had by accident partly dislodged it in pricking the *Leptoclinum* from the stone. He points out that in this case we have the curious result that the *Leptoclinum*, while itself protected by sharp spicules against certain enemies, is thus armed to some extent for the benefit of the *Lamellaria* which preys upon its vitals.

We have received from Messrs. Hedley and Suter a copy of their "Reference List of the Land and Fresh-water Mollusca of New Zealand" (*Proc. Linn. Soc. N. S. Wales*, ser. 2, vol. vii., 1893, pp. 613–665), which enumerates no less than 184 species, as against the 15 first recorded by Gray in 1843. This jubilee list is therefore interesting in that it affords an index of the progress made in the study of an important section of Antipodean Malacology within half-a-century of its inception. The authors maintain that in proportion as the New Zealand fauna becomes known does its insularity stand out, and they enumerate no less than fourteen well-known molluscan genera which have been erroneously imposed upon it, though whether, as they say, *Helix* be one of them, very much depends upon whose classification of a very difficult group be followed.

Those interested in the microscopic details of the structure and contents of the plant-cell will find matter to their taste in the "Beiträge zur Morphologie u. Physiologie der Pflanzenzelle," edited by Dr. Zimmermann, of which the first volume has recently appeared. It includes, in 322 pages, fifteen separate communications, for twelve of which the editor is himself responsible. Among the items discussed are the internal structure of leucoplasts, which are not always homogeneous as hitherto supposed, but sometimes contain well-defined spherical bodies or *leucosomen*. Another body so far unrecorded, the *granula*, is found to be of widespread occurrence in the assimilating tissue of plants. The proteid-crystalloids, their detection, distribution, and properties, are dealt with in two of the longest papers, while the chromatophores of variegated and chlorotic leaves respectively form the subject of other two. As regards the last mentioned, the author demonstrates the presence of small, colourless corpuscles in the leaves of plants which have had no iron salts in their food; on supplying an iron tincture the corpuscles grow and become green. There is also a short account of the properties, reactions, and distribution of the oil-plastid. Two papers refer to the growth of the cell-wall, and one, by C. Correns, is concerned with the internal structure of the cell-membrane of some Algæ. The last-named author also supplies an account of some points in the structure and life-history of a green fresh-water alga, *Apiocystis Brauniana*, Naeg. The volume is well illustrated with five coloured plates and 23 figures in the text.

In the *Journal of Botany* for July, Mr. Rendle has a note on a potato which is depicted bringing forth young potatoes from its interior through clefts in the skin. The occurrence which, though previously described, has never been satisfactorily explained, is now said to be due to the production of shoots from the base of the bud or "eyes" as a result of the continued removal of the normal shoots. The tubers were stored in a cellar as usual, and the sprouts which appeared in the spring were plucked. In the case described the potato, perhaps an exceptionally vigorous one, devised a new means for using the large quantity of soluble nutrition which was being produced to supply the growing shoots, and as it was not allowed to form aërial shoots, developed from the base of the bud where the chemical processes would be specially active, tubercle-bearing shoots penetrating the substance of the mother tuber, and providing themselves with roots and a corky protecting covering in the same way as if they were pushing through the soil.

The writer refers to a similar physiological occurrence in the Mexican *Agave*. The Indians cut off the large spike-like terminal inflorescence at an early stage and then collect the sugary liquid which the plant continues to pour into the scooped-out top of the axis till it is exhausted, withers, and dies down. In both cases the chemical processes are suddenly rendered useless by removal of the organ for which the soluble nutrition was required; in both these processes continue, in the *Agave* for the gratification of the natives, whereas in the potato in question a new means for its use is devised. Of course, as the young tubers grow the mother shrivels proportionately, having to nourish its progeny from its own tissues.

We regret to observe that the editor of the *Journal of Botany* has taken to heart, as a serious criticism, a playful, but complimentary note on the abundance of cryptogamic papers in his pages, which appeared in Natural Science for June. It has, however, given him the opportunity of boasting the proud traditions of the *Journal* in the matter of fair play—a thing well-known to all botanists, but not made more savoury by an editorial announcement.

In a supplement to the *Leeward Islands Gazette*, Mr. C. A. Barber reports on the failure of the cacao crop in Dominica in the season of 1892–93. The failure is apparently not the result of any specific disease, but partly due to a reaction after a previous very successful season, and partly to a slovenliness in cultivation which is general in the island, and tends to induce an unhealthy tone in the trees, whereby the effect of climatic or unfavourable local influences is exaggerated.

Mr. H. O. Forbes' memoir on "The Chatham Islands: their Relation to a former Southern Continent," appeared last month, not in the Geographical Society's *Journal*, but in the *Supplementary Papers* of the same Society.

We understand that a shaft is to be sunk at the Dover Boring, at an estimated cost of £50,000. It was chiefly owing to the fact that the work was entirely under the control of Mr. Brady and Sir Edward Watkin that the boring was ever completed; it will be interesting now to see whether, under a board of directors, so much will be accomplished with so steady an end in view as was the work in which only two participated. Professor Boyd Dawkins is, we believe, one of the board.

We are glad to observe that our reference to the neglected state of the geological collection in the Museum of the University of Malta has attracted some attention in that benighted island. We may assure those interested that our information was not obtained from a resident, but from a geologist who is acquainted with most of the University collections of Europe, and who tells us that that of Malta was, five months ago, in a more disgraceful condition than any other he had visited. A correspondent, who states that he writes "officially," but at the same time requests that his name may not be divulged to anyone, points out that Mr. John H. Cooke is teacher of Mathematics and English in the Lyceum, not lecturer in the University.

The Geological Committee of Russia has just marked the completion of its first ten years' work by the issue of a geological map of the empire, produced chiefly under the direction of Drs. Karpinsky, Nikitin, Tschernyschew, Sokolow, and Michalski. The map bears 45 district notations, partly in colour, partly in signs, and the scale is about 40 miles to the inch. Much of the country is monotonous, being covered with post-Tertiary deposits, but the more striking features—such as the limit of extension of erratic blocks and the transgression of the old marine deposits in the Caspian region—are all duly indicated.

The Geological Survey of Queensland has recently issued a report on "Geological Observations in British New Guinea in 1891," by A. Gibb Maitland, with three maps and two plates of sections. The formations now known are, besides coral reefs and surface deposits, the Post-Tertiary Kevori Grits, the supposed Tertiary Port Moresby Beds, and the Boioro Limestones of undetermined age, with metamorphic and igneous rocks. The coral rocks present no features of special interest. The Boioro Limestones are doubtfully placed beneath the Port Moresby Beds, and Mr. Maitland discovered no fossils; they sometimes contain flint nodules. Useful details are given, and the report concludes with a bibliography.

Palæontologists will be glad to learn that Professor K. A. von Zittel has now completed his *Handbuch der Palæontologie*, and the last two parts will shortly be issued. His general account of the distribution of the extinct mammalia has just been published in advance in the *Sitzungsberichte* of the Bavarian Academy of Sciences, and a translation will shortly appear in the *Geological Magazine*. So valuable a comparison of the extinct mammalian faunas of different parts of the globe cannot fail to arouse a considerable amount of interest among those who have specially studied the subject of distribution; and the general survey now before us is one that has long been urgently required.

Commencing with the view that during the Secondary period the entire globe was inhabited by Multituberculates and Polyprotodont Marsupials, Dr. von Zittel considers that early in the Tertiary three great centres of mammalian development and dispersion were established. The centres in question were (1) Australia, (2) South America, and (3) Europe, Asia, Africa, and North America. Commenting on the alleged connection of South America, South Africa, and Australia by means of an Antarctic continent, he urges that had such connection taken place during the Tertiary period we ought surely to have found South American types of Edentates and Perissodactyles in Australia; and it must be confessed that those who urge the doctrine in question will have much difficulty in attempting to disprove the contention. At the same time, the evidence of the fossil Marsupials of Argentina shows that at some time there must have been a connection between the Australian and Neotropical faunas.

Special attention is directed in the memoir to the extinct faunas of Argentina and Patagonia, in the course of which it is shown that if the earliest of these be correctly assigned to the Eocene period, its mammals had attained a degree of specialisation far and away ahead of their North American and Old World contemporaries. All the

Ungulates, for instance, had attained tall-crowned or hypsodont molar teeth, and the same was the case with the Rodents, which had likewise discarded milk-teeth. Indeed, *Homalodontotherium* (we trust we have spelt the name correctly) is the only form which can be regarded as a generalised one; most of the Ungulates having the digits reduced to three, and the carpus of the interlocking type. The list of this earliest Santa Cruz fauna, as it is termed, includes only Marsupials, Edentates, Toxodonts, Typotherioids, certain aberrant and peculiar types of Perissodactyles, Hystricomorphous Rodents, and Primates allied to those now inhabiting South America; and the same holds good for the overlying Patagonian fauna, which has been correlated with the Miocene. All the genera found in these formations may accordingly be regarded as having originated in South America. When, however, we reach the still higher beds of Monte Hermoso and the Pampas, which have been identified with the Pliocene, we meet for the first time with Tapirs, Horses, Llamas, Deer, Mastodons, Murine Rodents, and true Carnivores, which are evidently immigrants from North America, where they occur in strata devoid of the remains of any of the above-named exclusively Neotropical forms. If further confirmation be required that the connection between the two halves of the continent did not take place before the Pliocene, it is forthcoming by the evidence of an incursion of South American Edentates into North America during that epoch. It may be added that the occurrence of monkeys belonging to the Cebidæ in the earlier Argentine Tertiaries affords some confirmation of the view that the monkeys of the Old and New Worlds have had an independent origin.

Palæontologists in the Argentine Republic have little opportunity for comparing their fossils with similar remains discovered elsewhere, and certain unfortunate rivalries have led to much confusion in the nomenclature even of the best-known genera and species. We are thus glad to announce that Mr. R. Lydekker, under the auspices of the Royal Society of London, leaves England on August 24, to examine the fossil vertebrata in the museums of Buenos Ayres and La Plata. He goes in response to the special invitation of Dr. F. P. Moreno, founder and director of the great museum in the new city of La Plata; and with his great experience of the Old World fossil vertebrata, he ought to render much service to Zoology by personally revising the work of his colleagues in the remote republic.

I.

Rainfall and the Forms of Leaves.

THE following notes are taken from an admirable paper by E. Stahl in the *Annales du Jardin Botanique de Buitenzorg*, vol. xi., pp. 98–182, on leaf-forms in relation to rainfall, chiefly based on observations of tropical plants at Buitenzorg. On arriving in Java (November, 1889), he was impressed with the great humidity of the atmosphere as well as with the extraordinary rainfall, and he was led to ask how the plants met such conditions. In the tropical forests they are not only subjected to daily thunder showers during the rainy season, but the sun's rays scarcely penetrate through the thick foliage, and the air is saturated with moisture which causes a perpetual drip from the leaves. An examination of desert forms, with their arrangements for economising their scanty water-supply, has thrown so much light on the peculiarities of certain plants of our temperate climates, that plant adaptations to opposite conditions on an equally large scale promised a rich and suggestive field of study.

Leaves, as we know, differ greatly in their behaviour towards atmospheric water ; from waxy leaves the drops roll off, leaving no perceptible moisture behind, while other leaves, without this provision, tend to retain the moisture on the leaf-surface, and are weighed down and often broken off, in the tropics, by the added pressure on the brittle leaf stalk. The retention of moisture on the leaf after the rain is over interferes too with the transpiration, with which the rise of water in the stem with the supply of mineral salts to the growing and assimilating organs is connected. This function is provided for, and the difficulty so far met, by the great leaf-surface, but there are also various methods by which plants with easily moistened leaves dispose as speedily as may be of their superabundant water-supply. One of these methods is the adoption of the sleeping position by leaves, such as those of the sensitive plant, that possess this power of movement. Jungner,[1] a previous observer, says : "As soon as rain falls the horizontal leaves bend upwards as in the sleeping condition, and the rain-drops easily run off by the base of the leaf."

The most frequent way of disposing of the excessive moisture, however, is by draining it off by means of long points to the leaves.

[1] "Anpassungen der Pflanzen an das Klima in den Gegenden der regnerischen Kamerungebirge." *Botanisches Centralblatt*, 1891, p. 358.

These Stahl calls "*Träufelspitzen*," or dropping-points, and the rapidity with which tropical plants shed the rain-drops by this means is very astonishing. In a comparatively moist atmosphere, after a heavy rain, such plants as *Ficus acuminata*, *Angiopteris evecta*, and many others possessing *Träufelspitzen* had been long dry, while European or Australian forms with leaves ending in blunt tips, as the Oak, Apple, *Eucalyptus*, etc., yielded on shaking them a heavy shower of drops.

It is instructive to glance over any monograph of tropical genera and note the frequent use of the terms, "*folia longe acuminata*," "*folia acuminatissima*," "*folia caudata acuminata*." These *Träufelspitzen* occur also on the lobes of divided leaves, and on the pinnæ of compound leaves, but they are, perhaps, most remarkable on entire ovate leaves, such as those of *Ficus religiosa*, where the midrib grows out into a linear point sometimes 75 mm. long, the leaf itself measuring about 100 mm. from the stalk to the base of the point. In some plants the prolonged midrib of the leaf is a fairly wide channel, but the most ordinary form is that of a tapering narrow point slightly bent back and curved at the end, a peculiarity which is found in many European species. As the water trickles down the inclined narrow points, it passes from the upper to the under surface before dropping from the leaf, and the bent tip accelerates this action very distinctly.

Stahl tried several experiments to test this theory as to the advantage of *Träufelspitzen* to the plant. He took six leaves of *Justicia picta*, one of the Acanthaceæ, with leaf points a centimetre long, and slightly bent at the tip. From three of these the points were cut, and the ends carefully rounded; they were then fixed on a board, which was placed at an angle of 30 degrees, and sprinkled with water. The rounded leaves he found retained some of the moisture for an hour, the others were quite dry in 20 minutes or less. The same results were obtained with other similar leaves.

The veins of the leaf play a very important part in the drainage of water from the surface, especially when they are deep-seated, or, as in the Melastomacean type, where the parallel veins bend in towards the apex. In plants the leaves of which slant towards the stem, the water flows in a centripetal direction, and there we find such contrivances as the rows of hairs in the Bird's Eye (*Veronica chamædrys*), which act by capillarity like blotting paper, and draw away the water from the leaf-surface. Many plants with similar rows of hairs have also deep nerves and channelled stems, as the Blind Nettle (*Lamium album*), Hop, etc. Kerner, in his *Pflanzenleben*, vol. i., p. 85, discusses the importance to the plant of the direction in which the water is conveyed down by the leaves. He considers that it is directly related to the position of the roots, and that the object is to supply moisture to the young growing rootlets. But this seems hardly necessary in the case of tropical plants. It seems more probable that the advantage gained by the leaf is, as already noted, the lightening of the weight, the aid to transpiration, and, lastly, the continual and thorough cleansing of the surface from insects, fungi, etc. We can verify this on our home plants. After a shower, the pointed leaves of ash, willow, etc., have had the dust quite washed off, while rounded leaves like those of the oak are still dirty. This cleansing of the leaves is of immense importance to tropical plants, as all will allow who have seen how they may become overgrown with hepatics, algæ, mosses, and lichens. These epiphytes, which have many contrivances to enable them to germinate and grow on the leaf, seriously interfere with its function, and we are justified in looking for arrangements by which the leaf will make it difficult for the epiphytes to get a footing. In the end they are generally successful, however, and the leaves with longest lives have the heaviest load to bear.

Plants with shining satin-like leaves, often tinged on the underside with red, form a great group much cultivated for the sake of the beautiful foliage. *Anthurium crystallinum*, *Philodendron Lindeni*, with begonias and many others, belong to this group. The peculiar brilliancy of the leaves is due to papillose outgrowths of the epidermal cells. These exert a capillary attraction on water, and the drops left after a rainfall are thus dispersed by the papillæ into a thin sheath which evaporates quickly and secures the rapid drying up of the leaf. This capillarity was tested by dipping the tip of a leaf of *Anthurium* in water, when the moisture was drawn up on the leaf 140 millimetres.

The long points to the leaves are certainly, in countries like West Java with a heavy rainfall, the chief means by which the plant disperses the excessive water-supply. In China and Japan, where the same conditions of humidity and great heat occur, they are a striking feature of the vegetation. An interesting exception is found in plants native to the drier regions along the shore or up on the mountains which have rounded or crenate leaves. In our own woods, while we occasionally find *Träufelspitzen* on the smaller herbs, as *Melampyrum sylvaticum* which has long tapering pointed leaves, they are very frequent on plants of a larger growth, especially on trees and shrubs that grow by preference in shady woods or along the banks of rivers, such as the ash and the willow; those that seek a drier soil, different kinds of *Prunus*, roses, etc., have no such provision. The same difference in structure appears in a more pronounced form when we compare the vegetation of different climates. In the Eastern United States, where great heat alternates with heavy thunder showers, there is a great abundance of tropical forms, much more so than in Central Europe where the summer is drier and colder; but even within the limits of the same genus, the American species have distinctly longer pointed leaves than their European relatives. In Europe, Stahl says, he noticed that longer points were developed on leaves in moist regions than where the ground and atmosphere were drier.

In further support of these observations, Stahl made a careful examination of the leaf-forms of one or two very widely distributed genera. Quoting from Pax's monograph on the genus *Acer*, he presents to us first the species belonging to Europe: *Acer platanoides*, which in a wild state inhabits damp localities, and the Sycamore (*A. pseudo-platanus*) have pointed leaves; the Maple (*A. campestre*) grows on the plains and uplands of Central and Southern Europe, and the lobes of its leaves are blunt, as are those of *A. monspessulanum*, also from Southern Europe. In the United States and in Japan, the different species have leaves with long pointed lobes, and this feature is still more marked in the Himalayan varieties. *Acer integrifolia*, from Southern China and Japan, has a simple, entire leaf which ends in a long point. Though he does not (without other evidence) refer plants without *Träufelspitzen* to a dry climate, yet he concludes that when they possess this feature so strongly marked as in *Ficus religiosa* and others, they must grow in a country with a heavy rainfall. Stahl thinks also that this peculiarity, which is so entirely one of adaptation to climate, might throw some light on the conditions of growth of fossil plants.

In plants wholly confined to the tropics, many young leaves assume a hanging position on unfolding, and remain vertical for some days after attaining their full size. This is undoubtedly a protection from the heavy rain, and saves them from the devastation that overtakes so much of the vegetation in these destructive showers. When the leaves are stronger they become horizontal, though some very large leaves, as those of *Anthurium Veitchii*, remain vertical throughout their life. It would be interesting and useful to be able to calculate the exact force of the rain-drop as it strikes the leaf. This has not been worked out, but that the blow is heavy anyone knows who has been caught in one of these showers, or who has heard the noise made by the rain falling on palm leaves. Kny, in the *Berichte der deutschen botanischen Gesellschaft*, 1885, has directed attention to the danger incurred by leaves from rain and hail, and to the leaf-tissues and leaf-forms specially adapted to meet this emergency. In simple undivided leaves we find either great elasticity and bending power, or a strong leathery structure. In other leaves we have division into laminæ; either, as in many palm leaves, this takes place regularly by the splitting of a layer of cells as the leaf unfolds, or it is effected later by a tearing apart of the tissue. The division reaches its highest development when the laminæ are quite separate leaflets, as in most ferns and in many dicotyledonous plants.

The adaptation of leaf-form to function and environment is nowhere more strikingly seen than in those plants that at different times produce different kinds of leaves. The first leaf of the stately fern *Platycerium alcicorne* is unstalked and kidney-shaped, the mantle-leaf of Goebel,[2] and lies flat on the ground, thus protecting the young

[2] "Morphologische Studien.—I. Ueber epiphytische Farne u. Muscineen." *Annales du Jardin Botanique de Buitenzorg*, 1887.

roots from being dried up. The later leaves are erect and much divided. We find the same contrivance in one of the aroids, *Pothos aurea*, the stem of which is at first creeping, and bears flat sessile undivided leaves to protect the roots; when the stem ascends the leaves become stalked and pinnate. There are numerous examples of heterophylly among our European herbs; in many of the Compositæ, Cruciferæ, etc., the upright stalk bears linear, pinnate, or lobed leaves, while those forming the rosette of radical leaves are larger, and lie flat on the ground. Still another interesting point in leaf-adaptation is found in the nervature; either the nerves are parallel as in most monocotyledons, or they anastomose to form a network as in most dicotyledons. In the former case the leaf is simple, and of much stronger tissue; the complicated net-veining is found in wide-spreading leaves, and serves as a protection against splitting. In the Fern group the simpler type of parallel nervature prevails rather than the other, but during the Mesozoic age net-veined fern leaves were very abundant up to the close of the Chalk period. Then dicotyledons with leaves of a similar structure began to appear, and seem gradually to have taken their place.

Stahl closes the paper by a short examination of the leaf-tissues as they are affected by environment. In the long, slanting or bending leaves of monocotyledons the upright palisade cells of the assimilating tissue are replaced by cells the longest axis of which is parallel to the leaf-surface. By the massing of the strengthening elements towards the centre of the tissue, between the upper and under surface, the elasticity of the leaf is secured, and there is further provision against bending strains in those leaves with parallel veins, each of them being surrounded by strands of strong fibres.

ANNIE LORRAIN SMITH.

II.

On the Zoo-Geographical Areas of the World, illustrating the Distribution of Birds.

I HAVE been asked to tabulate the different regions, sub-regions, etc., of the world, of which I spoke, and which I illustrated in my recent course of lectures on the "Geographical Distribution of Birds" at the Royal Institution. I do this with some hesitation, because, as I explained in these lectures, our want of zoological statistics for vast tracts of the Old World makes it almost impossible to give exact definitions of the natural areas of any of the Palæogœan divisions of the globe.

With America it is different, as has been shown during the last few years by the admirable work of Dr. C. Hart Merriam and Mr. J. A. Allen on the distribution of North American Mammals, by means of which these authors have presented us with excellent maps defining the natural regions of the North American fauna. Mr. Allen has also recently published ("Auk," 1893, pp. 97–150 pls. iii., iv.) a separate paper on the zoo-geographical divisions of North America based upon the class Aves, and in my lectures I have followed the main conclusions of this paper, as I think it is better for ornithologists to adopt faunal regions based upon the distribution of *birds alone*, when we have some data to go upon. Until Mr. Allen published his avi-geographical maps, I was only too content to work by means of the maps furnished by Dr. Hart Merriam and himself from a study of the mammalia of North America, but I believe that we shall ultimately arrive at sounder conclusions with regard to the zoo-geographical divisions of the earth's surface, if each section of zoologists works out the statistics of his own branch, independently of those of other zoologists, so that at some future date the results can be correlated. This I can hardly expect to see accomplished in my day, but I hope that the present paper may be found to be a small contribution to this desirable result.

The American naturalists may well be envied for, and congratulated by their European colleagues on, the success which has resulted from their patient collection of materials. This has extended over many years, and now leaves them in the proud position of having better statistics to work upon than are possessed by the ornithologists of any other portion of the globe, with the exception, perhaps, of our own islands.

While, therefore, many of my conclusions with regard to the extent of the provinces of the Old World must be purely conjectural from want of definite knowledge, there are certain sub-regions which may be suggested, and their boundaries defined, with some confidence.

Some of Mr. Allen's conclusions ("Auk," 1893, pp. 97–150) with regard to the main divisions of the Old World are the same as those of Dr. Reichenow, and I think that they are, in both instances, too sweeping; but the recognition and definition of an Arctic Zone or "Realm," as Mr. Allen calls it, is a fact which must henceforward be admitted by all ornithologists. Its southern boundary is probably along the isothermal line of 50° (mean summer temperature), and may be coincident with the northern limit of conifers.

Mr. Allen's "North Temperate Realm" includes both hemispheres to the northern border of the palm belt. Its southern line corresponds with the isotherm of 77° mean annual temperature, but this constitutes a very roughly defined southern border for the zoological areas of the Old World.

The southern border of palm-growth follows the southern isothermal line of 77° (mean annual temperature) but by no means defines natural borders for Mr. Allen's Indo-African Realm or Dr. Reichenow's Æthiopian and Malayan Regions, and it is better, as it seems to me, to stick to our old terms at present, instead of defining the limits of the Old World regions and sub-regions, with their ill-tabulated data, by analogy with the New World sub-regions, the boundaries of which are so much better known. It is for this reason also that I have not adopted Mr. Allen's nomenclature in its entirety. I may in due time be brought to speak of "Realms," but the same conservatism which prevents my adopting the trinomial nomenclature of the American zoologists of the present day, will prevent my discarding some of the old-fashioned, and, to me, expressive zoo-geographical terms. I cannot understand why the word "Nearctic" should be discarded. It was given by Dr. Sclater not in the sense of "arctic" but "northern" region of the New World, and is, in my opinion, apart from the priority which commands respect for its retention, a most simple and expressive term. My American colleagues will understand that if I have not carried their system of nomenclature into the zoo-geographical regions of the Old World, it is not from any want of respect to their work, for I heartily agree with their conclusions as regards North America; but a little time must elapse before we can confidently apply the same reasoning to the avifauna of the Old World. When Dr. Pleske and Professor Menzbier have finished their works on the birds of the Russian Empire, so that the confusion caused by the "lumping" tendencies of the Middendorffian school, so justly deplored by Dr. Stejneger, shall have been dispelled; when France, and Spain, and

Turkey, and Greece shall have been ornithologically explored and the results described in the complete American fashion; when the first collection of birds arrives from Timbuktu; when Arabia gives up its ornithological secrets; and when the parts of Siberia and Asia, of which nothing is yet known, have been explored; to say nothing of the high mountain interiors of many of the Malayan islands; then—and scarcely till then—shall we be able to generalise with some chance of success.

This deficiency in our information, however, need not prevent us from hazarding an idea—as has, indeed, often been done before now—of some of the avi-geographical regions of the Old World, and I place before my readers the divisions which I introduced into my Royal Institution lectures, leaving it for future statistics to confirm or destroy my conclusions. I begin, therefore, with the New World, adopting, for the purposes of my lectures, the old divisions of the Nearctic and Neotropical Regions:—

A.—Nearctic Region (Northern Region of the New World).

I. Arctic Sub-Region.
II. Alaskan Arctic Sub-Region.
III. Aleutian Sub-Region.
IV. Cold Temperate Sub-Region.
V. Warm Temperate Sub-Region.
  1. Humid Province.
    α. Appallachian Sub-Province.
    β. Austroriparian Sub-Province.
  2. Arid Province.
    γ. Campestrian Sub-Province.
    δ. Sonoran Sub-Province.

B.—Neotropical Region (Southern Region of the New World).

I. Antillean Sub-Region, including the Greater Antilles and Southern Florida, as also the Lesser Antilles, with the exception of Trinidad, Tobago, and the islands off the coast of Venezuela.

II. Central American Sub-Region.
  1. Mexican Province.
  2. Isthmian Province.

Until my friends, Messrs. Salvin and Godman, have finished their monumental "Biologia," it will be impossible to define accurately the boundaries of the several provinces, especially as fresh facts are being brought to light every day. A division may be hazarded on the lines of Mr. Salvin's conclusions published in the *Proceedings* of the Zoological Society for 1867 (pp. 131, 2) and 1870 (pp. 177, 8). Thus we shall probably get two provinces, which may be called:—

1. The *Mexican* Province, including Central America from Costa Rica northwards to Mexico, leaving out the Central Plateau,

which forms part of Mr. Allen's Sonoran Sub-Province. On the northern boundaries of the Mexican Province, Mr. Salvin's remarks ("Ibis," 1889, p. 242) must be consulted. On the eastern side it seems to follow the forests as they cling to the mountains almost to the Rio Grande, and the presence of Tinamous, Motmots, and Toucans mark the limit of the Neotropical Region. On the northern side the limits of the Mexican fauna are somewhat more extended and reach to about Alamos in Sonora.

2. The *Isthmian* Province, extending from Costa Rica to Panama.

For the rest of the Neotropical Region, I find that nothing can be proposed in modification of the four sub-regions proposed by Mr. Salvin, and published by Professor Newton in his article on Birds, in the "Encyclopædia Britannica." They are delineated in the accompanying map.

III. The Patagonian Sub-Region. This includes the southern portion of South America up to Bahia Blanca on the east coast, "thence in a north-easterly direction, passing to the eastward of Mendoza, and then northward along the eastern and higher slopes of the Andes, until it crosses the Equator, and after trifurcating on either side of the valleys of the Magdalena and its confluent the Cauca, returns along the western slopes of the lofty Cordillera, until it trends seaward and reaches the Pacific Coast of South America somewhere about Truxillo, in lat. 7° S." (*Cf.* Newton, *t.c.*, p. 744.) Mr. Salvin tells me that he considers that, besides the trifurcation of this region in Colombia above referred to, there are outlying portions of the same sub-region in the mountains of Santa Marta and Merida.

IV. The Brazilian Sub-Region. This "marches with the foregoing until somewhere near Potosi in Bolivia, whence it turns to the north-east, and, avoiding the water-shed of the Amazons, strikes, perhaps, the Paranahyba, through or along which it makes its way to the Atlantic." (*Cf.* Newton, *t.c.*, p. 744.)

V. The Amazonian Sub-Region limited to the southward by the Brazilian boundary, the western frontier of the Amazonian sub-region seems to turn off before the eastern confines of the Patagonian sub-region are reached, and, leaving a space intervening, it pursues a generally northward course, at a lower level, on the western bank of the Huallaga, and crossing the great stream, whence it derives its name, in somewhere about long. 77° W., lat. 5° S., it pursues its way towards the mouth of the Orinoco." (*Cf.* Newton, *t.c.*, p. 744.)

VI. The Sub-Andean Sub-Region. "This begins in the south with a narrow strip of land, before mentioned as intervening between the comparatively low-lying Amazonian sub-region and that portion of the Patagonian which runs along the lofty Peruvian Andes, and is believed to extend from the frontiers of Bolivia to the table-land of Ecuador, rounding, on the one hand, the forked extremity of the Patagonian sub-region to the westwards until it meets the Pacific at Truxillo, stretching over five hundred miles of sea to the Galapagos Islands, following the Amazonian boundary to the Atlantic, while it comprehends the islands of Trinidad and Tobago, as well as those which lie on the northern coast of South America." (*Cf.* Newton, *t.c.*, p. 744.)

C.—The Palæarctic Region.

This may be divided roughly into three sub-regions, besides the

Arctic Zone,

which corresponds with the same zone in the New World, and becomes a circumpolar province. The greater part of the northern Palæarctic Region may be denominated the

I. Eurasian Sub-Region,

while the southern portion forms another sub-region, which may be called the

II. Mediterraneo-Asiatic Sub-Region ;

the one answering to Mr. Allen's "Cold Temperate Sub-Region," and the other to his "Warm Temperate Sub-Region."

A very good idea of the limits of the breeding areas of the Western European birds has lately been given by Mr. Henry Seebohm in his most useful introduction to his "Geographical Distribution of British Birds," and on discussing with him the natural geographical divisions of the European Avifauna, he suggests that the Scandinavian mountains must form part of the Arctic Zone which should extend as far south as the Doorefjeld, and this would correspond with the breeding range of several Arctic species, such as the Snowy Owl, &c. The Yenesei Valley, as Mr. Seebohm has before pointed out, is the natural boundary between the Eastern and Western Palæarctic birds, the Urals being by no means a strong natural boundary. Mr. Seebohm deprecates the division of the Eurasian Sub-Region into provinces, and I admit that, with the data at present in our hands, it is difficult to define the limits of natural provinces, but I expect that we shall ultimately have to recognise a Central Siberian, as well as an Eastern Siberian province. The former will be the breeding home of such representative forms as *Motacilla beema* instead of *M. flava*, *Corone sharpii* instead of *C. cornix*, *Sturnus menzbieri* instead of *S. vulgaris*, etc., while the Eastern Siberian Province will probably extend from the water-shed of the Lena to Kamtschatka. It will contain the breeding home of *Eurinorhynchus pygmæus*. Dr. Stejneger advocates the recognition of a distinct Kamtschatkan province. On this and other points connected with the distribution of Western Palæarctic birds, his admirable paper must be consulted (*Bull. U.S. Nat. Mus.*, no. 28). As in the case of the Palæarctic Region itself, the provinces will not be found to contain different families or genera, but will be characterised by representative species alone.

The following will, therefore, probably be found to be the natural areas of the Palæarctic Region :—

I. The Eurasian Sub-Region—
1. European Province—east to the river Ob.
2. Central Siberian Province. Between the Ob and the Lena.
3. West Siberian Province. From the Lena to the Pacific.

II. The Mediterraneo-Asiatic Sub-Region—
1. Mediterranean Province.
2. Mediterraneo-Persic Province.
3. Mongolian Province.

III. Mantchurian Sub-Region.

There is also another natural sub-region, which I propose to call:—

IV. The Himalo-Caucasian Sub-Region.

It consists of the highest Himalayas from 8,000–10,000 feet (ex. *Tetraogallus*), the mountains of Tibet and the Altai to the north, and westwards through the mountains of Persia to the Caucasus and the hill-country of Asia Minor.

The Mediterraneo-Asiatic Sub-Region has at least three distinct provinces with characteristic forms, as was shown by Mr. H. J. Elwes in his celebrated essay (*Q.Z.S.*, 1867, pp. 645–682):—

1. The Mediterranean Province.
2. The Mediterraneo-Persic Province.
3. The Mongolian Province.

The Mantchurian Sub-Region reaching from Eastern Siberia, south of the Stanovoi Mountains, west to Lake Baikal and south to the Yangtze basin, appears to me to be established by statistics. It contains a strong Indian element, with such species as *Eurystomus calonyx*, *Halcyon pileata*, *Hirundo tytleri*, *Pericrocotus*, *Suthora*, etc., and is further characterised by several eastern representative species of well-known western forms, such as *Trypanocorax pastinator* for *T. frugilegus*, *Colæus dauricus* for *C. monedula*, etc.

D.—The Ethiopian Region.

Of the Ethiopian Region it is only necessary to remark that I have greatly modified my views since I proposed zoo-geographical sub-regions of Africa in 1870. The great desert of the Sahara still remains a blank to us and may be called

I.—The Saharan Sub-Region,

but I have come to the conclusion that a second large natural sub-region can be admitted which I call

II.—The Sudanese Sub-Region.

The number of species which are common to Senegambia and to Abyssinia and Kordofan, proclaims that there must be a natural zoo-geographical sub-region stretching across between these two distant areas. Its northern and southern boundaries cannot yet be defined, but it is a somewhat important fact that a truly Sudanese form like *Melierax polyzonus* has been found in Mogador and Southern Morocco. The Delta of Egypt must be included in the Mediterranean Region, and I am not able to define the exact boundary of my Sudanese Sub-Region in the north-east. The occurrence of a *Centropus* in the Delta shows the presence of an African element even in Lower Egypt, but the regional boundary may be about the second Cataract, above which no Sun-birds (Nectariniidæ) are found.

III.—The West African Sub-Region.

This might well be called the "Afro-Malayan" Sub-Region, as it possesses species of *Turdinus*, a *Pitta*, some Diceidæ, and other curious forms which are Malayan in their affinities. It consists of the forest districts of Southern Senegambia, and embraces the whole of the West African forest region from the above-mentioned point to the Kwanza River, its continuity being broken at Accra, where the Sudanese Sub-Region breaks through to the coast with its attendant Bustards (*Otides*) and Hemipodes, and this will probably be found to be the case at other intervals of the coast-line. The forest region may also extend a little way across the Kwanza into the interior of Benguela. Certainly it embraces the whole of the Congo basin as far as the western water-shed of the Nile, as was shown by Bohndorff's collection from the Niam Niam country and by many species obtained by Heuglin and Emin Pasha in the Equatorial Province.

IV.—The Abyssinian Sub-Region, consisting of Abyssinia and Southern Arabia.

This not only embraces all the area which I assigned to it in 1870, but must be carried north of the Zambesi, recent explorations having shown that I was mistaken in believing the latter river to be a natural boundary. It may even be found to stretch, in a broken and disjointed manner, far into Equatorial Africa, as proved by the discovery of *Chera progne* and *Pyromelana taha* by Mr. Jackson in Equatorial Africa.

V.—The East African Region.

This is not a very natural sub-region, and may have to be sunk in one of the others; but at present such a number of forms seem to be peculiar to it that it is best to recognise it till our present knowledge of the East African avifauna has been tabulated and brought up to date.

The South African Sub-Region.

It is to be noticed that in the map I have indicated the existence of a

1.—Cape Province.

By this I intend the Cape Colony south of the Karroo and reaching eastwards to the neighbourhood of Port Elizabeth and East London. Quite a number of distinct forms inhabit this province.

2.—The Natalese Province.

There is another province, which commences, probably, in British Caffraria, certainly about the Berea, near Durban, and extends along the Drakensburg mountains, including the forest regions of the Transvaal, such as the Lydenburg district, and reaches the Zambesi, whence I believe it will be found to stretch across to Benguela and Angola.

Lastly, I propose to recognise a

V.—VICTORIAN[1] OR CAMAROONIAN SUB-REGION,

consisting of the elevated mountains of Central East Africa, and extending from the highlands of the Lake Regions across to the peaks of the Camaroons. This is a natural sub-region and is characterised by a number of forms. The recent collections sent home by Mr. H. H. Johnston, C.B., show that this mountain fauna extends to the Shirè Highlands, as is evidenced by the presence of *Cryptospiza* and the inevitable green *Xenocichlæ*, which we now look for at a certain elevation, and which are known from the Shirè Mountains, Kilimanjaro, Elgon, and the Camaroons. The occurrence of many peculiar species and genera such as *Pædilorhynchus* in the Uganda Highlands, and again in the Camaroon Peaks, shows that there is a close connection between the Avifaunæ of these elevated mountains, and, doubtless, Kenia and Ruwenzori will be found to belong to the same system. The highlands of Shoa certainly belong to it, and some of the Abyssinian mountains will also, in all probability, have to be included.

VI.—LEMURIAN OR MASCARENE SUB-REGION, CONSISTING OF MADAGASCAR AND ADJACENT ISLANDS.

E.—INDIAN REGION.

This follows the usual boundaries laid down by Dr. Sclater and Dr. Wallace. The division of the Indian Peninsula into provinces will probably be determined by the mean annual distribution of rainfall, as shown by Mr. Hume in his interesting paper ("Stray Feathers," vol. vii., pp. 501, 502). This paper has not received half the attention which it deserves, and many of the problems connected with the presence of a Malayan element in the western portion of the peninsula and the mountains of Southern Ceylon become less difficult of comprehension when we find that they belong to countries where the rainfall is 70 inches and upwards. In the Himalayas we have apparently the foci of three separate sub-regions; there is a strong Palæarctic element illustrated by the number of species of *Carpodacus*, *Pyrrhula*, etc. There are also recognisable two more sub-regions.

Another Sub-Region would be

The Himalo-Chinese Sub-Region, somewhat as defined by Mr. H. J. Elwes, and illustrated by such forms as *Polyplectron*; while a third

[1] Not named directly in honour of Her Most Gracious Majesty, but intended to show that Mount Victoria in the Camaroons, and the Victoria Lake country in Equatorial Africa, belong to the same natural sub-region.

natural sub-region seems to be the Himalo-Malayan Sub-Region, which includes the high mountains of Burma, the Malayan Peninsula, Sumatra, Java, and Borneo. A representative form would be *Psarisomus*. The Indian Region would, therefore, be divided as follows :—

1. Indian Peninsular Sub-Region.
2. Indo-Malayan Sub-Region.
3. Indo-Chinese Sub-Region.
4. Himalo-Malayan Sub-Region.
5. Himalo-Chinese Sub-Region. The high mountain portion of the Indo-Chinese Sub-Region.

In the Australian Region, east of Wallace's line, I think that the following sub-regions can be fairly well recognised :—

I. Celebean Sub-Region.
II. Moluccan Sub-Region.
III. Papuan Sub-Region, which should include North Eastern Australia with its *Casuarius* and *Tanysiptera*.
IV. Australian Sub-Region.
V. New Zealand Sub-Region.
VI. Fijian Sub-Region.
VII. Hawaiian Sub-Region.

R. Bowdler Sharpe.


ZOOGEOGRAPHICAL AREAS
ILLUSTRATING THE
DISTRIBUTION OF BIRDS
By R. BOWDLER SHARPE.
1893.
On Lambert's Equivalent Projection.
ISOTHERMAL LINES YEAR
REFERENCE
TO ZOOGEOGRAPHICAL REGIONS, SUB-REGIONS, PROVINCES, & SUB-PROVINCES
A. NEARCTIC REGION.
1 Arctic Sub-Region.
2 Alaskan Arctic Sub-Region.
3 Aleutian Sub-Region.
4 Cold Temperate Sub-Region.
Warm Temperate "
5 Humid Prov.: Appalachian Sub-Province.
6 Humid Prov.: Austroriparian Sub-Province
7 Arid Province: Campestrian Sub-Province.
8 Arid Prov.: Sonoran Sub-Prov.
B. NEOTROPICAL REGION.
1 Antillean Sub-Region.
Central American Sub-Reg.
2 Mexican Province.
3 Isthmian Province.
4 Patagonian Sub-Region.
5 Brazilian Sub-Region.
6 Amasonian Sub-Region.
7 Sub-Andean Sub-Region.
C. PALÆARCTIC REGION.
1 Arctic Sub-Region
Eurasian Sub-Region.
2 European Province.
3 East Siberian Province.
4 Manchurian Sub-Region.
Mediterraneo-Asiatic Sub-R.
5 Mediterranean Province.
6 Mediterraneo-Persic Prov.
7 Mongolian Province.
8 Himalo-Caucasian Sub-Reg.
D. ETHIOPIAN REGION.
1 Saharan Sub-Region.
2 Sudanese Sub-Region.
3 West African Sub-Region.
4 Abyssinian Sub-Region.
5 East African Sub-Region.
6 South African Sub-Region.
7 Cape Province.
8 Natalese Province.
9 Camaronian Sub-Region.
10 Lemurian Sub-Region.
E. INDIAN REGION.
1 Indian Peninsular Sub-Reg.
2 Indo-Malayan Sub-Region.
3 Himalo-Malayan Sub-Reg.
4 Himalo-Chinese Sub-Region.
F. AUSTRALIAN REGION.
1 Celebean Sub-Region.
2 Moluccan Sub-Region.
3 Papuan Sub-Region.
4 Australian Sub-Region.
5 New Zealand Sub-Region.
6 Fijian Sub-Region.
7 Hawaiian Sub-Region.
The Contour-Lines for 600 and 6000 Feet, and the 100, 1000, 2000, 3000, and 4000 Fathom-Lines, are indicated by Faint Lines.

III.

Earthworms and the Earth's History.

THERE has been recently a considerable discussion in the pages of *Nature* as to the probability of a former extension northwards of the antarctic continent. The discussion has also affected the columns of this Journal, which published last month an abstract of the recent views of Mr. H. O. Forbes on the subject. Mr. Forbes believes in this continent. We quite agree with him, though for reasons different to those which he urges.

It seems to be an obvious platitude that those animals are most important for use in arguments relating to the past connection of land masses which do not migrate of their own free will across the sea, and are incapable of being carried over by accidental modes of transit, and yet, with one exception to be mentioned presently, the group of all others which best fulfils these conditions has been completely ignored by writers on the subject of geographical distribution. The group to which we refer is that of the earthworms.

The earthworm, like the agricultural labourer, is wedded to the soil; more so, indeed, for the labourer does occasionally emigrate, but the earthworm never, and for the very good reason that sea-water is fatal to his constitution. Although earthworms do not apparently ever cross the sea by any of those modes of transit which are held to account for the range of other animals, there is sometimes (again like the agricultural labourer) "assisted emigration." Commerce between different countries is apparently responsible for the transference of earthworms from one country to another—particularly, of course, the exportation and importation of plants. That this is really the case is capable of the easiest proof; any collection of living plants from a foreign country is nearly sure to contain a few worms, besides, of course, insects and other small creatures. Indirect but still convincing proof of man's agency in affecting the distribution of earthworms is shown by the wide range of the common earthworms of Europe which belong to the genera *Lumbricus* and *Allolobophora*. In Europe and North America these two genera are the prevailing types, to the nearly complete exclusion of any others. In such distant and widely-separated regions as New Zealand and South America, the same genera are met with, but they are not the prevailing types.

Now, there is not, so far as I am aware, a single exception to the truth of the statement that in all these cases the foreign species are identical with those found in Europe and North America. Were the forms in question really indigenous, different species, or at the very least, varieties might be fairly expected to occur ; but they do not occur. Any difficulties in the existing distribution of animals are apt to be explained in one or two ways; either a continent is evoked from the vasty deep for this occasion only, or tree trunks and floating *débris* are pressed into the service. As has been already mentioned, earthworms are killed by salt water ; so that we must assume a former land connection to explain their presence on both sides of a strait of whatever width. It is a most remarkable fact that works dealing with geographical distribution and the geological arguments to be derived from a study of animal distribution have not made use of this important material ; the only paper known to us which does make use of this group is by Professor Spencer on the fauna of Tasmania (*Australasian Assoc. Adv. Science*, Presidential Address, Section D, 1892). Other terrestrial invertebrates are doubtless of use ; but up to the present, hardly anything except insects and land shells have been studied from this point of view. But in neither of these groups are the anatomical relations between the genera and species so clear as might be wished. This partly destroys their value, as does also the fact that they possess facilities for migration, passive or active, not possessed by the earthworm. Sealed up in its shell by the temporary operculum, a snail is capable of extended travel ; butterflies can, as we know, fly for long distances ; the earthworm has none of these advantages; the cocoons might, it is true, be conveyed on the feet of birds, but they are often below the soil, sometimes very far down ; this mode of transit has very possibly occurred in the case of the aquatic worms allied to the earthworm, which deposit their cocoons at the margins of streams and rivers frequented by birds ; in this way we may possibly account for the wide prevalence of the common "red-worm" of our rivers (*Tubifex rivulorum*).

Applying these facts to the particular case in question, it seems to us that the former existence of a southern continent is rendered at least very probable. The earthworm fauna of New Zealand, which is fairly well known at present, is characterised by the presence of the genus *Acanthodrilus*, this genus and its immediate allies forming nearly the whole of the indigenous earthworm fauna of the country. Australia, on the other hand, is characterised by quite a different family, the Cryptodrilidæ, comprising three or four genera, perhaps more ; *Acanthodrilus* is represented by only two species, which are found upon that side which is believed to have had, at a remote period, a connection with New Zealand. The Australian family is, on the other hand, only represented by two or three species in New Zealand. The genus *Acanthodrilus* (using the genus in its widest sense) is also characteristic of Patagonia and South Georgia (so far as is known), and the more southern parts of South America. The only worms known from Kerguelen and from Marion Island are members of the same genus. " Ethiopian " Africa has many species of the family which differ more from those of New Zealand than the latter do from those of Patagonia—in fact, they have been placed in a separate genus, *Benhamia*; a few members of the same genus or sub-genus occur in the West Indies and one or two in India and Malaya. It is only, however, in Patagonia and New Zealand that this genus is the prevailing one; in the other countries mentioned it occurs, but there are numerous other and more abundant forms. The important point to be noticed is that, as we pass northward from the antarctic region, this particular genus thins out, until in the continents of Europe and Asia it is hardly to be found.

The former existence of a habitable antarctic continent with arms stretching to New Zealand, Africa, and Patagonia seems to be the clearest way of explaining these facts.

F. E. Beddard.

IV.

Some Useful Methods in Microscopy.

HAVING been occupied during the spring of 1892 in studying the early stages in the development of *Loligo* and *Sepia* at Naples, I found myself unable to make any progress in solving the problems presented by these difficult objects until I had obtained a satisfactory method of preparing and mounting blastoderms, quite freed from yolk, for study of surface-views under the microscope. After trying many methods, I at last hit upon one which, while exceedingly simple in application, not only gave the most beautiful results for these blastoderms, but has also proved applicable for other objects which are not more than a few cell-layers in thickness, such as early chick embryos, and still more so, of course, for unicellular objects, such as Protozoa. It seemed to me, therefore, desirable to describe briefly the methods employed by me, since they may prove useful to other zoologists. In so doing, however, I do not in any way wish to lay claim to any originality or precedence in inventing them. For all I know they may have been already described by many other investigators. Wherever I give any method which I have obtained from an author I shall give the reference to him; where I do not do so, my apparent oversight is to be set down to my ignorance of the now vast literature of the subject.

The method employed by me for cephalopod blastoderms is exceedingly simple in principle, though its mode of application varies, of course, with the nature of the object, and may require a high degree of manual skill, to be obtained only by much practice. It is as follows. The fresh objects are first brought into Hermann's fluid for a short time, until all the cells are fixed; this is, as a rule, effected in 2–5 minutes, but the proper length of exposure to the action of the reagent can best be determined by the eye; as soon as the cells become white and opaque they are sufficiently fixed. Too long exposure is injurious to subsequent staining.

After the Hermann's fluid, the objects are carefully washed in two or three changes of water to extract all the reagent. This also is an important point for staining. After washing, the objects come into either alum carmine or Paul Mayer's[1] newly invented stain carm-alum.

[1] "Ueber das Färben mit Carmin, Cochenille, und Hämatein-Thonerde." *Mitth. Zool. Stat. Neapel.* Bd. x., Heft. 3, p. 489.

Here they are left for an hour or two until completely stained. They are then washed with water and brought up through the alcohols—30 %, 50 %, 70 %, 90 %, and absolute—into oil of cloves, and then mounted in Canada balsam.

The above is the complete plan of procedure for this method. Before, however, proceeding to describe its practical application to the objects in question, a few words are necessary concerning the agents mentioned.

Hermann's fluid was, as its name implies, invented by Hermann[2] in his studies on nuclear division, etc., in the testis. It may be described as a paraphrase, so to speak, of the well-known Flemming's fluid (the second or strong solution) in which the 1 per cent. chromic acid is replaced by 1 per cent. platinum chloride, thus:—

Platinum chloride 1 per cent.	15 parts.
Osmic acid 2 per cent.	4 ,,
Glacial acetic	1 part.

I always make it up according to the following recipe:—Take one of the ordinary sealed glass tubes containing a gramme of osmic, as it is commonly sold, and break it in a bottle of a capacity of about 300 cc.; then pour on it 50 cc. of pure distilled water, 12·5 cc. of glacial acetic, and 200 cc. (more accurately 187·5 cc.) of a 1 per cent. solution of platinum chloride. Shake the contents of the bottle up together and put it aside. By the next morning the osmic will have dissolved and the mixture will be ready for use.

Alum carmine is too well-known a staining fluid to every zoologist to require description here; but the very valuable carmine stain recently invented by Dr. Paul Mayer,[3] to which he has given the name of Carmalum ("Carmalaun"), may not be so familiar to the majority of English zoologists. The essence of his discovery consists in using the pure colouring principle, carminic acid, instead of commercial carmine. Carminic acid can be obtained from Dr. Grubler in Leipzig at the rate of about four marks for ten grammes. Having obtained the carminic acid, the method of making up the staining solution is exceedingly simple, and can be carried out as easily in the domestic kitchen as in the well-equipped laboratory, thus: mix together in a suitable vessel one gramme of carminic acid, ten grammes of ammonia alum, and 200 cc. of distilled water. Heat the mixture up to about boiling point, until the ingredients are dissolved, and then, after cooling, carefully pour off or filter the liquid, which is now ready for use. A small crystal of thymol or some other antiseptic should be added, in order to keep it free from organisms. Carmalum is a dark-red liquid with great staining powers, having especially the great advantage that, like picrocarmine, it will give a beautiful stain to objects that have been fixed in pure osmic acid, which alum carmine will not do. It tinges the protoplasm slightly as well as the nucleus,

[2] *Arch. f. Mikroskop. Anat.* Bd. xxxiv. (1889), pp. 58–60.
[3] *Loc. cit.*

hence it is greatly to be recommended, over alum carmine, for objects which it is desired to cut into thin sections, but in the case of objects which it is desired to mount *in toto* it is liable to overstain and render them opaque, and should therefore, in my opinion, be avoided ; at any rate, this is so with chick embryos.

I shall now proceed to describe the manner in which this method may be applied to each of the following classes of objects, (1) to cephalopod blastoderms; (2) to chick embryos; (3) to protozoa.

The eggs of Sepia or Loligo are probably well-known objects to the majority of zoologists, and it is not necessary for me to describe them further than by stating a few essential facts about their structure. Although the eggs of the two genera differ widely in appearance they agree in being more or less oval in form and possessing a relatively enormous mass of yolk, at one pole of which is placed the blastoderm, which is comparatively minute at an early stage, though it soon grows over the yolk. Outside the yolk comes a delicate membrane, which, for the sake of description, we may term the chorion, without wishing to prejudge its homologies. In the early stages the chorion is in close contact with the ovum, but separates more and more from it as the embryo becomes formed. Outside the chorion come several gelatinous coats. In Loligo the egg is embedded with a number of others in a common jelly, but in Sepia each egg is separate and has its own concentric coats of jelly.

In order to describe my method I shall take the egg of Loligo, which is rather more difficult to manipulate than Sepia, on account of its smaller size, though for both eggs the method is essentially the same. Moreover, my remarks apply more especially to the earlier stages, from the commencement of segmentation up to the formation of the three layers, during all of which time the blastoderm has the form of a disc or inverted saucer, its curvature being very slight. After it has commenced to grow rapidly round the yolk, it is, of course, impossible to prepare good surface views, for the simple reason that its curved form renders it physically impossible to flatten out the cup-shaped blastoderm without breaking it. Let it be supposed, therefore, that we wish to obtain a series of preparations, either to show the segmentation of the egg and the formation of the germ-layers, or to show nuclear figures.

The first step is to free the egg from its gelatinous envelopes and leave it only surrounded by the chorion. This can be done without much difficulty after a little practice. In Loligo I found it easiest to remove the jelly with a pair of mounted needles in a shallow vessel containing sea-water. The needles employed for this purpose should be as rigid as possible, very springy needles being likely to damage the structures. Now, if one needle be made to pierce the jelly, without of course touching the chorion, and then be held with its point resting on the bottom of the vessel, and slightly slanting, while with the other hand the other needle be made to cross it rapidly in such a way, that while its point scrapes along the bottom of the vessel its edge scrapes against that of the stationary needle, an exceeding sharp and clean cut can be made through the jelly, which is snipped as cleanly as with a pair of scissors. (See accompanying Fig. 1 and explanation.) With three or four such snips, which, with practice, occupy as many seconds, the chorion is almost completely denuded of its adherent jelly, without the blastoderm suffering the least injury. In Sepia the outer coats of jelly may be removed by simply cutting them with scissors and cautiously squeezing out the egg.

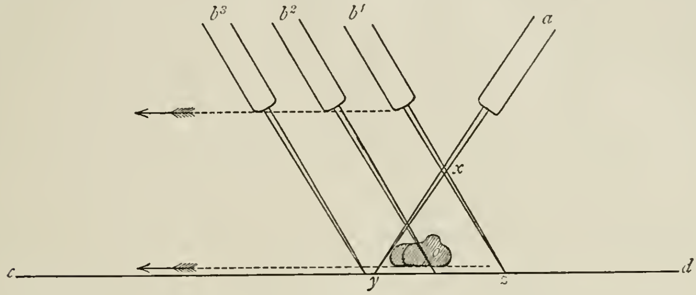

Fig. 1.

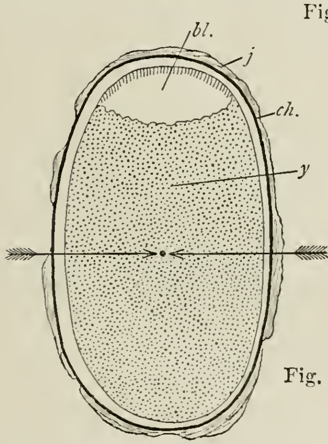

Fig. 2.

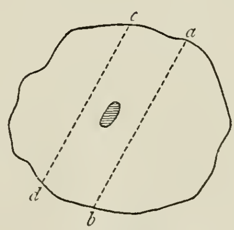

Fig. 3.

Fig. 1.—Diagram to illustrate the method of snipping an object in two with two needles, *a* the stationary needle, b^1, b^2, b^3, three positions of the moving needle, *o*, object, *cd* bottom of the vessel. The arrows and the dotted lines indicate the direction in which *b* moves. The object is at first enclosed in a triangle, *xyz*, the plane of which should coincide with the plane in which it is to be cut. As *b* moves the triangle *xyz* is reduced to a point, and the object, if soft, is irresistibly cut in two in the plane of *xyz*.

Fig. 2.—Cephalopod Egg.—*bl.* Blastoderm. *ch.* Chorion. *y.* Yolk. *j.* Fragments of Jelly.

Fig. 3.—Diagram to explain section-cutting (p. 122).

We now have the egg enveloped only in the chorion, with the minute disc-shaped blastoderm at one end. The blastoderm is transparent and almost invisible. The egg is now removed from the sea-water and dipped into a capsule containing Hermann's fluid. After a few seconds the blastoderm shows up very plainly as an opaque white patch. It should be left for about a minute longer and then removed to a shallow vessel containing distilled water, for the double purpose of washing out the reagent and of commencing to pick out the blastoderm from the yolk.

The process of freeing the blastoderm from the yolk is a delicate operation and more easily described than done, as it requires much skill and practice in manipulation. The proportion of failures to successes is at first very large. I proceed as follows. The egg naturally falls upon its side, as represented in the accompanying diagram, Fig. 2. The first manœuvre is to spear it right through the middle with a needle, in the spot to which the two arrows are pointing. Then, the impaling needle being held still, a snip is made with another needle in the direction of one of the arrows, after the manner described above, and, immediately after, a second snip in the direction of the other arrow, so as to cut the egg into two halves across its equator. By this method, which is very easy after a little practice, the egg with its enveloping chorion is cut in half cleanly and sharply, without injuring anything, in a way that would be impossible with scissors or knife. One half, of course, contains the blastoderm resting on a hemispherical mass of semi-fluid yolk, and enveloped in a cup-shaped chorion. With a little care the yolk and blastoderm can now be drawn right out of the chorion, which henceforth troubles us no longer.

We now have the blastoderm resting on a rather amorphous mass of yolk. We may be either intending to stain the blastoderm in alum carmine before it has been into alcohol, or we may wish to bring it into alcohol unstained, and then colour it with hæmatoxylin or aniline stains. In the former case, the further removal of yolk is best delayed till after staining. The yolk with the blastoderm still adhering to it is carefully taken up on a lifter (of course, with the blastoderm uppermost) and put into fresh distilled water in order to wash out the Hermann's fluid completely, and thence it is taken into alum carmine, where it should be left one to three hours. At the end of that time it is put again into distilled water, when the yolk very readily separates away clean from the blastoderm, the alum carmine seeming to have a slightly macerating action on the yolk. If, on the contrary, the blastoderm is to be brought into alcohol without staining, the process is exactly the same with the omission of the alum carmine, the blastoderm being left longer in water in order to macerate and separate off the yolk.

The blastoderm, stained or unstained, is now freed from yolk and in distilled water. It is not all plain sailing yet, however. When the blastoderms come into the weaker grades of alcohol, they become excessively sticky, and will adhere to the walls of the vessel, to the needle, or to anything they touch, coming away from it in pieces when detached. Moreover, they have a tendency to curl up. All these unpleasant habits, however, can be cured by a simple device, which I have also found applicable to other objects. It is as follows. The blastoderm is placed in a watch-glass containing distilled water, in which a coverslip is submerged. The coverslip, of course, only touches the watch-glass with its four corners, and there is a space underneath it, between it and the bottom of the watch-glass. The blastoderm is now carefully floated on to the middle of the coverslip, so that its primitively external or upper surface is downwards and touching the glass. Then with a syringe or pipette the water is carefully drawn off till it falls below the level of the coverslip, so that the blastoderm is left stranded on the coverslip. As soon as this has been effected the coverslip can be lifted up with a forceps with the blastoderm upon it. The blastoderm must not be allowed to dry completely up, but all the superfluous moisture must be drawn off, so that it is pressed closely to the coverslip by capillary attraction. Its cells will then, in most cases, stick very tightly to the coverslip, which is now turned over and transferred to a second watch-glass containing 30 per cent. alcohol, in which it is placed, with the blastoderm *downwards.* It not unfrequently, however, happens that the blastoderm becomes unstuck and flies off with great rapidity when it comes into the 30 per cent. spirit. When this occurs the same performance must be gone through, in 30 per cent. alcohol, as has just been done in distilled water, and the coverslip, with the blastoderm stuck on to it again, is removed into 50 per cent. spirit, of course with the blastoderm downwards. It very seldom comes unstuck a second time. The coverslip, with the attached blastoderm, can now be transferred with the greatest ease from one liquid to another, and stained, if necessary. Finally, it is brought into absolute alcohol, and then into a watch-glass of oil of cloves, without, however, there being sufficient oil of cloves to immerse the coverslip, but only enough to wet the side to which the blastoderm is attached. The coverslip is then placed on a slide, after being supported with wax feet at its corners to prevent the blastoderm being crushed, and mounted in Canada balsam.

In this way most admirable preparations of blastoderms can be obtained, which not only show cell-structure and nuclear division to perfection, but also are all that could be desired from the morphological point of view; the radiating cells, for instance, which, at a late stage of segmentation, spread out from the edge of the blastoderm over the yolk, being most perfectly preserved. I have satisfied myself, by careful examination, that if the method is carefully carried out, no cells are left behind on the yolk. The method of sticking the blastoderm to the coverslip gives the further advantage that, while the blastoderm can be effectually protected from being crushed when mounted on the slide, it is only separated by the thickness of the coverslip from the objective, and can therefore be studied with any power that is desired, however high.

If, on the other hand, it is desired to cut into sections the blastoderm so removed, the process is just the same as above, with the difference that a thin slice of liver is substituted for the coverslip. I use amyloid human liver kept in weak spirit for this purpose. The blastoderm can be made to stick to it, as a rule, in just the same way as to the coverslip, and then liver and blastoderm together can be cut into sections.

I have used Flemming's fluid instead of Hermann's, but not with such good results. Not only do the objects not stain so well after it, but they become very brittle, and therefore more difficult to extract from the egg. The best stain for general purposes is certainly alum carmine, and borax carmine should be avoided. I have obtained, however, a beautiful preparation for nuclear figures by bringing the blastoderm unstained into alcohol, and then staining it first in gentian violet dissolved in aniline water, and secondly in orange G., washing out both stains at the same time in absolute alcohol.

For chick embryos, the mode of applying this process, though essentially the same, is very different in practice. Here there is no difficulty in extracting, say, for instance, a chick embryo of 36 hours from the egg and freeing it from the yolk, but it is liable to curl up completely in the reagents, and then to get so brittle that it is impossible to straighten it. The method I have employed with good success is as follows. The egg is opened and the blastoderm extracted in the usual way in warm salt solution. The blastoderm is then dipped out with a flat shallow dish, such as the lid of a capsule. Here it is floated on to a small piece of black paper, in which an oval piece has been cut out of the middle, slightly larger than the area pellucida. The blastoderm is placed on the paper in such a way that the embryo and area pellucida are over the hole in the paper. By pressing down the edge of the blastoderm on to the paper with a needle all round it can be made to stick. This part of the process requires rather careful manipulation. The next thing is to carefully remove all the salt solution by means of a syringe, so that the piece of paper with the blastoderm attached is left stranded on the bottom of the vessel. Hermann's fluid is then carefully poured in, in sufficient quantity to cover the paper and the embryo. After it has been in this for a few minutes (until the area opaca is beginning to turn light brown), the paper with the attached embryo may be picked up with a pair of forceps and transferred to distilled water to wash out the reagent. It will be found that the Hermann's fluid has given the blastoderm a certain amount of rigidity, which enables it to resist the strain of being lifted about, combined, however, with a small amount of flexibility. Flemming's fluid, on the other hand, makes it very brittle and therefore more liable to break. If, moreover, it has been properly stuck down on the paper at first (which requires some practice), it will remain sticking and will not curl up. After being washed in two or three changes of distilled water it can be stained, which is best done in alum carmine if it is to be mounted whole, or in carmalum if it is to be sectioned. For mounting it whole there is no need to remove the black paper, the presence of which makes it easier to shift the blastoderm from one liquid to another, while it does not, of course, prevent the embryo from being studied under the microscope, if it has been carefully stuck over the hole in the paper. If the embryo is to be sectioned it can either be carefully removed from the paper with a needle, after having come into strong alcohol, though it is then difficult to prevent it curling up a little; or the embryo may be embedded, paper and all, in paraffin, and the paper then carefully cut away with a sharp knife by paring down the block of paraffin until only the embryo and area pellucida are left.

Early chick embryos mounted in this way, after Hermann's fluid and alum carmine, in the first place show the general feature (medullary groove or canal, notochord, primitive streak, etc.) most excellently as low power objects, but also show the most perfect cell detail if carefully focussed with a high power. The branching mesenchyme cells are especially well-preserved, and the delicate network formed by their processes is beautifully shown, giving a wonderfully life-like amœboid appearance when focussed down. Nuclear figures are to be seen nearly everywhere. I have not seen any method which gives better results that this very simple one, which only takes about four hours from the time of opening the egg to the time of mounting the embryo in Canada balsam. The alum carmine gives a good stain, but one which is rather too delicate for thin sections, for which carmalum is preferable.

I will finally describe how this method may be applied to Protozoa, say, for example, *Paramecium*.[4] A drop of water containing the animals is placed on a slide and covered by a cover-glass supported at the four corners by wax feet. The drop should be as free as possible from *débris* of all kinds, since it hinders the free passage of liquids through the space under the coverslip. The wax feet should be high enough to keep the Paramecia from being squeezed, but low enough to prevent them being able to move very fast; that is to say, the coverslip should just not touch them. A drop of Hermann's fluid is then placed at the side of the coverslip and cautiously drawn through by blotting-paper (or better, filter paper) placed at the opposite side of the coverslip. In doing this it is necessary to be very careful that the living and freely swimming animals are not swept out from under the coverslip by the current before the reagent has reached them. This may be prevented with a little trouble by drawing the Hermann's fluid half through, and then putting another drop the other side of the cover-glass and drawing it back again. When once, however, the Hermann's fluid gets to them, it not only kills them instantly but also sticks them to the slide, so that rapid currents can be drawn over them without their moving at all. As

[4] This method is really a modification of one which I learnt in Professor Bütschli's laboratory in Heidelberg in 1888. Then I used Flemming's fluid instead of Hermann's.

soon as all the animals have in this way been killed and fixed on to the slide, all difficulty is passed. After the Hermann's fluid has been allowed to act for a few minutes it is washed out with water, drawn through in the same way by filter paper, and when well washed, the stain, alum carmine or carmalum, is run in. This should be left to stand for an hour or so in a damp chamber to prevent evaporation, and then washed with water and brought up through the alcohols into oil of cloves, which is finally succeeded by Canada balsam.

This method, apart from its simplicity and easiness, has the advantage for such forms as Ciliata of preserving cilia, undulating membranes, etc., very well indeed, besides giving good results for the nucleus and internal protoplasmic structures. I do not find it so successful, however, for Amœbæ, for which osmic acid or its vapour, followed by picrocarmine or carmalum, gives better results, though more difficult to apply, since the objects do not become fixed to the slide. I have obtained a most beautiful and instructive series of sections through a *Pelomyxa* which was fixed with osmic acid and stained with carmalum, the sections being further coloured on the slide with various aniline stains. On the other hand, the Hermann's fluid and alum carmine method succeeds very well for *Actinosphærium* and for Gregarines.

I may mention in this connection that I obtained an excellent preparation of a plasmodium of *Badhamia*, that was creeping over a slide, in the following way: after fixing with osmic vapour and staining with picrocarmine,[5] it was floated on to a coverslip in 30 per cent. alcohol, in a watch-glass, and made to stick to it by drawing off the liquid, in the same way as has been described above for cephalopod blastoderms. Not only was the general form of the plasmodial network well preserved, but the minute structure, especially the nuclei, were beautifully shown.

I have now described ways of applying this method of a short exposure to Hermann's fluid, followed by a carmine stain, to three very different classes of objects, which may, perhaps, serve as types for yet others. I believe, for instance, that it would prove a valuable method for studying the segmentation of the egg of the fowl, or of other meroblastic ova. The fixing reagent in question was, however, invented by Hermann to be applied to larger objects for a much longer time, in order to study all detail and nuclear figures, just in the same way as Flemming's fluid is usually employed. Hermann originally employed it for the testis of the mouse and salamander, the organ being removed and hardened *in toto*. I have also obtained very good results, especially as regards nuclear figures, for the testis of the axolotl. Two were removed from an axolotl in October, and placed in Hermann's fluid, one for three days, the other for three months. In the latter the structures were a little darker, but not

[5] Weigert's picrocarmine, prepared by Dr. Grubler of Leipzig.

otherwise altered. They were then washed in running water under the tap for at least 24 hours. It is frequently necessary to wash objects in this way, and when they are small they are liable to be lost. I may recommend, therefore, the following simple and absolutely safe piece of apparatus. A piece of thick glass tubing, about nine inches long, is closed at one end by having a piece of gauze tied over it. It is then supported with the closed end downwards in a vessel about six inches in depth, which stands in the sink.[6] The object to be washed is placed in the tube on the gauze, and water is allowed to trickle from the tap into the upper open end of the tube. The water passes over the object and through the gauze into the outer vessel. When the latter is full it overflows down the sink. In this way even very minute objects can safely be left all night exposed to the current, provided they cannot pass through the gauze. At the same time they are in the direct current.

The testes after being thus washed were hardened in alcohol and cut into sections, and stained on the slide with various anilines,[7] and in this way most successful preparations of nuclear figures were obtained, giving a beautiful differential stain in combinations of different anilines. I have also obtained very good sections of *Lumbricus* in this manner, but here, unless the sections are very thin, they require to be bleached before staining, owing to granules in the cells being blackened by the osmic. Bleaching was done by placing the slide of sections in a bottle of 80 per cent. alcohol, having previously covered the bottom of the vessel with a layer of crystals of chlorate of potash.[8] A few drops of concentrated hydrochloric acid were then cautiously added, and the bottle subjected to slight warmth (by placing it on the paraffin bath). In about an hour's time the chlorine evolved has bleached the sections.

I have above briefly mentioned a method of fixing the cephalopod blastoderms on to thin slices of liver in order to section them. This method is also so useful for orientating minute objects in paraffin for

[6] It is not, of course, in any way necessary that the glass tube and the vessel should be nine inches and six inches in height respectively, but only that the tube should be considerably longer than the vessel is deep, in order that the water which runs through is obliged to pass through the gauze, and cannot overflow from the top of the tube.

[7] For all the anilines used, namely, Safranin, Gentian Violet, Eosin, Fuchsin, Fuchsin S., Dahlia and Orange G., the following recipe (recommended by Hermann, *loc. cit.*) was used for making up the solutions:—

Staining substance (dry)	1 gramme
Absolute alcohol	10 cc.
Aniline water	90 cc.

Anilines made up in this way stain very intensely and sharply, washing out being done in absolute alcohol. Safranin may, however, be washed in acid alcohol. These solutions do not, however, keep very long, especially Gentian Violet, Eosin, and Dahlia, and require to be used fresh.

[8] This method was, I believe, first described by Kühne, as a means of depigmenting retina; it can be used both for objects *in toto*, or, as here described, for sections on the slide.

cutting sections that, at the risk of stating what is already well known, I venture to give an account of how to use it.[9] Let us suppose that the object to be cut is either small and circular, with, however, a very definite axis of symmetry, like a cephalopod blastoderm, or that it is minute and with differentiated axes, like a Paramecium or a sponge larva, and, further, that it is required to cut sections through it having a definite relation to its planes of symmetry, *i.e.*, either transverse or longitudinal. Cut a thin slice by hand from the liver and bring the slice and the object into 90 per cent. alcohol. The slice of liver need not have any particular outline. Place the slice of liver on an ordinary section-lifter, without, however, removing it from the alcohol. Then, holding the lifter with the liver on it in one hand, with the other float the object on to the middle of the piece of liver. Now raise the liver, with the object on it, carefully out of the alcohol, and with the hand that is free let a drop of Mayer's glycerine and albumen solution, such as is used for sticking sections on the slide, fall right on the object, and then at once immerse liver and object again in the alcohol. If this is properly done (it requires a little practice) the albumen will at once coagulate round and over the object and stick it firmly to the liver. If it is not rapidly immersed in the alcohol after dropping the glycerine and albumen on it, the glycerine is liable to cause skrinkage in the object.

We now have the object fixed on the slice of liver, as shown in the accompanying Fig. 3 (p. 115). The next process is to put the piece of liver, still in alcohol, under a lens or low power, and with a sharp knife, carefully cut one side parallel to the object along the dotted line, *a b* ; after which the other side can also be cut parallel by the eye along the line *c d*. We now have our object fixed to an oblong piece of liver in such a way that the long axis of the liver corresponds to the long axis of the object, and it is now quite easy to imbed it and cut it, liver and all, transversely or longitudinally at will.

I will finally mention a method which I have found most useful in cutting sections of eggs containing yolk, such as cephalopods, or hard chitinous objects. It was first, I believe, described by Heider.[10] Two solutions are made, one of celloidin, the other of gum mastix, in ether, to which a small quantity of absolute alcohol (about $\frac{1}{10}$ of its volume) has been added. The mastix solution should be thick and syrupy, and the celloidin also as thick as can be got.[11] Equal quantities of these two solutions are mixed together and put away for future use.

[9] I do not know who first invented this method, but I myself learnt it from my friend, Dr. Otto Maas, during my stay in Naples.

[10] "Die Embryonalentwickelung von Hydrophilus piceus, L." Erster Theil, Jena, 1889, p. 12.

[11] Ordinary collodium may be used instead of the celloidin solution.

When a difficult object has to be cut, a small quantity of the mixed mastix-celloidin solution is put into a tube or small bottle, and diluted with ether mixed with a little absolute alcohol until it is quite thin and fluid. We will suppose that, meanwhile, the block of paraffin containing the embedded object is fixed in position on a Jung microtome, or some similar kind of instrument, ready to be cut. Before cutting each section the surface of the paraffin block is painted over with the thin mastix-celloidin solution by means of an ordinary soft paint brush. It almost instantaneously sinks in a little way into the paraffin, and, in the same instant, the ether evaporates, leaving the mastix-celloidin mixture behind. The excess of the mastix-celloidin is then rubbed off with the finger, and the surface of the paraffin block should be polished with the finger by rubbing it several times. If now a section is cut, it will be found to be held together by the mastix-celloidin mixture which has penetrated into the object, and thus prevents the breaking up of the section that so often follows when yolk or chitin is cut. By this simple, if rather tedious, method, I have obtained uninjured series of sections not only through eggs containing yolk, but also through the thorax of a large mygaloid spider, in which the chitin had not been softened in any way. I have sometimes also found it necessary for things stuck on liver, as the liver is liable to become very brittle.

E. A. Minchin.

V.

Recent Additions to our Knowledge of the Eurypterida.

SINCE the publication of Dr. Henry Woodward's "Monograph of the British Fossil Merostomata" in 1878 (3) the number of important papers dealing with this group has been comparatively small. The additions to our knowledge of the true structure of these interesting Palæozoic forms have, however, been of great value, and their zoological affinities may be said to be fairly well established.

The classical works of Huxley and Salter (1), Hall (2), and Woodward demonstrated that the Eurypterids were Arthropods whose body consisted of a carapace followed by twelve free segments and terminating in a telson, the surface being more or less covered with scale-like markings. The under surface of the carapace bore a number of legs—five pairs, according to the above-cited authorities, and it is in connection with these that some of the most important additions to our knowledge have been made. These legs consist typically of a basal joint, the inner margin of which is armed with teeth and serves as a jaw, much as in *Limulus* or *Apus*. At the posterior end of the tooth-bearing margin is attached (6) a small oval epicoxite exactly similar to that found in the same place in *Limulus*, and at the anterior angle is attached the long, usually six-jointed ambulatory appendage.

The last pair of legs is characterised in all members of the group, except *Stylonurus*, by its greater size and usually somewhat flattened form, and ends in an oval plate. This is usually termed the swimming foot, but it seems more probable that it was used for anchoring the animal firmly in the soft mud of the sea bottom, and possibly also for shovelling up the sand and mud when the beast wished to conceal itself.

In *Pterygotus* the most anterior pair of appendages are large pincers, probably prehensile in function, and evidently attached in front of the mouth, as jaw bases are wanting. They seem to have consisted of only three long joints, though owing to the crumpling of the cuticle of the proximal joint there is often an appearance of a greater number. Behind the mouth there are—according to Schmidt (4)—four pairs of ordinary walking legs, followed by the large "swimming feet." This would make the whole number of appendages six instead of five, as described by Huxley, who only recognised three pairs of walking legs.

In *Slimonia* and *Eurypterus* the full complement of post-oral appendages (five pairs) has long been recognised. In the former the first pair, described by Woodward (3), and by him termed "antennæ," is modified to form tactile organs, but is undoubtedly post-oral in position, as the basal joint bears teeth. Pre-oral appendages have been described in *Eurypterus fischeri* by Schmidt (4) as a pair of fine antenniform structures. In *Slimonia* and in *Eurypterus scorpioides* and *E. conicus* the pre-oral appendages have been described (6) as small stout pincers, much like the cheliceræ in *Limulus*, and corresponding generally to the big pincers in *Pterygotus*, though probably masticatory in function rather than prehensile. The difference between these appendages as described in *E. fischeri* and *E. scorpioides*, etc., is very great, but we must wait for further evidence before making the apparently logical change of dividing *Eurypterus* into two distinct genera, or even families. There would be a certain awkwardness in founding one's classification on a structure which has only been described in three species out of the forty or fifty in the genus.

The legs in the other genera of Eurypterida are less well-known. In *Stylonurus* the last two pairs are enormously elongated. The other legs are only known by fragments, but among the fragments figured in a recent paper by Hall and Clark (5) is a well-marked pair of cheliceræ, which were no doubt pre-oral in position. Limbs intermediate in form between the elongated type of *Stylonurus* and the broad "swimming feet" of *Eurypterus* have been described in *Dolichopterus* (2) and *Drepanopterus* (8).

The hard parts, other than appendages, on the ventral surface of the carapace consisted of the epistoma in front of the mouth and the metastoma behind it. The epistoma was described by Huxley and Salter in *Pterygotus*, but they were misled by the direction of the scale markings on it, and tried—as pointed out by Schmidt—to get it in hind side before. It consists in *Pterygotus*, in which it is best known, of a thin, semicircular plate, which covered the space between the anterior margin of the carapace and the mouth, the pre-oral appendages being attached close to its posterior border. The reversed position of the scale markings, which have their convex side directed forward, would seem to point to its being morphologically the inturned front border of the carapace. It probably corresponds to the hypostoma of Trilobites, which occupies the same position.

The metastoma is a heart-shaped plate attached along the middle line to the ventral wall of the body, between the bases of the last pair of legs and extending outwards and forwards so as to enclose the jaws in a kind of chamber.

The margins of the carapace are often bent over on to the ventral surface to a greater or less extent. This is particularly well-marked in *Stylonurus*, where more than half of the under sufacee is covered in this way.

The ventral surface of the free segments of the body is much less well-known than that of the cephalothorax. The first two segments are covered by the genital operculum, which consists of a pair of plates meeting in the middle line and having a median lobe attached to them. This median lobe varies in form in the same species according to the sex, and may be considered as a copulatory organ in the male and an ovipositor in the female. In some species, *e.g.*, *Slimonia*, one form of this organ shows distinct signs of having been eversible. Underlying this genital operculum are a number of leaf-like structures, almost certainly branchial in function, which appear to have been attached to the body-wall, not to the operculum, and which, therefore, probably represent the reduced appendages of the second free segment. This segment has lost its ventral hard skeleton, and the appendage has become reduced owing to being covered by the genital operculum.

The appendages of the segments behind these first two have only recently been made out. Schmidt (4) describes them in *Eurypterus fischeri* as consisting of a series of four pairs of plates, each pair united in the middle line, and covering the whole ventral surface of the segment to which it belongs. The ventral surface of the body is, according to his description, entirely devoid of sclerites in these segments, and the plate-like appendages bear branchiæ on their posterior surfaces. He describes, though with less detail, a similar arrangement in *Pterygotus ossiliensis*. In *Slimonia*, on the other hand, the abdominal appendages have been described (6) as consisting also of four pairs of plates, which, however, with the exception of the first pair, do not meet in the middle line, but are successively smaller from before backwards. The ventral surface of the segments has, further, well-developed sclerites extending across the whole width of the body. In both cases the chief point is established that the Eurypteridæ had plate-like abdominal appendages bearing branchiæ on their posterior surfaces, and, in a general way, comparable to those of *Limulus*.

Fragments of some very interesting forms have been described from the Lower Carboniferous by Peach (7) under the name of *Glyptoscorpius*. These fragments appear to have belonged to a large animal (over 1 foot in length), the surface of whose body was covered with highly-developed scale markings. The limbs ended in a double claw similar to that of the Scorpions, and the animal was further provided with a pair of appendages closely resembling in structure the pectines of Scorpions. This form was probably aquatic in its habits, and represents not the direct ancestors of Scorpions, for they had appeared long before, but a collateral branch. True Eurypterids existed simultaneously with this form, though possibly many so-called Eurypterid fragments from the Carboniferous belong really to *Glyptoscorpius*.

As regards the zoological position of the Eurypterids, the establishment of the number of cephalothoracic appendages as six, the first pair being pre-oral and—at all events, in most forms—cheliceræ, seems to considerably strengthen the argument for their relationship to the Arachnida. The presence of abdominal appendages also makes them agree much more closely than before with *Limulus*, and, if the lung-books are to be derived from such appendages, with *Scorpio*, &c. It has been pointed out, however, (6) that the great development of the genital operculum and consequent partial suppression of the second free segment is a point of some morphological importance, and contrasts strongly with the development of the same parts in *Limulus*, and even more strikingly in *Scorpio*. On the other hand, it agrees very closely with the arrangement in the Pedipalpi, in which a similar suppression of the second segment has taken place, and it seems probable that these latter are more nearly allied to the Eurypterids than are the Scorpions. This view would tend to separate *Glyptoscorpius* somewhat from the Eurypterids, but as the only evidence of relationship is the presence of scale-markings which are common to many other forms (Trilobites, Phalangidæ, etc.), there can be no serious objection to this. The relationship of these forms to the Crustacea is not so evident, as they show no special points of affinity with any one group. The absence of that special modification of three pairs of appendages to serve as mouth organs, which is characteristic of all crustacea except the Ostracoda, indicates that their point of union must have been very low down the Crustacean stem, and the very definite number of segments and arrangement of appendages in the Eurypteridæ indicates on the other hand that they are removed a considerable distance from any such primitive type. Insofar as they are somewhat more primitive forms than the recent terrestrial Arachnids, they may undoubtedly be said to be nearer to the point of union of the Crustacean and Arachnid lines of descent, but, unless great, and, it seems to me, undue importance is to be attached to their aquatic mode of life, they can hardly be termed intermediate forms.

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Malcolm Laurie.

VI.

Supposed Fossil Lampreys.

SOME months ago, in dealing with our knowledge of the "Forerunners of the Backboned Animals,"[1] reference was made to a singular little organism, *Palæospondylus gunni*, which seemed to indicate the existence in the early Devonian period of a highly-organised member of the group to which the modern lampreys and hag-fishes belong. This fossil, it will be remembered, was discovered by Dr.

Restoration of *Palæospondylus gunni*, somewhat enlarged, by Dr. R. H. Traquair. *c.* cirri; *t.p.* anterior part of cranium; *p.a.* posterior part of cranium; *x.* paired plate-like structure behind the head.

Marcus Gunn in the Lower Old Red Sandstone of Caithness, and our notice was based partly upon the brief original description by Dr. R. H. Traquair, partly upon some specimens obtained by Mr. Donald Calder, of Thurso. We are now glad to announce that, within the last month, Dr. Traquair[2] has published a fuller and more accurate description of the organism, based upon new specimens; and the

[1] NATURAL SCIENCE, vol. i., pp. 596–602 (1892).

[2] R. H. Traquair, "A Further Description of *Palæospondylus gunni*, Traquair," *Proc. Roy. Phys. Soc. Edinb.*, vol. xii., 1892–93, pp. 87–94, pl. i.

announcement is made with all the more satisfaction, since we are able to confirm his emendations and conclusions from a personal examination of a fine series of specimens kindly lent by Mr. Wm. Tait Kinnear, of Forss. Thus far the fossil has only been met with in a single stratum at one quarry near Thurso, and the circumstance is a striking illustration of the accidental character of palæontological discovery, inspiring hope for the future in problems which yet seem beyond solution.

Dr. Traquair appends some good figures of his new specimens of *Palæospondylus*, but we only copy his restored outline in the accompanying illustration. It is now proved that the appearance of a great ring-like lip-cartilage, indicated in our former figure in Natural Science, is due merely to the crushed rim of the anterior part of the skull (*t.p.*), which Dr. Traquair thinks may be equivalent to the palatine region of the lamprey's skull. There is still, however, no evidence of jaws; and it is not quite certain whether the nose was single or paired. The supposed dorsal shield behind the head curiously proves to consist of a pair of oblong plates, apparently not external, but certainly not yet capable of interpretation. Between these the ring-vertebræ are spaced, but behind they form a continuous chain. There are short and stout neural spines in the abdominal region, but no ribs; and the slender neural and hæmal spines in the short caudal region prove to be as already described. No traces of paired limbs can be detected in any specimen.

That *Palæospondylus* is one of the forerunners of the modern Marsipobranch fishes, as Dr. Traquair originally suspected, is thus rendered still more probable by the latest researches. We can only hope that, ere long, similar skeletons may be discovered in a finer and more compact matrix than that of the Caithness flagstones, where the bituminous character of the fossils usually obscures the smaller details.

A. Smith Woodward.

VII.

The Origin of Monocotyledonous Plants.

IN the recently-issued number of the Linnean Society's Journal (vol. xxix., p. 485) is a paper by the Rev. George Henslow, entitled "A Theoretical Origin of Endogens from Exogens through Self-adaptation to an Aquatic Habit." Endogens are, of course, monocotyledons and exogens dicotyledons, and the author argues that the former originated from the latter as the result of adaptation to an aquatic habit. It may be of interest to see on what grounds he bases this theory of the origin of one great group of angiospermous plants from the other.

Of geological evidence there is none, and in the paragraph headed "Survivals" the calculation is not easy to follow. We read, "the belief that endogens are of very early origin is supported by the fact that so many orders of this class include very few genera. Thus, according to the 'Genera Plantarum' of Bentham and Hooker, there are 13 orders out of 166 which have only four or a less number of genera, while two orders have six. Taken together, therefore, these amount to nearly 8 per cent. of the whole class. It need hardly be added that monotypic animals and plants, as well as those orders and genera with but few members, are always regarded as survivals on the principles of evolution and represent a lost ancestry." According to the "Genera Plantarum" there are only 34 monocotyledonous orders, 15 of which, or 44 per cent., contain six genera or less. 165 orders of dicotyledons and three of gymnosperms are recognised. That there is some connection between an aquatic habit and endogenous structures is evident from the proportion of aquatic orders; for, while only 7 per cent. of exogenous British orders are aquatic, the proportion of endogenous orders is 53 per cent.; and of the 224 exogenous orders given in Le Maout and Decaisne's "Analytical Botany," nine may be regarded as aquatic, or 4 per cent.; and of 55 endogenous orders, 18, or nearly 33 per cent.

The numerous points of similarity in morphology, anatomy, and histology between the two groups are adduced as evidence of community of descent. Indications of an adaptive character are also to be seen everywhere, and the author's object "is to show that many of the characters common, more especially to the vegetative organs of endogens, and regarded as points of affinity, are just those which are characteristic of the adaptations of exogens to an aquatic, and, in some cases, to other habits of life."

As regards the points of difference between the two groups, a most important one lies in the structure of the embryo, in the presence of one or two cotyledons. To quote again, "assuming a monocotyledon to have descended from a plant with two cotyledons through an aquatic habit, do we find any instances in support of this view among existing dicotyledons, which are now aquatic, or have presumably descended from water-plants?" *Trapa natans* is instanced. The view generally taken of the embryo of this plant is that there is one perfect cotyledon, and one rudimentary. Drs. Gibelli and Ferrero, who have lately studied the embryology, consider, on the other hand, that the mature embryo is an amorphous mass or true thallome on which is developed a single bud, the plumule; it is, in fact, a degradation resembling that of parasitic and semiparasitic plants, such as Orchids, *Orobanche*, *Balanophora*, etc., and of some aquatic genera, *e.g.*, *Zostera*, *Hydrocharis* and *Utricularia*. Hence, from the example of *Trapa natans* it is quite as legitimate, perhaps more so, to conclude that endogens originated from exogens through a parasitic as through an aquatic habit. *Ranunculus Ficaria* is given as another case. It is admitted, however, that it is not a true aquatic plant, but has, the author thinks, without doubt descended from an aquatic ancestor, one of the reasons being that it has only one cotyledon! This may be, but it is rather begging the question to use it as an example in support of the aquatic theory.

The most instructive instance, however, it is said, would seem to be that of *Carum Bulbocastanum*, which has a monocotyledonous embryo, and an embryology almost identical with that of *Sparganium ramosum*, while its final form is that of the wheat embryo. *Carum Bulbocastanum*, however, inhabits chalky fields, and, like *C. Carui*, in which also one cotyledon is frequently rudimentary, is decidedly not aquatic. Again, the difficulty is got over by supposing that these two species, as well as other umbelliferous genera with linear cotyledons and finely dissected foliage, were ancestrally aquatic; but linear cotyledons are no proof of an aquatic habit, as will be seen by turning over the pages of Sir John Lubbock's "Seedlings"; in the case of umbellifers the shape may facilitate their escape from the often tough-walled fruit. *Hydrocotyle vulgaris*, the aquatic umbellifer *par excellence*, has ovate cotyledons. Anyhow, we can hardly accept these as evidence.

Utricularia, again, has the undifferentiated embryo so characteristic of Orchids and other parasites, and cannot be classed as monocotyledonous. *Erigenia bulbosa*, an anomalous umbellifer, growing in damp and flooded places, has also a very reduced embryo.

In *Cyclamen* the cotyledons first appear as two small eminences, one of which grows no further, while the other forms a large green leaf, resembling in all respects a foliage leaf. Gressner, however, maintains that both cotyledons become leafy, but in any case the embryo and early stages of growth of *Cyclamen*, which, whatever its ancestry, is certainly not now aquatic, bear no great resemblance to that of an ordinary monocotyledon.

Mr. Henslow admits that more than one cause may produce the same or analogous effect, and that while he attributes the total or partial arrest of one cotyledon to an aquatic habit, a similar arrest may arise from atrophy in consequence of the way in which the embryo is folded in the bud, as Sir John Lubbock has shown to be the case in *Abronia arenaria*. The fact, however, remains that not a single undisputed instance is given of the arrest of one cotyledon producing a monocotyledonous embryo in any aquatic exogen. The wholesale degeneration to a quite undifferentiated embryo, as obtains, for instance, in *Utricularia*, does not help the theory.

In the section headed "The Embryo" the author refers to an observation of Van Tieghem's "that the angular divergence between the cotyledon and the next new organ is 180° (generally in endogens), or, if lateral, 90° (as in exogens). When this latter case occurs in endogens, it implies that one cotyledon is due to an arrested condition of the other of two opposite cotyledons; so that the first leaf belongs to a second pair decussating in position with the cotyledons. If the first leaf be at 180°, then it would appear to have usurped the position of the cotyledon that is lost; but it is not strictly on the same level as the cotyledon, inasmuch as the first leaf is completely included within it, as may be seen in *Asparagus*, figured by Irmisch." Thus whether the first leaf alternates with or is opposite to the cotyledon the latter may be supposed to represent the survivor of a pair. No instance is given of the alternate position, but *Tamus communis* is quoted as a parallel instance to *Asparagus*, in which Dutrochet has shown that the first leaf is exactly opposite the cotyledon, is very rudimentary, and dies early, and he has no hesitation in calling it a second cotyledon. The appendage to the embryo opposite the single cotyledon of certain grasses, which several botanists regard as a rudimentary cotyledon, is also instanced.

We can, however, hardly accept these as proof of the origin of the endogenous embryo from an exogenous by assumption of an aquatic habit. In reflecting on the monocotyledonous embryo, we are struck with the variety of form manifested, rather than by evidence of arrest or degeneration. Of course in Orchideæ we see the degeneration, or want of differentiation, characteristic generally of a more or less parasitic habit, and the same holds in Burmanniaceæ; but in the Palms, Grasses, Scitamineæ, and Bromeliaceæ the cotyledon is a complex structure, comprising two distinct, and often clearly-separated portions; one the sucker, resident in the seed, dissolving and absorbing the store of endosperm, the other the sheath, pushing above ground, becoming green, and protecting the developing plumule. Many Irideæ have embryos built on the same plan, but occasionally, as in *Sisyrinchium*, and sometimes in *Iris*, the apex of the cotyledon, having functioned as a sucker, follows the lower part out of the seed, the whole appearing above ground as a subulate green leaf in the characteristic liliaceous fashion. In the Hydrocharideæ and Naiadeæ, the hypocotyl and cotyledon are sometimes coherent into a more or less oval mass terminated by a small radicle, but in others, as *Stratiotes* and *Halophila*, the single cotyledon is distinct from the hypocotyl, and in the former genus large and fleshy. It is also large and distinct in *Alisma*. The absence of distinction between hypocotyl and cotyledon crops up now and again in dicotyledons, and is not necessarily connected with an aquatic habit. It has been already mentioned in *Utricularia*, but is also well shown in the Brazil nut (*Bertholletia*) and another closely-related myrtaceous plant, *Lecythis Zabucajo*.[1]

The sheathing petiole so characteristic of the leaves of monocotyledons, is next brought forward in support of the theory, as it also obtains in many aquatic exogens, *e.g.*, Ranunculaceæ, Umbelliferæ, etc. It is especially characteristic of the Umbelliferæ, but unless we assume this an aquatic order, it seems equally well adapted to land plants, especially herbaceous ones with large leaves, where it will materially strengthen their attachment to the main axis. In the enormous leaves of palms this factor must be a very important one; in fact, it seems to supply a more rational explanation than the persistence of an adaptation to an aquatic habit. If such a habit is the cause of the sheathing form of leaf, why is the form so extraordinarily well-developed in the grasses, which is not an aquatic order. The argument is, however, pushed still further, and it is suggested that the coleorhiza, the sheathing portion which encloses the root in grasses, is a development of the sheath, representing the decurrent sheaths of two united cotyledons, or, perhaps, of one only completely investing the axis.

The "pseudo-coleorhiza" of *Tropæolum* is introduced as throwing light on the origin of the root-sheath in endogens; and the author proceeds to adduce several points in favour of an ancestral aquatic habit for the genus, as nowadays it is, unfortunately, a land plant.

In the first place, however, as Mr. Henslow himself mentions, the structure in *Tropæolum* is formed from the bases of the cotyledons, which are sagittate, being produced downwards on either side as four lobes surrounding the embryo, so that if we suppose them to be completely welded together, "they would form a perfect coleorhiza over the end of the radicle"; but this is no argument in favour of the origin of the coleorhiza from a sheathing petiole.

Thus we cannot agree with the statement with which the first part of the paper concludes, viz., "that there is at least enough presumptive evidence to frame a theory that the monocotyledonous embryo has been derived from a dicotyledonous one by a suppression of one cotyledon"; the cause of the suppression being "the degene-

[1] *Cf.* Lubbock's "Seedlings," vol i., pp. 539–41.

rating effect of the aquatic medium in which the ancestral endogens originally and primarily grew."

A large part of the remainder of the paper is devoted to evidences furnished by the order Nymphæaceæ, which is remarkable for having many points of agreement in structure with endogens, and, as it is aquatic, may be looked to in support of the theory.

As regards the embryo, some abnormal cases of the yellow water-lily (*Nuphar luteum*) described by Hegelmaier are cited, where the two cotyledons were more or less united, and the two halves very unequally developed. These are quoted as "an important and independent witness to the gradual arrest of one cotyledon." It seems rather a teratological phenomenon likely to occur anywhere.

In discussing the arrest of the primary root, Mr. Henslow is, we think, clearer, and adduces what is, at first sight, a more promising piece of evidence. A very characteristic feature of monocotyledons is the disappearance of the primary root, that, namely, produced by elongation of the radicle of the embryo, and the subsequent development of adventitious roots. Now this also obtains in Nymphæaceæ, aquatic umbellifers, and other water-loving exogens. Hence the argument that its presence in endogens which to-day are not aquatic, *e.g.*, grasses, is due to inheritance from aquatic ancestors. It must, however, be remembered that fibrous roots occur in exogens which are not aquatic, and as regards the grasses, one of the largest orders of Angiosperms, when we remember their habit, and how they frequently grow in loose soil or dry exposed situations, the spreading fibrous root seems to meet the case so perfectly that the hypothesis of an aquatic ancestry appears unnecessary. Following a suggestion of Mirbel's, the author explains the arrest of the primary root in endogens as follows. When the cotyledons are raised above ground and act as leaves, they can maintain the existence of the tap-root and enable it to grow until the foliage of the plumule is sufficiently developed to carry on the work. If the cotyledons remain below ground, "the plumule *at once* develops its leaves perfectly, as in the oak, and its foliage can then nourish the tap-root instead of the cotyledons doing so." In submerged aquatic plants the first leaves are more or less rudimentary, and, "being under water, their assimilating powers are greatly impeded," so that the primary root perishes for want of nourishment.

Now, as a matter of fact, as Sir John Lubbock points out in his "Seedlings" (p. 356), when the cotyledons remain beneath the ground the plumule does not, as a rule, "*at once* develop its leaves perfectly," but the primary ones are reduced to scales, often passing gradually into the form of the adult leaf. This happens in the oak, and the tap-root is nourished, not by the plumule, but by the fleshy cotyledon, which also supplies nutriment for the growth of the stem. In this way the first green foliage leaves are borne well up in the air and raised above the humus, dead leaves, etc., which might have buried the seed. Mr. Henslow observes that the seeds of aquatic endogens are generally exalbuminous, but as the hypocotyl or hypocotyl and cotyledon are very much enlarged, there seems no more reason for starvation of the primary root than in the case, for instance, of the oak.

In the same section it is remarked that the arrest, presumably as the result of an aquatic habit, is not confined to the primary root in the Nymphæaceæ (where, by the way, there is a good stock of albumen in the seed), but is continued into the first-formed leaves which pass through various stages of development. Now, if Mr. Henslow will refer again to the work on "Seedlings," he will find that this gradation is quite the rule where the adult form of the leaf is not a fairly simple one. See, for instance, the figure of *Clematis* on p. 82, where the mature, much-divided form is only gradually assumed.

Attention is also drawn to the agreement between these and occasionally other aquatic exogenous orders and endogens in several points of anatomy. Such are the scattered closed bundles and the large wood vessels which are probably "due to the particular degenerating effect of a watery habitat." As the author himself observes, such appearances are by no means confined to water plants among dicotyledons. Any habit which allowed a relaxation in the effort for self-support, such as that of climbing, might, and in fact does, have a very similar effect. It is pointed out that the arrangement in *Peperomia* is practically identical with that of *Commelina*. Analogous degradations occur in parasites, *Cynomorium coccineum* and *Helosis guyanensis* having the bundles "scattered through the ground-tissue, exactly like any endogenous stem."

As regards the comparison of the foliage of *Victoria regia* and *Sagittaria*, considering the close relation in form between the adult leaves, *orbicular-peltate* and *sagittate* respectively, it is not remarkable that the stages by which the normal form is reached should be similar.

From its resemblance to cotyledons "the prevailing ribbon-like form of submerged leaves of endogens," seen also in *Hippuris* and *Callitriche*, is regarded as of a more "embryonic" character than the dissected type of submerged leaves of exogens, and the conclusion is drawn "that those terrestrial endogens which still retain a linear form (*e.g.*, grasses), or ensiform (Iris), or other similar type of leaf or rather phyllode, may be regarded as representing the ancestral submerged ribbon-like form"; while those with distinct lanceolate or cordate blades, like *Convallaria* or *Maianthemum*, "represent the floating types of existing aquatic endogens." The sagittate form seen in many aroids has primarily arisen like the same forms in Nymphæaceæ and *Sagittaria* from an aquatic habit. This reasoning seems based on somewhat superficial characters. We find classed together as representing one type leaves so totally different as the linear grass leaf and the ensiform one of the Iris. The former has a well-marked dorsi-ventral structure and a clear differentiation into distinct sheath and blade, separated by the characteristic ligule. Moreover, anatomically, the blade is that of a normal aërial leaf. The Iris, on the other hand, has equally well-marked isobilateral structure and properties. It is dangerous to draw conclusions from similarity in shape. Is the tendency towards a cordate form in the geranium leaf the result of a former floating habit, and the still more divided outline in some species evidence of a former submerged state? The hypothesis of the origin of the aloëidal type of leaf from an ancestral form like *Stratiotes aloides* seems as gratuitous.

In the section headed "Venation," the author again attempts to show that the terrestrial leaves of endogens are primarily aquatic, but have taken on aërial features, such as reticulations, stomata, etc., the idea being that a parallel or curvinerved venation is an aquatic adaptation.

The facility of tearing seen in the submerged leaves of *Nymphæa*, in those of bananas, palms, and blades of wheat, is referred to an enfeebled structure primarily due to a watery life. One can, however, see a nearer cause in the case of leaves like the banana and palm, which are not at once shed from the stem, but gradually droop. This process will be aided by the tearing, and they will occupy less space, and cause less drag on the stem, than if they remained entire. We think, too, that a better explanation of the holes in *Monstera* leaves might be found than descent from an aquatic fenestrated leaf like that of *Ouviranda*.

Finally, as regards the flower of endogens, the author suggests that the local stimulations set up by insects may equalise, if not surpass, the degrading influences of an aquatic medium. Presumably, this has happened in three important aquatic orders—Hydrocharideæ, Pontederiaceæ, and Alismaceæ—where the flowers are certainly conspicuous. As, however, a large number of genera of Naiadaceæ, Typhaceæ, Juncaceæ, Cyperaceæ, and others are greatly degraded, it is claimed that "the evidence from coincidences is accumulative, while comparative anatomy justifies the conclusion that there has been a distinct 'cause and effect' in the reduction of the floral structures."

The evidence given, however, does not seem particularly conclusive, comprising a derivation of the spike or spadix of Aroids, Typhaceæ, *Potamogeton*, and others from the branched inflorescence of Alismaceæ and Juncagineæ by suppression of the flower stalks and perianth whorls, and the reference of the quaternary arrangement of the floral organs to a reversion to a very primitive condition of an opposite and decussate arrangement. The suggestion is put forward in a foot-note that the use of the lodicules of grasses is to act as reservoirs of water; it is, however, not easy to see why water reservoirs should be wanted in a grass-flower.

Having briefly reviewed the evidence advanced in favour of the theory, we can only say that whatever has been the origin of monocotyledons, their origin from dicotyledons "by self-adaptation to an aquatic habit" does not seem to have been established in the paper before us. Mr. Henslow has instanced many points illustrating a community of descent of the two groups, and also shown that, in each, plants adapted to a watery habit have certain morphological and anatomical resemblances, as community of descent would in fact lead us to expect, but in our opinion he has not established the origin of one from the other. The study of the origin of great groups is a most fascinating one, but it is also extremely difficult, and one especially in which superficial resemblances must not be pushed too far.

A. B. Rendle.

VIII.

The Recapitulation Theory in Biology.

I HAVE read with interest the articles on the above theory by Dr. Hurst and Mr. Bather in NATURAL SCIENCE for March, April, and May. Mr. Bather states that my work has been inspired by the Recapitulation Theory; and Dr. Hurst is pleased to retort that this is a fantastic theory accepted as a creed by myself and others. Now I am not altogether sure that Mr. Bather is absolutely correct in speaking of my work as inspired by the Recapitulation Theory; what I have upheld is a "Law of Earlier Inheritance." That this law, of necessity, leads to a theory of recapitulation is no doubt true; but it is the law of Earlier Inheritance which has guided my work. Upon this law I have set forth my views, and have shown by comparisons of the ontogeny with the (say, assumed) phylogeny in Ammonites that recapitulation is and can only be imperfect.[1] As these views are fully detailed in a paper on "Some Laws of Heredity,"[2] I need not go over that ground again. Dr. Hurst will find that I have fully taken into account the similarity of embryos of dissimilar species—which seems to be a stumbling block to his acceptance of Recapitulation.

I will assume that Dr. Hurst has not read this paper, or any notice of it; in which case it is curious that, approaching the subject from a totally different point of view, he has defined variation in nearly similar, though wider, terms to what I used some months before.[3]

So far I will suppose that Dr. Hurst and myself are practically in agreement; but it is useless to discuss a question of this kind unless the one knows exactly what the other requires. My object, therefore, is to point out where it seems necessary for Dr. Hurst to amend his statement.

In page 198 Dr. Hurst denies any *causal* relation between ontogeny and phylogeny; in page 197 he says, "the more the adult structure comes to be unlike the adult structure of the ancestors, the more do the late stages of development undergo a modification of the same kind." By "late stages of development" Dr. Hurst means, I presume, premature development—a point I will notice later. Now, as it stands, the sentence conveys to the mind the idea that the premature stages of an individual become altered in consequence of alteration to the mature stages, which are not formed till later. This

[1] Inf. Ool. Ammonites (*Pal. Soc.*, 1892), part vi., p. 288, footnote 2.

[2] Proceedings Cotteswold N. Club, vol. x., part iii., p. 258, Oct., 1892. Translated into German, forming vol. xviii., series i., of *Darwinistische Schriften*, March, 1893.

[3] *Op. cit.*, p. 261.

is clearly an impossibility—any changes in the adult man of 30 cannot affect his structure when he was a boy or child; nor do I imagine that Dr. Hurst means this, though his sentence seems to imply it. If, however, he wishes to say that the more the adult man comes to be unlike his ancestors the more do the premature stages of his descendants undergo a modification of the same "kind," he has obviously admitted the causal relation between ontogeny and phylogeny which he wishes to deny.

There is another sentence in the same page which puzzles me entirely—"in order that any structure of the adult which varies, and hence ceases to exist as an adult structure at all." The word "structure" seems to be open to objection. I presume Dr. Hurst means "character," but then I do not understand the "hence," unless Dr. Hurst wishes to say "and hence ceases to exist *solely* as an adult structure." An adult "structure" may vary in the direction of becoming greater or less, and yet may remain an adult "structure," but it will not remain solely an adult "structure." By the law of earlier inheritance illustrated in Mr. Bather's table (p. 279), it gradually becomes an adolescent "structure." Thus, if the adult "structure" be x, and it varies in subsequent generations gradually to become $2x$, the adolescent "structure" will by then be x; and as in subsequent generations adult $2x$ becomes $3x$, the adolescent x becomes $2x$. Hereby we obtain a record; yet Dr. Hurst says that in order to produce a record "as the adult structure varies in one direction, the late stages of development must vary in another direction." The question is, then, what does Dr. Hurst mean by "late stages of development"? and, also, what meaning ought to be attached to the phrase? The dictionary explains development as "the series of changes in the growth from first to last of an organised being." Obviously, growth includes not only increase as a whole, which in some organisms is continuous throughout life, but it also includes increase in any part. Thus, the growth of hair on man's chest in middle life is part of his development, and the expansion of the stomach in senility is also part of his development. I contend that the late stages of development are the stages of senility; and that to give the term any other meaning is a mistake, which, I am aware, is not confined to Dr. Hurst.

Further, I contend that loss of teeth and loss of hair in man are part of his development—though they are strictly retrogressive phases of development. Then under these circumstances I must ask Dr. Hurst what he regards as acceleration of development, because he probably uses the term in a different sense to that in which I have been accustomed to see it employed.

Lastly, in regard to development, I would remark that, in any discussion, it is most important to clearly distinguish the stages of development in ontogeny and the stages of development in phylogeny; for what Dr. Hurst has written fails to convince me that there is not a close connection between the two.

S. S. Buckman.

IX.

Professor Blake "On the Bases of the Classification of Ammonites."[1]

THE above is the title of the Presidential Address to the Geologists' Association. No wonder that the members of that association were pleased. Professor Blake is just the man for the task, he is "one of the best geological critics in England"—at least, so we have been told, and we are frequently reminded of the fact; he has studied the Cephalopoda, and he has written upon them, and treated them from the mathematical point of view; he is a member of the Council of the Geological Society, and he gave his address as president of the Geologists' Association. No wonder that the members of that association, who were so thankful "for his concise account of one of the most difficult groups of animals—the Cephalopoda"—published last year, should be glad of enlightenment concerning the classification of Ammonites.

Hitherto the members of the association as a whole have been somewhat at sea about Ammonites. It was a dangerous experiment for one of them to air any of the new (?) generic names in the hearing of his fellows. The powers that be, so he was promptly told, still clung to the good old genus *Ammonites*, with its six or seven thousand species; and new generic names were tabooed. No division of this grand old genus could be allowed, even though Professor Blake had, nearly twenty years ago, shown what could be done in the generic division of Ammonites. At last Professor Blake thought the time had come to rescue the members of the Geologists' Association from outer darkness, and accordingly he has once more bravely tackled the question of these new generic names.

According to Professor Blake the chief bases of Ammonite-classification are four—the form, the size (!), the ornaments, the sutures. He mentions other characters, for instance, the length of the body-chamber, only to dismiss it with the extraordinary remark that "if the interior part of the whorl be occupied by the previous whorl, the loss of space is apt to be made up for by the length of the last chamber; but no general rule can be laid down." We should think not, except

[1] ON THE BASES OF THE CLASSIFICATION OF AMMONITES. Presidential Address. Proc. Geologists' Association, vol. xiii., part 2, pp. 24–40, 1893.

it was to state exactly the opposite. How closely Professor Blake has studied recent literature, to find that the length of the body-chamber is considered of the least value among present-day authorities! We have not been able to do so; but we do not wonder at it, because, in our opinion, to consider the *length* of the body-chamber only is absurd. What is required is the *capacity* of the body-chamber. In an evolute, divergent-sided Ammonite the length of the body-chamber would be short, but its capacity might be exactly the same as in an evolute, compressed Ammonite with a body-chamber three times the length. The capacity of the body-chamber, in proportion to the whole shell, may become a basis for classification—its mere length never can be.

The form of the Ammonite is stated to depend on the curvature (p. 25). Alluding to the fact that he published a mathematical paper on the subject, Professor Blake proceeds to explain how to measure the curvature. He gives us three methods. He first "assumes that the rate of curvature of the inner edge is the same as that of the outer." But of what use is this method among Ammonites? The rate of curvature of the inner edge must always be greater than that of the outer edge which subtends it, otherwise Ammonites would very soon fulfil the Yankee's definition of a point—"the small end of nothing whittled down." Nevertheless, our mathematician proceeds to use this wonderful method, and finds thereby that there is considerably less curvature in an involute Ammonite than in an open-whorled *Crioceras*! Now if we take his second method and apply it to the very figures he has given us—the curvatures of the involute Ammonite and of the open-whorled *Crioceras*, work out the same within 0·01.

In the next paragraph we are introduced to the wonderful "*Ammonites transmogrificabilis*, Blake;" and the Professor sets himself to contradict what he had stated in the paragraph above. As a result of his measurements he found that an involute Ammonite had a smaller curve than an open-whorled *Crioceras*; here he tells us that "*without altering the curvature*"—the italics are ours—he can make at will an involute Ammonite or an open-whorled *Crioceras* by means of some wonderful diagrams (pl. i., figs. 3, 4, 5). When, however, we come to apply to these figures his first method of estimating the curves—the method he himself thought fit to employ for figs. 1, 2, pl. i.—we find he has altered the curvature very greatly, for the ratios are fig. 3, 1·53; fig. 4, 1·44; fig. 5, 1·25; and "the greater the ratio the less the curvature" (p. 26).

Now, we confess to little mathematical faculty ourselves. To us a sum is like a joke to the proverbial Scotchman—only to be done *weeth deefeculty*; but even our inferior faculty would have suggested that to estimate the curve of an Ammonite-shell it was necessary to consider not only the peripheral, but also the antiperipheral, or inner-marginal curve. There is no difficulty in seeing that the inner-marginal curve in fig. 4 is greater than in fig. 5, and that in fig. 3 greater than in fig. 4. We should have supposed that to estimate the curves of these shells it was necessary to take the mean of the peripheral and anti-peripheral curves—by which method, of course, fig. 3 is curved the most.

Professor Blake takes all this trouble about the curves to prove, first, that "an involute Ammonite *(Phylloceras)* has *more curvature* than an open-whorled *Crioceras*"; secondly, that "the difference between the form of *Phylloceras*, *Arietites* and *Crioceras* has *nothing whatever* to do with their curvature,"—two statements which do Professor Blake great credit, as they do not contradict each other in the least!

In dealing with the involution, Professor Blake makes the remarkable statement that Ammonites are necessarily more evolute in youth than in later life. *Am. salisburgensis*, Hauer; *Amm. gonionotum* and *fallax*, Benecke; *Amm. polymorphus* and *dimorphus*, d'Orbigny; *Am. polymerus*, Waagen; *Am. brocchi*, Sowerby; and the majority of the *Stephanocerata* support this view, we suppose. Really Professor Blake's perception must be keen; he sees what no one else could do in the matter of these Ammonites. Then he proceeds to say that Hyatt and Buckman seek an evolute ancestor for every form, and finds fault with them in consequence. How confidently the members of the Geologists' Association may rely on Professor Blake's accuracy! Hyatt ("Genesis of the Arietidæ") gives an *involute* form like *lævigatus* as the ancestor of certain species; Buckman ("Monograph," p. 283) places an *involute* shell like *globosus* as the ancestor of the Polymorphidæ, and also (p. 289) states that the first stage of Ammonite ontogeny discloses a form like *globosus*, such, therefore, is the ancestral form he would seek.

In connection with the thickness (p. 28), Professor Blake announces a discovery which is new to us: "the whorls of an Ammonite become naturally more compressed with growth." What a number of unnatural Ammonites there must be! For instance: *Amm. bechii*, Sowerby; *spinatus*, Brug.; *sauzeanus*, d'Orbigny; *semicostatus* (Young), Hyatt; *adelæ*, d'Orbigny; *francisci* (Oppel), Vacek; *sutherlandæ*, d'Orbigny; *goliathus*, d'Orbigny; not to mention many others which change from less compressed to more compressed whorls, and again to less compressed in infancy or adolescence, and change again to more compressed in adolescence or maturity. We should have thought that the only law which could be laid down safely was that, in anagenesis, the whorls tend to become more than proportionally inflated, and, therefore, less compression is likely to arise; while in catagenesis the whorls tend to be less inflated, and greater compression may result. Unfortunately, the law is not so simple as this. Catagenesis may produce a less compressed whorl, because the reduction, in proportionate capacity, may be attained by reducing the breadth and not the thickness of the whorl.

The size of the Ammonite is given as one of the bases of classification! The size! We congratulate Professor Blake on this discovery. We believe it to be entirely his own, and entirely original, and we will not say a word to destroy the charming simplicity of the idea. Incidentally, we are treated to a most interesting piece of information about "*Ammonites planorbis*, now called *Psiloceras*" —"the fry are abundant, but the race was really gigantic, as may be seen by the specimen, over three feet in diameter, in the British Museum."

It would be strange if true; but, happily, this giant is not a specimen of "*Ammonites planorbis*, now called *Psiloceras*"; it is a senile example of *Ammonites conybearii*, now called *Vermiceras*, which not uncommonly attained the size of three feet across. It was not a contemporary of *planorbis* at all; but it lived much later, namely, in the time of *Pentacrinus tuberculatus*. That the label is incorrect in the British Museum implies no fault to anyone connected therewith. For one man to arrange the whole of the Cephalopoda—a subject which engages the attention of many specialists—is a too gigantic task; and accuracy in all cases cannot, therefore, be expected. That the public who take all the labels in the British Museum for gospel are thereby misled, is a pity; but while an overburdened museum-curator might successfully plead excuses for an incorrect label, no such plea can be allowed to such an authority on Cephalopoda as Professor Blake.

To illustrate the confusion which arises in the mind of the student —not the confusion which arises in his own mind on a subject about which he poses as a teacher—Professor Blake compares (p. 29) the generic grouping given by Buckman and by Futtlerer (*sic*), and remarks what "most observers" would do in such cases. How useful to appeal to "most observers"! "Most observers" would not make egregious mistakes in classification in any science. Does Professor Blake wish us to believe *vox populi vox Dei*; and does he forget that it was not only Carlyle who uttered a sentiment akin to *vox populi vox stultorum*? When the immortal Mr. Pickwick went to the election and shouted "Slumkey for ever!" he was asked, "Who is Slumkey?" He replied, "I don't know; it's always best on these occasions to do what the mob do." "But suppose there are two mobs," said Mr. Snodgrass. "Shout with the largest," said Mr. Pickwick. It will always be a case of the specialists against οι πολλοι. If Professor Blake wishes to emulate the immortal Mr. Pickwick, and shout with οι πολλοι, he is quite free to do so. It is to be hoped that when asked his reasons he will be equally candid, and reply "I don't know."

"Most observers," says Professor Blake, "would place the former (species put by Buckman into two genera) not only in the same genus, but almost in the same species, and the latter (Futterer's), not only in distinct genera, but in distinct families." We don't doubt it for a minute; and thereby the observers would show their profound knowledge of ontogenetic and septal details in both cases! If a schoolboy were to class a whale as a fish, Professor Blake would be down on him for his ignorance; but the blunder in a schoolboy would be more excusable than are such remarks about Ammonites from Professor Blake.

We have given so much time to the criticisms of these various matters, that we cannot allude to numerous statements in this extraordinary paper; but we must not pass the cream of the whole article. "By comparing the figures [in the plate] . . . we seem to see how the *Stephanocerata* (pl. i., fig. 11) may have arisen from *Hammatoceras* (pl. i., fig. 10); and by comparing this with pl. ix., fig. 1, of Vacek's work almost from *Harpoceras*. In another direction we can pass towards *Perisphinctes* (pl. i., fig. 13), and in a third towards *Aspidoceras* (pl. i., fig. 12), and there is little to distinguish them in the outlines"! What skill and study must have been expended to construct such a complicated genealogy as this! We venture to say this effort has never been surpassed, not even by a would-be populariser of Science.

The *Stephanocerata* (pl. i., fig. 11)—an *inflated* shell with a spinous centre and costate outer whorl—"may have arisen from *Hammatoceras* (pl. i., fig. 10)" which is a *compressed*, wholly costate form, and has not the ghost of a spine about it, apparently; and this, "almost from *Harpoceras*" (Vacek, ix., 1) a *less* compressed fossil with a *smaller* umbilicus—evolute in youth but more involute with age! "In another direction we can pass towards *Perisphinctes* (pl. i., fig. 13)"—by a species which has remarkable and very distinct septa, namely, a peculiarly abbreviated siphonal lobe, and a lateral lobe specialised for a very particular purpose—features altogether unknown in *Perisphinctes*. "And in a third towards *Aspidoceras* (pl. i., fig. 12)"—except for septal details.

Of ontogeny Professor Blake takes not the smallest account. He writes a paper on the bases of the classification of Ammonites, and never so much as mentions ontogeny, except to admit the similarity of ontogeny to phylogeny as a guide in genealogy. Does Professor Blake know what these words mean? and has he applied to this genealogy a principle which he has utterly ignored in his paper? According to this genealogy, the facts of evolution are not given clearly enough nowadays; and we have always applied them the wrong way about. Obviously, we have to learn that the details of embryology show whither a species is going, and the details of senility which illustrate whence a species has come!

We are indeed grateful to Professor Blake and his genealogical table for giving us such information as this, and for showing us how to use it. We shall be able to correct the misapprehensions in regard to evolution under which we have hitherto laboured. For the future we can construct a genealogy like this:—Here is a monkey who grows less hairy in his old age—he shows how monkeys may have come from man; here is a man who is losing his teeth in old age; he shows how man may have come "almost" from a whale. A fish and a whale are very much alike, "at least, there is little to distinguish them in the outlines"—in another direction, a whale may have developed into a fish. To anyone acquainted with Ammonites, remarks like these are every bit as intelligent as those of Professor Blake; but while their value can be gauged almost by boys in the schoolroom, the pity of it is that so few people are able to properly appreciate the remarks of the ex-President of the Geologists' Association.

One last sentence we will notice in this strange address. "The modern genera of Ammonites . . . do not range much beyond the ancient idea of species" (p. 38). Certainly, the following does not represent the modern idea of species. "The family of shells allied to this species is so very variable that there is no mean between naming every minute difference . . . and throwing them into groups. I have chosen the latter method, and unite all those which may be called inflated S. *commune* under the present name, and those that stand in the same relation to S. *annulatum* under the next. They seem to develope (*sic*) tubercles indiscriminately . . . , etc."—("Geology of Yorkshire (Cephalopoda)," p. 300.)

This, of course, is the ancient idea of species—date, 1874; but the old masters in Science had the merely paltry modern ideas concerning species—they actually did not "throw" forms into "groups," *vide* Sciences more ancient than Palæontology; and they took the trouble—even those who dealt with Ammonites—to observe differences in involution, costation, ornamentation, and septal details. It is a pity that they did; no doubt they would have saved themselves this trouble if they could have foreseen the "ancient idea of species."

In conclusion, then, we must express our conviction that an efficient paper on the Bases of the Classification of Ammonites yet remains to be written; and it will not be accomplished by a man who—whatever he may, at one time, have done among Cephalopods—has for some considerable time turned his attention to matters entirely different, and yet imagines that with a few weeks' study he can become master of the work of several years.

SOME NEW BOOKS.

A Dictionary of Birds. By Alfred Newton, assisted by Hans Gadow, with Contributions from R. Lydekker, C. S. Roy, and R. W. Shufeldt. Part I. 8vo, illustrated. London: A. & C. Black, 1893. Price 7s. 6d. nett.

The appearance of a general work on birds by an ornithologist of the long experience of Professor Newton may well be regarded as marking an epoch in the science of which it treats; and the foretaste afforded by the issue of this instalment leads us to believe that this opinion will not be belied when the whole volume is completed. Like certain

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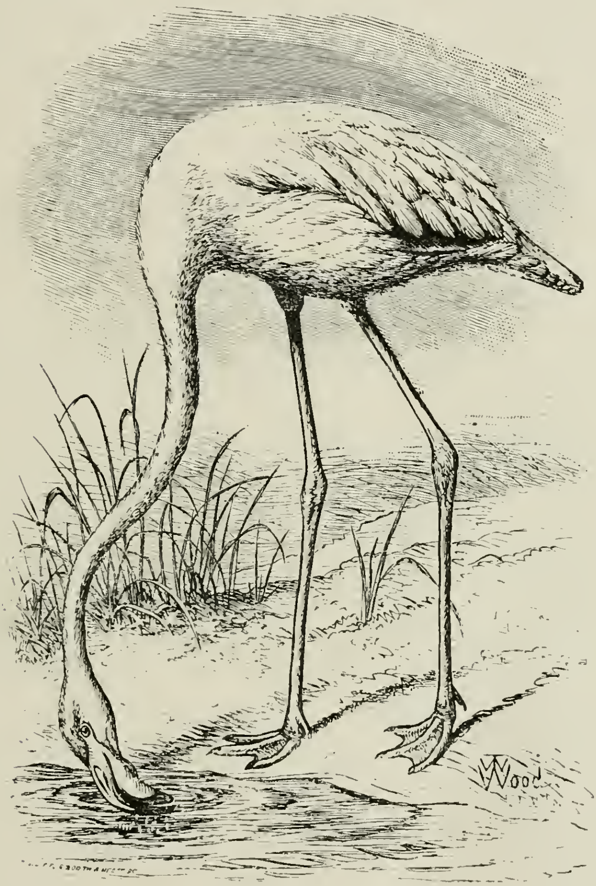
other zoological works issued by the same enterprising publishers, the manual before us is based upon a series of articles written by its author for the ninth edition of the "Encyclopædia Britannica"; but, we believe, the new matter is in this instance still greater than in the case of the other works. The present work differs, moreover, essentially from the others of the same series in that, as indicated by its title, it is in dictionary form. In deciding to treat his subject from the alphabetical rather than from the taxonomic standpoint, the learned professor, to use his own words, says that this plan has been adopted "because I entertain grave doubt of the validity of any systematic arrangement as yet put forth, some of the later attempts being in my opinion among the most fallacious, and a great deal worse than those they are intended to supersede. That in a few directions an approach to improvement has been made is not to be



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denied; but how far that approach goes is uncertain. I only see that mistakes are easily made, and I have no wish to mislead others by an assertion of knowledge which I know no one to possess." These are strong words, and we are glad to see them coming from an ornithologist of the long standing and well-deserved reputation of Professor Newton; more especially since we have had occasion elsewhere in this Journal to raise our own feeble protest against the succession of crude and ill-matured schemes of avian classification with which the ornithological world is flooded. Admitting, then, the wisdom of the author's choice in selecting the alphabetical form, we have no hesitation in saying that the plan and execution of the work are in every way worthy of the high reputation of its author; while the excellence of the illustrations, and the careful manner in which a complicated typography has been executed, reflect credit alike on author and publishers. In calling in the aid of such a master of avian anatomy and morphology as Dr. Gadow, for articles coming within his special province, the author shows that he does not pretend to omniscience even in his particular branch of zoology, and thus affords an example which might be followed with advantage by others. The other writers mentioned on the title-page have likewise contributed articles on certain branches of ornithology with which they are more especially acquainted; and, so far as this instalment permits of forming a judgment, the work thus seems as complete and authoritative as it could well be made. That its style, although necessarily condensed, is terse and elegant, all acquainted with the author's previous writings stand in no need of being informed; while its freedom from typographical errors affords a refreshing contrast to some works that have recently come under our notice. Indeed, we have hitherto detected but a single misprint (p. 281, line 2 from bottom), and that is the fault of one of his contributors rather than of the author himself.

Of the illustrations, two of which we are enabled to give as samples, some are entirely new, others are reduced from figures previously published, while others again have been borrowed directly from earlier ornithological writers; and we are glad to see Swainson's exquisite figures of beaks and heads once more brought prominently before the public.

In our limited space it would be impossible to give a detailed notice of any of the articles; and, indeed, if such space were available, we should be puzzled to know which to select for special commendation. We may mention, however, that each bird, or genus of birds, is generally described under its vernacular name—whether this be English or foreign—although cross references will be found under the scientific titles of the genera. Among the articles that have specially interested us, are those on the cassowary and emeu, which are admirable samples of what a brief semi-popular description of a bird and its habits should be; while the article headed "Extermination" cannot fail to arouse a general, if somewhat mournful, interest. We may, however, be permitted to ask the author why he states under the head of Emeu that this "is the only form of Ratite bird which naturally takes to the water, and examples have been seen voluntarily swimming a wide river." We do not for one moment venture to say that the statement may not be perfectly correct, but at the same time we should like to know what reason the author has for discrediting the following account of the habits of the Rhea (or Ostrich as it is there called) given in the "Voyage of the 'Beagle.'" Darwin there writes that "it is not generally known that ostriches take readily to the water. Mr. King informs me that at the Bay of San Blas and at Port Valdes, in Patagonia, he saw these birds swimming several times from island to island. They ran into the water both when driven down to a point, and likewise of their own accord when not frightened; the distance crossed was about two hundred yards. When swimming, very little of their bodies appear above water; their necks are extended a little forward, and their progress is slow. On two occasions I saw some ostriches swimming across the Santa Cruz river, where its course was about four hundred yards wide, and the stream rapid." Surely the statement of two independent observers requires refutation before it is authoritatively asserted that emeus are the only Ratites that naturally take to the water.

R. L.

DR. H. G. BRONN'S KLASSUNGEN UND ORDNUNGEN DES THIER-REICHS.—AVES. By Hans Gadow, Ph.D. IV. Abtheilung. 42 u. 43 lieferung.

THE anatomical portion of this most important work was completed some few months since, and we have now before us the first two numbers of the systematic part. Veritably they are children of promise, and, unless we are much mistaken, the sum of Dr. Gadow's labours will be two invaluable volumes, each the perfect complement of the other, beautifully symmetrical, nicely balanced.

In these two parts Dr. Gadow presents us with a pithy sketch, which practically embraces all that is worth knowing in the history of Systematic Ornithology. Beginning with an "Historical Survey," he tells us that only those systems have been noticed which have either advanced—or hindered—our knowledge, as well, also, those which, although possessing practically no influence to-day, yet are of interest, inasmuch as they serve by reflected light to illuminate preceding systems. This is surely a wise course, inasmuch as it will enable us to appreciate much that would otherwise of necessity have remained more or less obscure.

After briefly referring to the three great epochs in the history of avian classification, so ably demonstrated by Dr. Sharpe in his "Review of Recent Attempts to Classify Birds," the author has a few words to say anent the errors of yesterday, and the dangers of employing one particular character, after the fashion of patent medicines, to remedy all evils and solve all difficulties. Next follow some thirty-four examples of the most important systems of classification, beginning with that of Linnæus in 1735, and ending with Dr. Sharpe's in 1891. In every case we have appended a brief yet all-sufficient summary, giving the chief points of interest or importance. Of these the author has selected for special mention some dozen examples, Huxley, Garrod, Sclater, and Newton being amongst the number.

The classification of Linnæus is shown to have been based upon the work of our countrymen Willoughby and Ray, of whom we are justly proud. Be it noted that Dr. Gadow here awards them full credit for their labours, considering them the founders of "Systematic Ornithology." To Merrem belongs the honour of having founded the sub-classes Ratitæ and Carinatæ, which have received recognition in every system of importance up till now. Nitzsch, with his researches in the Carotias, Osteology and Pterylography, and Müller on the organ of voice in the Passeres, have placed us under an everlasting debt. With Huxley's classification, the author tells us, began a new epoch, and, indeed, his work is too well known to need comment here. To Garrod, Dr. Gadow points out, we owe much. The Homalogonatous and Anomalogonatous arrangement of the muscles of the leg, the Holorhinal and Schizorhinal condition of the nostrils, the patagial muscles of the wing, and the syringeal muscles, are all points upon which this indefatigable worker enriched our knowledge. Garrod attacked the vexed problems of Ornithological Classification with intense zeal and a very considerable amount of success, but, as we are remined, not entirely without error. Fürbringer's work is warmly praised, and, as the author points out, no ornithologist can proceed far without consulting his ponderous tomes. Dr. Sharpe's work, previously referred to, is, we think, deservedly considered an "important production," as all who have occasion to dabble in the mysteries of the complicated subject will allow.

As before stated, Dr. Sharpe's work is the last on the list—to our surprise, for we expected to find at least a reference to a most suggestive, if only tentative, scheme by Dr. Gadow himself, which appeared in the *Proceedings* of the Zoological Society for March, 1892; although it is more than probable that this will not be adopted in its entirety in the present work, yet we venture to think it should have been included; doubtless, however, the author has good reasons for its omission.

We come now to what might be called a diary of remarkable events in Systematic Ornithology—a sort of developmental history of classification. This retrospect (Rückblick) is arranged in a more or less tabular form, and is designed to show the more important changes of position which birds of doubtful affinity have been subjected to. To take an example at haphazard we quote the following: "Müller, 1847, *Cypelhus* removed from the Oscines, and placed with *Caprimulgus*." (First) recognition of the order *Oscines* ("Klärung der Oscines"). Some twenty-nine more or less well-known workers are quoted in this connection, beginning with Mohring in 1752, ending with Fürbringer in 1888.

Under the next heading, "Taxonomical Outlines," the author deals with the general principles of Taxonomy, and discusses in turn genealogical trees, that phenomenally arbitrary system the "Key," and the various organs used by systematists as "Characters" wherewith to differentiate various groups. Finally, we have a very valuable table of "Characters" used in classification, which are expressed chiefly by signs, for the sake of brevity and clearness. Seeing that these signs are carefully described in the text immediately preceding, there should be no difficulty in assimilating them. We would suggest, however, that eventually they should be published in one large sheet for use separately.

Dr. Gadow is to be congratulated on the decided "hit" he has made, and we are sure that we do but echo the sentiments of other workers in this field when we say that we are anxiously looking forward to the appearance of parts 44 and 45. Nevertheless, we will try to keep in view that very useful proverb, "Eile mit weile,"—a motto which Dr. Gadow wisely adopts. W. P. P.

Manual of Bacteriology for Practitioners and Students. By Dr. S. L. Schenk, Professor in the University of Vienna. Translated (with an appendix) by W. R. Dawson, B.A., M.D. (Dublin). 8vo. Pp. 310. London: Longmans and Co., 1893. Price 10s. nett.

This, the most recent text-book in English upon the new science, appears to be well translated and to merit a longer notice than is sometimes allowed to translations; for Dr. Dawson has added a valuable and timely appendix upon the mode of vaccination for cholera, a disease which has already paid us a visit this year—a visit which was, fortunately, brief; he has also included an account of Professor Marshall Ward's interesting observations on the influence of light upon bacteria, which have only just been published in the *Proceedings* of the Royal Society.

The best way of coping with many pathogenic bacteria appears to be to let daylight in upon them; it has been stated that a few minutes of direct sunlight are enough to kill the deadly bacillus of consumption, and that even diffused daylight is fatal in a longer or shorter time according to its intensity. Unfortunately, this mode of treatment is hardly applicable when the bacilli have got into the body; but they might be lessened in numbers, and the danger of infection thus diminished, by thorough ventilation of suspected localities. An open drain is neither pleasant to the eye nor agreeable to another sense which largely comes into play where bacteria abound, but it is from many points of view more healthy than a close drain, where, hidden from the sun's rays, the fellest varieties of bacteria can multiply unchecked. For practical purposes, too, any mode of dealing with typhoid is very important; it is an exceedingly common disease, and there must be few people who have not lost some friend or relative by its means; the germs of this complaint are killed by sunlight of four to seven hours' duration.

To those who have not followed recent bacteriological work, some of the figures given in this book will be surprising. One knew ever so many years ago that the large spirilla had a flagellum at each end by which they moved; now it is known that most kinds—even some of the most minute—are provided with a bunch of these vibratile processes; and this accounts, perhaps, for the rapid progress through an organ, or a system, as the case may be, of some infectious complaint; the typhoid bacilli from the great number of flagella which spring here and there from the organism are called "spider cells." As is well known, many different kinds of bacilli—alike enough on a superficial inspection—can be distinguished by their different reaction to staining fluids; and this important means of detecting a given bacillus is impressed upon the mind of the student in the work before us by the use of differently coloured figures in the text. We have, indeed, nothing but praise for the illustrations, which are not only good but abundant.

The word "Bacteriology" is used in the book before us in a sense rather wider than that usually applied to it, and the author says something about other kinds of unicellular parasites. There is a growing tendency to ascribe cancer to a Gregarine; and the important researches of Drs. Rüffer and Walker are justly quoted (by the translator) in this connection—researches which have since been confirmed upon the Continent. Gregarines are such abundant parasites in the lower animals (insects, earthworms, etc.), where their existence appears to be attended with such slight inconvenience to their hosts, that it is, perhaps, remarkable how extremely dangerous they are in the form of cancer in the human species. F. E. B.

On English Lagoons: Being an Account of the Voyage of Two Amateur Wherrymen on the Norfolk and Suffolk Rivers and Broads. By. P. H. Emerson. 8vo. Pp. 298. London: David Nutt, 1893. Price 7s. 6d.

We took up this volume with a feeling by no means favourable to the author, for of late years we have been overwhelmed with books on the Norfolk Broads. Mr. Emerson, however, has a far better claim than most to write on the subject, for he has lived the whole year round on the broads, and can describe them as they appear in a severe winter as well as during the ordinary season for visitors. One fault only we have to find. His pictures of the broads, though striking enough to those who already know the district, will often scarcely be intelligible to strangers, for he has a habit of leaving out essential parts of the description—parts so familiar to the author and to the natives that they have ceased to be remembered.

Mr. Emerson is a good field naturalist, and his book is full of interesting notes on the habits of the birds, though there is nothing strikingly novel. Among the notes on the fish, there is a very curious one on a large take of eels at Geldeston Lock in December, after a very severe frost, and with the river full of ice and snow. As eels usually hybernate in winter, this was most unexpected, and we are glad that a naturalist like Mr. Emerson was on the spot and can vouch for the fact. Can this sudden appearance of eels have been connected with the formation of anchor ice, which disturbed the eels in the muddy bottom in which they had been lying? The formation of anchor ice is not a common phenomenon in Norfolk; but the severe frost with clear sky which the author recorded a few days before ought to have caused its appearance. Mr. Emerson makes also a good many remarks on the manners and customs of the natives. He seems, however, to be much more able to appreciate the good points of a fish or of a bird than of a human being; our own experience has not led us to consider the Norfolk people so utterly boorish as he pictures them.

Manuel de Géographie Botanique. Par Oscar Drude, traduit par Georges Poirault et revu et augmenté par l'auteur. Livraison I. Paris: P. Klincksieck, 1893. Price fr. 1.25.

There are so many who find French easier reading than German that a translation in the former language of Drude's important book will be very generally welcomed. The value of the present work is, moreover, enhanced by the fact that it is reviewed and augmented by the author so that it should be in every way up to date, a matter of importance when dealing with geographical distribution, a subject in which new series of observations on plant biology, or the better understanding of the flora of a region and its relations to neighbouring ones, may necessitate a reconsideration of hitherto accepted theories.

According to the advertisement, the complete work will form a volume of about 500 pages (8vo), and will include 4 coloured maps and 3 figures in the text. It will be published in 12 or 13 parts, each containing two or three sheets of text. We are glad to note that the parts will follow each other rapidly. The price of each part is 1 fr. 25, the subscription to the complete work, payable in advance, 15 francs.

Part I. contains 48 pages, and is clearly printed on good paper. It comprises an introductory definition of the subject, with a brief account of its relation to geology and physical geography, and also a portion of the first part of the book dealing with the influence of external agents on the plant organism. Under geographical agents are considered the influence of duration and intensity of light, of heat, cold and dryness; acclimatisation is briefly disposed of, and a section on phenology follows. Next, under topographical agents, the influence of the soil is discussed. We shall await with interest the succeeding parts of what promises to be a valuable work.

The Genus Masdevallia. Issued by the Marquess of Lothian. Plates and descriptions by Miss Florence H. Woolward. Part IV. London: R. H. Porter, 1893. Price £1 10s.

Masdevallias form a favourite genus with orchid growers, and are, besides, of great interest from a botanical point of view. They are confined to tropical America, inhabiting chiefly the damp higher regions of the Andes, but ranging up into Central America and southwards as far as Rio Janeiro. Though, speaking systematically, a good genus, it includes species widely different in appearance. Contrast, for instance, the brilliant *Masdevallia Harryana*, one of the most common in cultivation, with its broad-spreading perianth limbs, with the more sombre-coloured *M. coriacea* and it allies, where the perianth takes the form of an open fleshy cup; or, on the other hand, with the gorgeous *M. bella*, where the lip, no longer inconspicuous or hid within the tube, forms in its shell-like beauty a striking characteristic of the flower. The range of colour is as wide as that of form. Now a plain scarlet or orange, or, alas! even magenta, an almost pure yellow, or, but rarely, white; now, on the other hand, a rich blending of citron and crimson lake as a background to the pure white lip. To all of these Miss Woolward does ample justice in the beautiful plates which accompany and interpret the descriptions of every species.

Much of the material for the work is from Lord Lothian's own collection of orchids at Newbattle Abbey; but Miss Woolward has also had the advantage of specimens, both living and dried, from orchid growers and in herbaria, for purposes of comparison and description. The carefully-drawn floral dissections on each plate will be invaluable to the botanist. Consul Lehmann (German Consul in the Republic of Colombia) has supplied interesting notes on the habitat of those species with which he is acquainted in their native homes. The present number also includes a species from Brazil, which has not yet reached Europe, but drawings and descriptions of which have been supplied by Professor Rodriguez, the Director of the Gardens at Rio Janeiro. In the same issue a new species is described, a striking plant near *M. elephanticeps*, and apparently imported by Mr. Bull mixed with the latter plant, as it was purchased from him under that name.

Part IV., like its three predecessors, contains ten species, bringing the whole number up to forty, and we understand that Part V. is well on the way.

Illustrations of the Typical Specimens of Lepidoptera Heterocera in the Collection of the British Museum. Part IX. The Macrolepidoptera Heterocera of Ceylon. By George Francis Hampson. 4to. Pp. vi., 182, pls. clvii.–clxxvi. London: Published by the Trustees of the British Museum, 1893.

This ninth part of the British Museum Catalogue is devoted to the Macrolepidoptera Heterocera of Ceylon, and consists of a "General Systematic List," followed by "descriptions of species figured," containing a formidable number of new genera. There is an interesting illustration of protective mimicry shown by the caterpillar of *Cornibœna biplagiata*, in which the sides of each somite are produced into fleshy processes, upon which the larva fastens small pieces of withered leaves and sticks as a disguise. It may be as well to mention that this information occurs on p. 145, for as is usual in this series of volumes there is no index, and one has to hunt through page after page of text to find the details required.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Miss Maria M. Ogilvie, B.Sc., well-known for her researches in the geology of the Tyrolese Alps, has been promoted to the degree of D.Sc. in the University of London.

Dr. William H. Howell, of the Harvard Medical School, has been appointed Professor of Physiology in Johns Hopkins University; and the vacant chair of Anatomy in the same University has been accepted by Dr. Franklin P. Mall, late of Chicago.

Dr. Spyridon Miliarakis, curator at the Athens Botanical Museum, has been appointed Professor of Botany at the University; and Dr. Ed. Fischer has been appointed extraordinary Professor of Botany at the University of Berne.

Dr. U. Dammer and Dr. P. Hennings have been appointed assistant-curator and curator respectively in the Berlin Botanic Garden, and Dr. M. Gurke a curator in the Botanic Museum.

Mr. F. W. Gamble, B.Sc., has been appointed Assistant Lecturer and Demonstrator of Zoology in the Owens College, Manchester. Mr. H. B. Pollard, M.A. (Oxon), has been elected to a Berkeley Research Fellowship in Zoology in the same College.

Dr. Giessler, assistant at the Botanical Institute of the Jena University, has been appointed assistant at the Botanical Museum and Garden of the Göttingen University in the place of Dr. Hallier, who, as noticed in a former number of Natural Science, has become an assistant at the Buitenzorg Garden in Java. At the same University (Göttingen), Dr. Dreyer, from St. Galle, takes the place of Dr. Alfred Koch as assistant in the Institute of Plant Physiology.

We regret to announce the retirement, on account of failing health, of Sir J. William Dawson from the Principalship of McGill College, Montreal, an office he has held for nearly forty years. While relieved of administrative duties, Sir William hopes to be able to pursue with greater success his long-continued researches in geology and palæontology.

The Royal Saxon Mining Academy in Freiberg has had attached to it since 1767 a "Mineralien-Niederlage," or commercial branch for the supply of specimens. This is situated in the main building of the academy, and as a public office has been under the supervision of the director. We have received what is to be the first price list issued, written in remarkably good English, and showing that the mineral productions of Saxony can be supplied at most reasonable terms. Both A. G. Werner and A. Breithaupt have been the managers of this concern. Special stress is laid on the necessity of addressing letters FreibErg, Saxony.

The Essex County Council Technical Instruction Committee has issued a prospectus of lectures and classes in Chemistry and Biology. The course of instruction is graduated in an interesting manner, there being first "pioneer lectures" on subjects of every-day life, then short introductory series of a popular character, and subsequently systematic and practical courses. There are also "County Normal Classes" for the instruction of advanced students as teachers. Mr. David Houston is Staff-Instructor of Biology, with Mr. Wilfred Mark Webb as Demonstrator. Mr. J. T. Cunningham, of the Marine Biological Laboratory, Plymouth, has also arranged to commence lectures in October on the Natural History of marketable sea-fishes and oysters.

We have received the first part of vol. iii. (new series) of the *Journal of the Marine Biological Association of the United Kingdom.* In the report of the Director, Mr. Edward G. Bles, reference is made to a change in the regulations by which the shortest term for the renting of a table at the Plymouth Laboratory is reduced from one month to one week. This should induce investigators to take any opportunity which may occur at any time of the year to collect seasonable material, or to study living animals and plants available for the time being. Among the papers there are two important contributions to the fauna of the English Channel, one on Nemertines, by T. H. Riches, and another on Turbellarians, by F. W. Gamble. Mr. J. T. Cunningham discusses the "Immature Fish Question," and the colouration of the skin in flat-fishes. Dr. W. B. Benham gives a beautifully illustrated description of the post-larval stage of *Arenicola marina*; and there are two reports of economic interest by E. W. L. Holt and W. L. Calderwood.

The annual report of the American Museum of Natural History, New York, for 1892, has lately been issued. The opening of the Museum on Sunday afternoons appears to have proved a success; and the popular lectures, arranged with the aid of the professors of Columbia College, are highly appreciated by the public.

The Department of Botany of the British Museum has purchased the great collection of Diatoms made by Mr. Julien Deby, estimated to consist of about 30,000 slides. Although the Museum previously possessed a magnificent series, including Greville's, William Smith's, Gregory's, O'Meara's, Ralf's collections, etc., there is singularly little duplicate material in the Deby Collection, which was, before acquisition, the only rival of the Museum collection in importance. It has been handed over in excellent order, fully indexed and accessible to students.

The annual meeting of the Palæontographical Society was held on June 23, when a satisfactory report of progress was presented. Only two non-members of Council, however, attended. The Right Hon. Professor Huxley was elected President in succession to the late Sir Richard Owen.

The question of the admission of Women as Fellows was decided in the negative by the Royal Geographical Society, at a special general meeting held on July 3. The voting was 172 against, 158 for. The result is disappointing, but is what might have been expected under the circumstances.

Next year the Geological Society of London publishes the fiftieth volume of its Quarterly Journal. It has been decided to mark the event by preparing an index to the whole series, which will probably be published early in 1895.

THE First Report of the Scientific Society of University College of Wales, for session 1892–93, has just appeared. It is a modest little pamphlet of 12 pp. and contains besides business matters a tentative list of the Flora and Fauna of the district around Aberystwith. Since the formation of the Society, Oct. 20, 1892, the members have made eight excursions, chiefly to places of geological interest, their enthusiasm even taking them afield in November and December.

THE thirteenth report of the Manchester Microscopical Society is prefaced by an excellent portrait of the President, Professor Milnes Marshall. There are two readable presidential addresses, the one on "Death," the other on "Recent Embryological Investigations"; and Professor Weiss briefly discusses Symbiosis in plants, Mr. P. Cameron contributes part ii. of his description of the galls of Mid-Cheshire. with two plates; and Messrs. W. Moss and F. Paulden give an illustrated account of the reproductive organs of *Bulimus acutus*.

THE Guernsey Society of Natural Science and Local Research has completed the first decade of its existence, and the new part of its *Report and Transactions* contains several valuable papers. Mr. E. D. Marquand supplements his list of the flowering plants of Guernsey by an account of the mosses, hepaticæ, and lichens, and it is noteworthy how many species common in Devon and Cornwall appear to be absent in this island. Mr. W. A. Luff publishes a list of the Hemiptera-Homoptera, the first of the kind relating to any of the Channel Islands. Mr. G. Derrick describes the Guernsey clays, and in another geological paper Mr. A. Collenette considers that the raised beaches and associated phenomena prove Guernsey to have been completely submerged at a recent period. A brief account of the rocks of Alderney is also given by Mr. C. G. De La Mare.

CIRCULARS dated June 10 inform us that "The World's Congress of Geologists will meet in Chicago" immediately after the meeting of the American Association in Madison. The Geological Society of America will also hold its fifth summer meeting on the 15th and 16th of August at Madison. The Excursions proposed for the Geological Society of America will comprise the Lake Superior Region, the Dells of the Wisconsin, and the driftless area in the vicinity of Madison. Messrs. Wadsworth, Van Hise, Chamberlin, and Salisbury will conduct the Excursions.

THE Geologists' Association of London called a Special General Meeting on July 7 and passed some important alterations in the rules. It was decided that the names of those proposed as members should, after being read out from the chair, be declared duly elected unless a ballot was demanded. It is hoped by this means to avoid much unpleasantness and expenditure of time, while opportunity is afforded of an appeal to the ballot, if any one of those proposed for election is considered ineligible. Another important change is the increase of the commutation fee or life subscription to seven pounds ten shillings, being fifteen years' purchase. This is in accordance with the practice of other Societies, it having been found that the low commutation fee has been a loss to the Association for several years past. The third important change is an expulsion clause. We trust the Association has not been compelled to adopt this in self-defence, but rather as a terror to possible evil-doers. The clause appears to us to be very cautiously worded, and will no doubt prove valuable if required. Ugly as these clauses are, they are necessary for the general welfare of the members.

THE 42nd meeting of the American Association for the Advancement of Science will be held in August at Madison, Wisconsin, under the presidency of William Harkness. The president of the Geological Section will be Mr. C. D. Walcott, who discourses on "Geologic time as indicated by the Sedimentary Rocks of North America"; of the Zoological Section, Professor H. F. Osborn, whose address is to be devoted to "The Rise of the Mammalia"; of Botany, Professor C. E. Bessey, who speaks on "Evolution and Classification." Dr. J. O. Dorsey, the president of the Anthropological Section, will read a paper on "The Biloxi Indians of Louisiana."

The next Congress of the French Association for the Advancement of Science will be held at Besançon from August 3 to 10. M. A. Magnin, professor at the Besançon Faculté des Sciences, will preside over the botanical section, and he suggests as a subject for discussion the question, already touched on, but in an incomplete manner, of the formation and conservation of botanical collections, also the relations of the Jura flora to the vegetation of the Alps.

The Selborne Society celebrated the centenary of the death of Gilbert White, June 26, by a visit to the Hampshire village, immortalised as the scene of the unassuming naturalist's labours. Gilbert's father was squire of Selborne at the time of his son's birth in 1720. Gilbert, the eldest of several sons, went to Oxford, where he graduated in 1744, gaining a Fellowship at Oriel. He lived some time at the University, serving for a year as Senior Proctor. In 1753 he took a curacy, but two years later we find him at his father's house at Selborne, where he stayed till 1758, when his father died. He then did duty in Faringdon, a neighbouring parish, returning in 1784 to Selborne, where he remained as curate till his death in 1793. The "Natural History of Selborne" appeared in 1789, only four years before his death.

We have received some circulars from Mr. Joseph Coe, of 33 All Saints' Street, Hull, from which we learn that an excursion was conducted on June 12, 1893, to Mablethorpe, by some of the more energetic naturalists of Lincolnshire, for the double purpose of investigating the natural history and geology of the district, and considering the question of founding a "Lincolnshire Naturalists' Union." Professor Miall, Rev. Canon Fowler, Messrs. F. M. Burton, Denison Roebuck, John Cordeaux, and others were present, Professor Miall taking the chair; a favourable decision was obtained, and those present at the excursion were formed into a temporary committee to draw up rules and elect officers at Lincoln on July 1. At the meeting of this committee the necessary formalities were proceeded with, and the first officers elected. Mr. Cordeaux is to be the first President; Messrs. W. F. Baber and Joseph Coe, Honorary Secretaries; while the sectional presidents are Mr. G. H. Caton-Haigh (Vertebrate Zoology); Rev. C. W. Whistler (Conchology); Rev. Canon Fowler (Entomology); Rev. W. Fowler (Botany); Mr. F. M. Burton (Geology) Mr. R. J. Ward (Micro-zoology and Micro-botany). Field meetings will be held twice a year, once in the northern and once in the southern division of the county; the subscription is 5s., but those subscribing 10s. 6d. will receive the proposed publications of the society.

The Museums' Association, which formed the subject of our first comment last month, met, by permission of the Zoological Society, at 3 Hanover Square on July 3. The retiring President, Professor Boyd Dawkins, was unavoidably absent, and the chair was taken by the new President, Professor Sir William Flower, who delivered an opening address. On Tuesday, July 4, Dr. Jonathan Hutchinson opened the proceedings by a paper on "Educational Museums," which was followed by an interesting discussion. Mr. Ruskin's views on Museums were expounded by Mr. W. White, and Mr. Newstead read some notes "On the use of Boric Acid as a preservative for Bird-skins." Visits were paid in the afternoon to the Natural History Museum, and the South Kensington Museum. On the Wednesday, Dr. Sclater discoursed "On the Typical Forms of Vertebrated Animals suitable for Exhibition," Mr. Platnauer "On the Arrangement of Insect Collections in Museums," and Mr. Newstead "On the Preparation of Coccidæ for Museums." The day concluded with a visit to the British Museum, and a Conversazione at the Royal College of Surgeons. The Royal Wedding on Thursday fortunately provided an excuse for open-air work, and the members visited the Zoological Gardens and Sir John Soane's Museum. Thursday and Friday were devoted to exhibitions of museum appliances, or visits to various smaller private collections, while Saturday was agreeably spent by a visit to Dr. Hutchinson's private museum at Haslemere, and a ramble over the Hind Head district.

The papers read at the meetings will be printed later on, but Sir William Flower's address is already to be found in *Nature* (July 6 and 13). He again emphasised the absurdity of the crowding of specimens, pointed out the value of good labelling, isolation of objects, suitable backgrounds, and necessity of space, and referred to the insignificant amount expended by Government on the development of museums; he also referred to the contemptible offers which had lately appeared in the newspapers for curators of local museums—a subject mentioned in our comments last month. After speaking on the general theory of museum arrangement, Sir William Flower gave a sketch of the museums at the Jardin des Plantes, Paris, at Vienna, and at Berlin, and noticed "the most original and theoretically perfect plan for a museum," explained to the British Association by General Pitt-Rivers in 1888, finally presenting a plan of his own, which contemplates a one-storied, top-lighted building, in which the front galleries would be devoted to the public exhibitions, a second series of galleries would contain the reserve collections, while a third set of rooms would be occupied by the officers and students. In such a place as this the absurdity of having to walk a quarter of a mile and climb several staircases to compare a fossil plant with a recent plant, or a mammoth with an elephant, as one has now to do in the British Museum (Natural History), could not exist, and the officer in charge would be in direct communication with his reserve and exhibited series, instead of being, as in many cases, far removed. Another point brought out in this plan is the necessity for a lecture theatre, and an arrangement in construction of the officers' workrooms by which isolation from the main building is effected so as to permit those who desire to do so to remain for work instead of having to leave at an appointed hour. We hope to hear more of this plan, for many students would heartily welcome some radical changes in the present arrangement and working facilities of most public museums.

OBITUARY.

FRANCIS POLKINGHORNE PASCOE.

Born September 1, 1813 — Died June 20, 1893.

A VETERAN naturalist, well-known for his researches in Entomology, has passed away in the person of Mr. F. P. Pascoe. Born at Penzance eighty years ago, Mr. Pascoe was trained for the medical profession, and was always imbued with a taste for Natural History and travel. In 1835 he became M.R.C.S., and shortly afterwards entered the Navy as assistant surgeon, visiting Australia, New Zealand, the West Indies and the Mediterranean. In 1843 he retired from the service, married, and resided in Cornwall until the death of his wife in 1851. Subsequently Mr. Pascoe removed to London, when he gradually formed the great entomological collection now being acquired by the trustees of the British Museum. His first original paper related to a botanical subject, but all subsequent contributions dealt with his chosen field of research in Entomology, certain groups of Coleoptera. The results of his studies are to be found in the publications of the Linnean and Entomological Societies, and in numerous scientific journals, during the last thirty or forty years. His monographs of the Malayan and Australian longicorn beetles, in which the collections of Dr. A. R. Wallace are described, are among the most important of these. Although a believer in evolution, he was, with so many of the older systematists, opposed to the theory of Natural Selection. Two books, the latter of which appeared only three years ago, were written by him attacking the Darwinian position.

HENRY HUGH HIGGINS.

Born January 28, 1815.—Died July 2, 1893.

THE Rev. H. H. Higgins, whose death is chronicled by the daily Press as having occurred while writing a paper for the Museums Association Meeting, recently held in London, was born at Turvey Abbey, Bedfordshire. Entering Corpus Christi College, Cambridge, in 1833, he obtained his B.A. in 1836, was admitted to Priest's Orders in 1839, and became a curate at Wolverhampton and at Shrewsbury. In 1842 Mr. Higgins went to Liverpool, and it is in connection with the Liverpool Free Museum that his name has become famous. He was chairman of the Museum sub-committee of management for many years, and devoted much of his time to the arrangement of the collection. Mr. Higgins was the author of "Notes of a Field Naturalist in the Western Tropics," 1877, a "Synopsis of the Invertebrata," "Museums Memorandum Book," "Museum Talk," and several other pamphlets of a practical value on similar subjects. In 1880 he rendered special service to the Liverpool Museum by securing the duplicate Cirripedes collected by Darwin during the voyage of the "Beagle." He founded the Liverpool Naturalists' Field Club in 1860, and had been president of the Literary and Philosophical and the Microscopical Societies of Liverpool. The fine series of Coal-measure fossils from the railway cutting at Ravenhead near St. Helen's, now in the Liverpool Museum, was collected by himself. An excellent portrait of Mr. Higgins appeared in *Research* for July 1, 1888, and to the article which accompanied it, by Mr. O. W. Jeffs, we are indebted for most of this information.

THE death is announced of Dr. Henri Viallanes, Director of the Zoological Station of Arcachon. Dr. Viallanes, who was only thirty-six years of age, had paid considerable attention to the nervous system of the Arthropoda, and his early loss is much to be deplored.

WE also regret to record the death of Dr. John Rae, the Arctic explorer, and of Mr. James W. Davis, the geologist and archæologist, one of our valued contributors. Obituary notices will appear next month.

TO CORRESPONDENTS.

All communications for the Editor *to be addressed to the* Editorial Offices, *now removed to* 5 John Street, Bedford Row, London, W.C.

All communications for the Publishers *to be addressed to* Macmillan & Co., 29 *Bedford Street, Strand, London, W.C.*

All Advertisements *to be forwarded to the sole agents,* John Haddon & Co., *Bouverie House, Salisbury Square, Fleet Street, London, E.C.*

ADDENDUM.

On p. 59, line 38, after the words "Dr. Nansen on glaciation" add the words "are unsound."

NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

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NOTES AND COMMENTS.

The British Association.

BRITISH Science is again preparing for its annual festival, which will be inaugurated by Professor Burdon Sanderson, at Nottingham, on the evening of September 13. There are, as usual, many malcontents who disparagingly remark that the association has "had its day"; and we know a number of eminent biologists and geologists who have declared that this year their vacation shall not be interrupted by attendance at committees and in scientific meeting rooms. There is, however, every prospect that the forthcoming meeting will be no less a success than most of its predecessors; and the organising committees have done well in arranging a goodly number of interesting discussions. We note, moreover, that the President of the section for Geology (Mr. Teall) announces his Address to be not a mere general summary of recent progress, but to deal with no less fundamental a subject than the Doctrine of Uniformitarianism, as viewed from his own special domain of research.

If these discussions of wide themes were more generally encouraged than they are, and if the innumerable, desultory, technical papers, often only of local interest, were absolutely refused, the Sectional Meetings would be much more appreciated than is the case at present, and the Report would be relieved of a burden of brief "abstracts" that are of little use to anyone. However much the enthusiast may sneer at the "picnic element" in the gathering, it can never be reduced until narrow specialists are prevented from inflicting the details of their work upon the Sections. The British Association Meeting is a great reunion of amateurs and patrons of Science rather than of professed devotees, and it forms one of the most important sources of revenue upon which the latter can depend for assistance in their investigations. The programme ought, therefore, to be adapted, as far as possible, to a general scientific assembly, and there is then no fear that this great meeting will lose any of its long-sustained interest and importance in the intellectual life of the nation.

Trilobites.

Considering the abundance of the familiar Palæozoic fossils named Trilobites, it is singular how little is known of their appendages. Twelve years ago the researches of Mr. C. D. Walcott showed that the ventral side of the body was covered with a hardened arch over each segment bearing a pair of limbs, while the head was provided with four pairs of appendages, acting as jaws at the base, as in the modern King Crabs.

More than 20 years ago, Dr. Henry Woodward also discovered a kind of palpus attached to an inferior head-plate ; but conditions of fossilisation more satisfactory than any of those under which trilobites have hitherto been met with can alone permit much advance is our knowledge of the subject.

A stratum in the Hudson River shales, near Rome, New York State, it is interesting to note, has yielded materials for the first step in this advance. The discovery of a number of specimens of

Head of Trilobite showing Antennæ.

Triarthrus becki by Mr. W. S. Valiant, enables Mr. W. D. Matthew to contribute an important paper on the subject to the current issue of the *Amer. Journ. Sci.* (ser. 3, vol. xlvi., pp. 121–125, pl. i.) ; and it now appears that not only had the trilobites the appendages already observed, but also a well-developed pair of antennæ. We reproduce a figure of the best specimen above. The antennæ are composed of a great number of joints, each of which is conical, about half as long as wide, and smallest at the base. They seem to have been of a structure less firm and thick than the substance of the carapace; and just over the spot where they come out, the anterior margin of the head-shield is arched slightly upwards, apparently to afford space for their play to and fro. On the side of the same head there are also some impressions that may indicate the gills, though it is equally probable they are merely hairs on a narrow limb, serving as mouth-organs.

Trilobites are also discussed in the same journal (pp. 142–147, pl. ii.) by Dr. C. E. Beecher, who follows the work of Barrande in attempting to interpret the larvæ. Here, again, the fine character of the sediments in which the fossils are embedded (Lower Helderberg, near Albany, N.Y.) favours the investigation. Moreover, the facts are of especial interest in this case, because all the trilobites belong to highly ornamented and specialised genera. Some beautiful figures are given with detailed descriptions, and Dr. Beecher concludes by trying to generalise in reference to the normal life-history of a trilobite. The results are too technical for recapitulation here, but they show that before long zoologists will be able much more satisfactorily than hitherto to discuss the reference of these characteristic Palæozoic organisms to Crustacea or Arachnids.

Fishes at High Temperatures.

Dr. Lawrence Hamilton forwards some interesting statistics he has collected in reference to the existence of fishes in water of a high temperature. Some of the cases are very striking. Spallanzani, it appears, observed river carp living at a temperature of 106° F., and exhibiting no signs of uneasiness, though at 109° they began to struggle, and died at 116° F. Dr. John Davy (1835) showed that the Bonito had a temperature of 99°, while the water of the Mediterranean, in which it was, had only a temperature of 80°. Saussure stated that he found eels in the hot springs of Aise, in Savoy, at a temperature of 113° F. In 1882, Dr. Davy found that water at 85° F. killed trout by convulsions. A trout and a minnow were put in water at 70° at night, which by the next morning had sunk to 67°, when the trout was dead, though the minnow had not suffered. A salmon parr at 80° became convulsed and torpid, dying at 84°. Several fishes were deposited in water at 53° F.; the temperature of the water was gradually raised, and none showed signs of failing vitality till the thermometer rose to 82°, when the perch became prostrated, roach succumbed at 82½°, salmon at 83°, minnow at 85°, gudgeon at 85½°, dace at 86°, tench at 88°, and carp at 91°. Brandy restored all the fishes except the dace, which died.

In India, fishes at noon-day in their natural water remain in health at 92°; at 4 p.m., 86°; and at 6 p.m., 82°. Günther states that Cyprinodonts live in briny springs even at a temperature of 91° F.

Sir Emerson Tennent collected the following observations, which seem to require further proof or verification :—

In the hot springs of Ceylon, living carp, *Nuria thermoicus*, at

114° F.; members of the perch family, the *Apogon thermalis*, and the *Ambassis thermalis*, in water at 115° F.; a roach, *Leuciscus thermalis*, at 112° F.

In a hot spring at Pooree (in the province of Bengal), with the thermometer in the water standing at 112° F., carnivorous fishes have been discovered, which would indicate that these must have found and fed on living prey at the same high temperature. Further accounts, moreover, declare that in hot springs in Barbary, in North Africa, living fishes have been taken in water at 172°, while in Manila (one of the Philippine Islands) in water marking 187° F. While travelling in South America, Humboldt and Koupland stated that they saw fishes thrown up alive from a volcano in water at 210° F., but this is, of course, an absurdity which nowadays, it is to be hoped, no one will believe.

THE DWINDLING OF LIMESTONES.

IN an article on the Dwindling and Disappearance of Limestones (*Quart. Journ. Geol. Soc.*, vol. xlix., pp. 372–384, pl. xviii., Aug., 1893), Mr. Frank Rutley discusses the question, which he has himself raised, of the possible dissolution of limestones that were formed during the earlier periods of the earth's history. That limestone is dissolved by the action of acidulated waters, and that caverns, "pipes," and other features are formed in the rocks, are facts familiar enough. The author, however, contends that if unlimited time be conceded, there seems no reason why very thick beds of limestone should not wholly disappear; at all events, he thinks that thin ones may easily do so, and thereby certain pages in the life-history of particular localities may be lost for ever. Unfortunately for his argument, the author is unable to bring forward any special evidence in support of it. The Durness Limestone of Cambrian age must be of considerable thickness, and yet it has survived many vicissitudes. The Carboniferous Limestone over large areas, though in places riddled with caverns, presents in mass its full thickness; it passes by alternations of limestone and shale into the beds above and below, while the masses of the *insoluble* strata above and below are more extensively wasted away. Statistics of the amount of solid matter carried away by rivers and streams prove how large an amount of limestone is dissolved, but, except in the case of thermal springs, the loss of material is not very deep-seated.

Attention is drawn to the nodular character of some limestones, and the author very properly points out that in some cases a bed of jointed limestone may become weathered into isolated nodules. He proposes to call these nodules *residual*, as distinct from concretions or segregation-nodules. These residual nodules occur, so far as we know, only where the strata come near the surface; but they serve to confirm one of the author's conclusions, namely, "That bands of limestone-nodules may, in certain cases, represent what were originally *beds* of limestone." Under similar conditions, that is to say, near the surface, "bands or nodules of chert may represent all that remains of what were once beds of cherty limestone."

Whether the comparatively thin limestones in some of the older formations were originally much thicker, is a subject to which attention may profitably be directed.

Bibliography of Botany.

Mr. J. C. Bay, of the Missouri Botanical Garden, submits a scheme in the July issue of the *Botanical Gazette* for the production of a yearly bibliography of American botany. It will be on the lines of Just's well-known *Jahresbericht* which, Mr. Bay complains, pays little attention to American literature, and should give, (*a*) a list of papers and works in every department of botany, *absolutely* complete; (*b*) a review of each of these papers and works, short and perfectly *objective.* A third point is that, as the importance of such a work would be international, it should be published and sold separately and not assimilated by any report or periodical, as in the latter case it would be inaccessible to many botanists. The Smithsonian publications are referred to as so very expensive as hardly to be ever seen in private libraries in other countries. The publishing should therefore, as far as business management goes, be in the hands of a publisher.

The work should be undertaken by a committee among the members of which the editor should distribute the reprints of the papers received from botanical authors. A classification under fifteen heads is suggested, and Mr. Bay is willing to look after subjects falling under three of these. The whole work "could appear in July," *i.e.*, only six months after date; truly an admirable feature. We wish the scheme success. If a publisher can be found to do the thing well and satisfy himself on the score of finance, perhaps some English brother will be courageous enough to follow the example in our own country. At present, we have only Just, and of this the concluding part, *i.e.*, *the part containing the index* of the volume for 1890 bears the date 1893. The *Annals of Botany* started in 1887 with a record of current literature, but this ceased with the fourth volume, under the plea that space was too valuable! The Linnean Society might do it, but it would have to wake up.

The *Kew Bulletin of Miscellaneous Information* is beginning to justify its title in a novel and remarkable way. In the July number we have a list of the principal guests at a garden party given by the First Commissioner of Works on the lawn and reserve part of the Royal Gardens, adjoining Kew Palace. We learn that "more than a thousand people thronged the grounds," and that the "host and hostess, accompanied by about 200 guests, arrived at Kew by special steamers from the House of Commons Pier." Surely such details and records might be left to the *Morning Post* and the Society journals, which are not published by "Her Majesty's Stationery Office." The "miscellaneous information" is expected to have reference to economic botany, and not to the festivities of "Society." The Director of Kew is commonly supposed to have exhibited a courageous independence of the wishes of exalted personages, who have presumed too much in their visits to the Royal Gardens, and one would have expected him not to tolerate such doings at all, rather than to chronicle their occurrence in the *Bulletin.* Even if this account be followed by a more extended list of less eminent visitors on the August Bank Holiday, with a report of their innocent recreations, it will only show the Director's wide view of the limits of "Society." The one thing would be as unjustifiable as the other, in an official publication, which may, or may not, by its sale repay the Treasury for the cost of its production.

It might perhaps conduce to promptness if the editor of the *Kew Bulletin* read Natural Science. In last year's July number (vol. i., p. 327) we gave an abstract of a Colonial Report on the Aldabra Islands, a small group to the north-east of Madagascar; while the *Kew Bulletin* for July of this year supplies "some recent information about this little-known group," in great part consisting of a letter from the Administrator of the Seychelles, dated June 13, 1892, which merely repeats some of the facts noticed by us a year ago. About a third of the article is occupied with a letter from Dr. W. L. Abbott, an American, who visited the islands at the end of last year and collected specimens of most of the native plants, which were sent to Kew, but have, unfortunately, miscarried. His short note adds but little to our knowledge of the vegetation already gained from the account given by the lessee, Mr. Spurs, in the above-mentioned Colonial report. Dr. Abbott says: "There do not seem to be more than 35 indigenous species," while Mr. Spurs mentions 30. The only trees now in Aldabra are Casuarinas and Mangroves, but decaying stumps of "Porché" and "Rose-wood" show that these formerly grew to a considerable size, though now only small ones exist. Mr. Spurs referred to the destruction of the large trees, which he considers partly due to the hurricane of 1889.

Apropos of the *Bulletin*, why is it necessary to insert so many official letters spread out into numbered paragraphs like a child's lesson-book? If the information, which is often of interest, were boiled down and the numerous salutations, etc., omitted, the publication would be more readable, and the recent doubling of the price rendered unnecessary.

Dr. Otto Kuntze communicates some "Remarks on the Genoa Congress" to the July number of the Californian journal, *Erythea.*

Dr. Kuntze finding himself unable to prepare in time the elaborate critique on questions of nomenclature which he had promised to lay before the International Committee at the end of June, offers to Americans, through the pages of *Erythea*, a brief syllabus of leading exceptions to be taken to the doings of the Genoa Congress last year. Its resolutions, he says, must fail to obtain the force of international laws by reason of—

I. Four illegalities in its organisation and methods of procedure.

II. Three absurdities, by which the resolutions which they affect become illegal and impracticable.

III. *Violatio juris quæsiti.*

Is *Erythea* on the wane? The last three numbers (May to July) feel very thin, containing only 16, 20, and 16 pages respectively, whereas the journal started with 28 last January, and contained 24 in February and March, while the number rose to 32 in April.

Two alpine anemones, *Anemone alpina* and *A. sulphurea*, are the subject of a notice by M. F. Prévost Ritter, in the *Bulletin de l'Herbier Boissier*, no. 6. The second of these two is generally admitted to be only a colour form of the first, depending on the chemical composition of the soil. H. Christ, in his work on the origin of the Swiss flora, cites them as a most remarkable instance of the influence of the soil on the distribution of plants. In the Alps, he says, the white-flowered form (*A. alpina*) occurs exclusively on the chalk, while the one with a sulphur-yellow blossom is only found on clayey and sandy soil. So sharp is the limitation, that where the two soils meet the two varieties faithfully follow the line of division; while where the transition is gradual, the flower passes by a series of intermediate shades from white to yellow. In order to find out, if possible, the cause of this phenomenon, M. Ritter has, since 1886, been cultivating the seedlings of the two forms in sandy and chalky soils. He finds that, while *Anemone alpina* prefers the chalk, but will grow in sand, *A. sulphurea* will only grow in the latter. He also concludes from differences in the shape of the cotyledons, that the yellow-flowered plant must be considered not as a mere form or variety, but as a distinct species. He says, and his figures bear him out, that the seed-leaves of *A. sulphurea* are larger, and have a shorter and more rounded tip, while those of the *A. alpina* are narrower, oblong, and pointed.

In the *Journal de Botanique* for July 1 and 16, M. Hua describes as a new genus a plant from West Tropical Africa, in which the flowers are borne along the midrib on the back of the leaf. This anomalous position of the flowers is only of rare occurrence, appearing in a few almost or quite monotypic genera, such as the

Japanese *Helwingia* of the Araliaceæ, the saxifragaceous *Phyllonoma* from the New World, and *Polycardia* (Celastrineæ) from Madagascar. The common lime recalls, in a small degree, the same phenomenon, the stalk of the inflorescence adhering to the lower part of the bract and appearing to spring from the middle of that organ. Tropical West Africa boasted already two genera with epiphyllous flowers both belonging to the family Bixineæ, and *Mocquerysia*, the new one established by M. Hua (named after its discoverer, M. Mocquerys) resembles these in some points, and is placed by its author in the same natural order.

If ordinary placental mammals have evolved from pouched animals like the modern marsupials, rudiments of the pouch ought certainly to be recognisable in some of them. Dr. H. Klaatsch (*Morphol. Jahrb.*, vol. xx., pp. 276–288, 1893), has just made the interesting announcement that such rudiments can actually be observed in most placentals. Something of the kind has already been found in the lemurs, and one author has supposed that rudiments of the pouch can also be detected in the sheep. The detailed account of Dr. Klaatsch's extension of the evidence will be awaited with interest.

M. Adrien Dollfus provides a first supplement to his Catalogue of the Terrestrial Isopoda of Spain in the xxii. vol. of *Anal. Soc. Españ. Hist. Nat.*, 1893. The same mail brings to us a second paper of his on this group of animals from the Canaries, which adds to our previous knowledge obtained through him in a paper dealing with specimens from the Azores, Madeira, and the Canaries (*Bull. Soc. Zool. France*, 1889, p. 12). Seven new forms are described, and the essential portions figured, in *Mém. Soc. Zool. France*, vol. vi., 1893, pp. 46–56. It is quite possible that diligent search in England might reveal many interesting points in distribution of the wood-lice, and even result in the discovery of new forms.

Apropos of the "Plague of Wasps" in the South of England, a correspondent suggests our calling in the assistance of the toad. "A toad will," he writes, "sit for some time quite comfortably by a wasps' nest (in the evening, of course) and pick up the belated wanderers as they return home. The sting is absolutely disregarded, and not the faintest attention is paid to the warning colours of the wasp. An evening or two since I persuaded a large fat toad to take up a position actually blocking up the entrance to a nest. He waited there for a few minutes, during which he caught several wasps and made unsuccessful shots with his long tongue at others." One might, perhaps, instal a circle of toads round the orifice of the nest, and then, after stirring it up with a long stick, depart to give free play to the toads and wasps to fight it out between them.

A correspondent remarks that in our review of Professor Alfred Newton's "Dictionary of Birds" last month (p. 146), we overlooked the fact that the author had descended to a particularly low level in his criticisms on others. "The remarks on p. 133 regarding the Timeliidæ can only be described as the language of the gutter, and should hardly emanate from a member of one of our old Universities, a person of culture, and, we had hoped, a gentleman. But they impress upon us more than ever the old adage, 'Manners makyth man.'" We are not concerned here with the author's convenient neglect of many well-known publications—that will reflect upon himself in due season.

Dr. J. W. Gregory arrived safely at Mombasa, East Africa, on August 19, and he may be expected in England before the end of September.

According to the London *Daily Chronicle*, two members of the Geological Survey of Canada have left this summer for the exploration of Labrador. The exploring party will be absent from civilisation for two years; they will traverse the interior of Labrador from south to north, as well as from east to west, especially visiting the great Lake Mastassini, and the cataract of the Hamilton River, concerning the size and nature of which such fabulous tales have been told. They will winter with the Eskimos on Ungava Bay, where only four of the twenty-four hours of the winter day are daylight. They expect to return to Canada from Hamilton inlet, *viâ* a Hudson's Bay Company's steamer trading with England.

M. Mathieu Meig contributes to the *Feuille des Jeunes Naturalistes*, a paper of Excursions in the district of the Lower Carboniferous of Alsace. This is apparently one of a series, and deals with Bourbach-le-Haut, Massevaux, Rothhütel, Rossberg, Stauffen, Steinby and Hundsrücken. M. Meig commences with a useful bibliography of papers on the district, gives notes on the general divisions and composition of the Carboniferous beds of Alsace, and discusses the details in the form of an itinerary.

The August number of the *Quarterly Journal of the Geological Society* contains an important contribution to the Highland question. Mr. George Barrow has now traced a clear succession in Forfarshire, the comparatively thin strata being repeated by sharp folds, till they form a mass of enormous thickness. The various zones are all highly metamorphosed; but Mr. Barrow shows by analyses and the occurrence of particular minerals, that a certain zone is an altered quartzite, another is an altered limestone, and a third is probably a highly metamorphosed clay. The greater part of this change he considers to be due to the intrusion of huge sills or laccolites of muscovite-biotite gneiss.

Attention was directed to the Underground Waste of the Land in a former number of Natural Science (vol. ii., p. 124). The subject has also attracted the notice of Professor Lebour. In an article on certain surface features of the Glacial deposits of the Tyne Valley, near Corbridge (*Nat. Hist. Trans. of Northumberland*, etc., vol. xi., part 2, 1893), he states that a notable amount of sand and very fine gravel is continually being discharged from underground, by springs which issue at the junction of the Glacial sands and gravels with the underlying Boulder Clay. He thinks that the bowl-shaped depressions of the surface of the drift gravel and sand of the Tyne Valley—depressions that have the characters of "kettle-holes"—may be due to subterranean erosion. Similarly, features resembling the arched structure of Kames or Eskers may be caused by slipping, due to the eroding action of water below ground.

Another new British monthly devoted to the science of geology has appeared! We have before us *The Glacialists' Magazine*, vol. i., no. 1, August, 1893. Price 6d. It is edited by Percy F. Kendall, assisted by Warren Upham, C. E. De Rance, and J. Lomas, and its pages are crowded with misprints. Mr. De Rance, who is President of the Glacialists' Association, communicates his address, which was delivered last year, and discusses the geology of the northern Polar regions, concluding that no evidence of former Glacial periods is to be found among the strata. He is, however, scarcely just to the naturalists who accompanied Sir George Nares, when he says it fell to his lot to work up the geological results obtained. The observations were made by Colonel Feilden and others (very competent geologists), while the fossils were identified by Mr. Etheridge. An intrusive mass of Boulder Clay at Bidston, Cheshire, is described by Mr. A. R. Dwerryhouse; and there is a review of Sir Henry Howorth's "Glacial Nightmare" by the Editor. There are also short notes on various subjects, and an account of "Current Glacial Bibliography." The Bibliography, if rendered complete, will no doubt form a valuable record, but otherwise we are inclined to question the advantage of issuing a separate journal on the subject of Glacial Geology. Moreover, we fear that a modest monthly of 29 pages will hardly suffice for the verbosity peculiar to most writers on Glacial Geology.

Still another American journal relating to geology—the *Bulletin of the Department of Geology, University of California*—has made its appearance. As far as we can judge by the first part, which is all that has reached us, it would, perhaps, be more correctly entitled "Memoirs" than "Bulletin," for it consists of one paper of 59 pages, "The Geology of Carmels Bay," by Andrew C. Lawson, assisted by Juan de la C. Posada. As with most of the American scientific journals, this one is extremely well got up, printing and illustrations both being excellent; but we would like to suggest to our American cousins that the large size, thick paper, and enormously wide margins of all these new journals is a mistake. It is all very well to have one or two large handsome volumes, but when the series mounts to fifty or one hundred, everyone will grumble at the amount of space wasted. This is a matter in which we in the old country are best able to form an opinion, for we have many journals that have reached their fiftieth volume, and all true workers would contemplate with dismay the idea of having to allow double the space for each set.

We regret to learn that the *Midland Naturalist* receives so little support that its publication will be discontinued at the end of the present year.

Two numbers of an "Occasional Paper for Nyasaland" have been printed and published at the Universities Mission Press, at Likoma, an island in Lake Nyasa. They contain notes on Nyasaland Timber, by Mr. William Murray; and memoranda on the occurrence of Garnets and Beryls, by Archdeacon Maples. In future the paper is to be published four times a year, under the name of *The Nyasa News.*

London will shortly rejoice in another club. On and about August 17, a perfect rain of circulars announced to the scientific world that there was no club available "for social intercourse and réunion" of the fellows of the various royal and learned societies. We are glad to observe that so careful a distinction is marked out by the promoters between royal and learned societies. On the front of the circular is a list of "Gentlemen supporting the founding of the Club," whose names can, with patience, be dug out of the perfectly bewildering conglomerate of F.R.S.—F.R.S.G.S.—R.Z.S.I.—Brit. Ass. [*sic*],—etc., etc., in which they are embedded. Then follows the ground for establishing such an arcadia, all for six guineas a year, and lastly, on the back of the circular, comes a series of newspaper-like extracts from letters of the learned, in which the "positive want" is stated with consentaineity, while numerous wailings escape from extra-athenæum clubites. The whole is surmounted by a badge consisting of the American eagle, rather out of drawing, with wings resembling the star-spangled banner, and surrounded by an aureole, in which is hidden "Omnes artes inter se continentur." Colonel W. P. Hodnett, late of the 2nd Dorset (54th) Regiment, as hon. secretary, 63 St. James's Street, will attend to the despairing. *Sic iter ad astra.*

I.

On Epiphytes.

PLANTS are much more directly dependent on the nature of their physical surroundings than are animals, and display a correspondingly greater plasticity of form under varying external conditions. Hence among plants examples of structural harmony with the environment are both striking and numerous; but this malleability in the nature of a plant renders it more difficult to decide when we are dealing with a true instance of adaptation. A leaf exposed to direct sunlight transpires at a greater rate than it would in a damp shaded habitat; and we find that leaves which develop in the sunlight have a structure different to that of leaves of the same species formed in the shade. The leaves developed in the light are structurally so constituted that they transpire less than those formed in the shade, provided that both sets of leaves be exposed to the same conditions; but these peculiarities of structure are not transmitted to the offspring of the respective plants. Here we are not dealing with a case of adaptation—in the narrow sense—but with an instance of the direct effect of the environment on the individual plant. Now this is obviously very different to the case of desert plants, in which the leaves have structural characteristics (succulence, hairs, thick cuticle, sunken stomata, etc.), which are handed down to the offspring grown under any conditions. Here we can see that the leaves are constituted so as to avoid excessive loss of water—they are suited to the dry habitat. But can we at once pronounce this a case of adaptation to the peculiar mode of life on the desert? Decidedly not. Either the ancestor of the desert plant possessed another form, and only changed it gradually as it became a desert plant; or the ancestor possessed these structural peculiarities even before it became a desert plant, and, in fact, only assumed its present mode of life in virtue of these characteristics. In the latter case, correctly speaking, the desert plant is not adapted, though it is suited, to its peculiar mode of life. The environment has not impressed itself on the nature of the plant. That such cases are possible is shown by the fact that plants occupying widely different positions in nature often betray remarkable structural similarities. For example, many Epiphytes, desert-plants, littoral forms, neighbours of mineral springs, alpine plants, and temperate evergreens possess leaves which agree in being constituted so as to avoid too rapid transpiration (succulence or leathery or hairy nature, strongly developed epidermis with thick cuticle, water-storing tissue) (1). A plant evolved so as to suit one of the above-mentioned environments would incidentally become suited to one of the other modes of life ; and it might desert its old habitat and take up the new one for which it had casually become suited. This process has taken place ; even now we find plants of which some individuals are epiphytic in jungles, and others are terrestrial alpine plants. In other species, some of the individuals are littoral plants, which do not reappear inland until we reach the mountain tops; and what is true of these individuals is true of certain species of genera. The genus *Casuarina* is a littoral form and a Malayan alpine form, but does not occur in the jungles between these two extremes. So it becomes very difficult to say what characters in a plant are truly adaptive, *i.e.*, acquired with special reference to the present mode of life. And we can only solve the mystery in each case by, on the one hand, studying the physiological nature of the plant; and, on the other, tracing out its past history by means of morphological comparisons with allied forms, and observations on the geographical distribution of it and its relatives. The first person to recognise this was A. F. W. Schimper, whose work on tropical American Epiphytes (2) revolutionised this branch of botany. He introduced a new method of treating biological problems, which should serve as a model for subsequent investigators.

Epiphytes are subjects peculiarly suitable for the study of adaptation; for among them occur all stages from unmodified terrestrial plants right up to forms highly metamorphosed to suit their aërial mode of existence.

For our knowledge on Epiphytes we are almost solely indebted to Schimper, and the present article is almost entirely based upon his work. Goebel, however, who has followed—far behind—in Schimper's footsteps the study of the biological problems, has been at the trouble to collect facts observed by other investigators of Epiphytes, and has added a few examples illustrating the principles advanced by Schimper and others (3).

Compared with temperate forests, the moist tropical jungles are overwhelmingly rich in Epiphytes. There they vary in size from minute epiphyllous lichens up to the gigantic herbs or huge shrubs. Often an Epiphyte is itself loaded by countless others, on which, in turn, are crowds of smaller ones.

Most of these are confined to a few families :—Lycopodiaceæ, Ferns, Bromeliaceæ, Araceæ, Orchidaceæ, Urticaceæ, Piperaceæ, Melastomaceæ, Ericaceæ, Asclepiadaceæ, Solanaceæ, Gesneraceæ, and Rubiaceæ (as well as our temperate Epiphytes, Mosses, and Lichens). Although these families are few in number, they contain many epiphytic genera ; and, with the exception of the purely American family, the Bromeliaceæ, they occur in the tropics of both the New and Old World. These families must possess some characters which confer upon them the power of assuming an epiphytic mode of existence.

The first indispensable feature is that the seed shall be able to reach the tree on which it is to grow. The only two practicable methods of seed-dissemination are by the wind or by the agency of animals. This fact has been well illustrated by the observations of Loew (4) on the means of distribution of Epiphytes found on willow-tops in Germany. A walk along the banks of an English river, shaded by pollard-willows, reveals the fact that there is a rich epiphytic flora on these trees. Among them are not only algæ, and mosses, but also numerous herbs, and large rose and blackberry bushes. Willis and Burkill (5) found that, near Cambridge, 44·6 per cent. of these Epiphytes had fleshy fruits; 16·4 per cent. had burrs; 25·1 per cent. had winged or feathery fruits; 10·6 per cent. had very light seeds; while in only 2·9 per cent. was the means of distribution feeble or obscure.

So, in the tropics, the seeds of Epiphytes may be all ranged under three main classes:—

(i.) Those enveloped in a fleshy covering (many bromeliads);

(ii.) Seeds (or spores) very light and easily carried by the wind (*Dendrobium attenuatum*, the seed of which weighs ·00000565 grammes);

(iii.) Seeds light and with a mechanism for floating in the air. ("Wings" in *Rhododendron pendulum*, in which the seed weighs ·000028 grammes: tufts of hairs in Asclepiadaceæ: air-containing spaces in the envelope, *Æschynanthus pulchra*.)

In all cases of these regular tropical Epiphytes the seeds are small, so that they can easily become lodged in minute crevices on the surface of the host. Those characters are not adaptive. The seeds must have possessed them to a large extent when the plants were terrestrial; otherwise the latter could have never become Epiphytes. The structure of their fruits and seeds explains why certain families should not have Epiphytes among them—thus the heavy-seeded Leguminosæ. On the other hand, in certain families only those types which possessed appropriate seeds have become Epiphytes. For example, the Loganiaceæ frequently possess fleshy fruits, but the seeds are large excepting in *Fagræa*, which accordingly has epiphytic species.

Even when the seed has reached the surface it must rapidly make arrangements for the firm attachment of the embryo. Only a few observations exist on the germination of Epiphytes (*Clusia*, *Myrmecodia*, *Æschynanthus*, *Tæniophyllum*). In all these the primary root soon ceases to grow, and there is a precocious development of an extensive system of adventitious roots. In *Æschynanthus pulchra* (3) the radicle remains short, the inferior end of the embryo becomes flattened, and its periphery swollen. In this manner a species of adhesive disk is formed and applied closely to the uneven surface of the host. The attachment is rendered all the firmer by the formation of numerous root-hairs radiating from the peripheral swelling.

Thus the whole root-system of an Epiphyte—except at a very early stage of germination—is adventitious. The same is the case with shrubs growing in our own climate on rocks or walls. In both cases this is obviously the direct effect of the mode of life.

The flowers of Epiphytes display no adaptive characters.

Concerning the modes of reproduction, vegetative multiplication is frequent. The case of *Tillandsia usnevides* is interesting. Its pendent shoots are either whisked away by the wind or carried away by birds for the purpose of constructing nests. The transferred fragments send out new shoots, which twine round the branches of the new host. This *Tillandsia* only rarely produces flowers.

It is in the structural characters of the vegetative organs that Epiphytes display the greatest amount of adaptation.

Epiphytes grow in situations where the supply of nutritive solutions is meagre and fluctuating, and where, usually, only diffused light reaches them. Moreover, they live in positions in which firm support is essential. For fixation and the absorption of nutritive solutions, a strong development of surface of the root-system is indispensable. Owing to the poverty of their water-supply, measures must be taken to avoid excessive transpiration. A desert plant accomplishes this by diminishing its transpiring surface; but an Epiphyte cannot afford to act similarly, because it requires a large assimilatory surface in order to make up for the weakness of its illumination. The Cactaceæ illustrate this point. The desert forms often have swollen, rounded shoots which are sparsely branched; but the epiphytic forms possess flat, leaf-like shoots (*Phyllocactus*, *Epiphyllum*), or have numerous thin branches (*Rhipsalis*, *Cassytha*). It only remains, then, for the Epiphytes to have leaves constructed so as to retain their supply of water. Hence the leaves are often leathery or fleshy, and frequently have a strong development of cuticle.

The mode in which Epiphytes absorb nutritive solutions serves as a basis for their classification. Appended is a brief table to show the grouping:—

Class I.—*Epiphytes which absorb the nutritive solutions by means of their roots.*

Group (i.) Those which content themselves with utilising the water and humus occurring on the surface of the host-plant.

Group (ii.) Those which have attaching roots and also absorptive roots which descend to the ground.

Group (iii.) Those which have arrangements for collecting water and humus.

Class II.—*Epiphytes which absorb the nutritive solutions by means of their leaves.*

Class I.—Group i.

The first group of Epiphytes is made up of forms which merely absorb the nutritive solutions occurring on the surface of the host-plant.

The simplest of these do not differ in any way from terrestrial plants. They live in the gloomy depths of the forest on the mossy or furrowed bark of the lower parts of trees (Hymenophyllaceæ, *Lycopodium*, and an orchid *Stenoptera*).

However, most adopt measures to guard against injury from excessive loss of water. A few forms evade injury in this respect simply by their ability to endure being completely dried up without being harmed. This is well known in the case of lichens and mosses; but certain tropical polypodies are endowed with similar vitality. Rainless weeks of the blazing West Indian sun are insufficient to scorch out of existence *Polypodium incanum*, parched though it be.

In the majority of genera there are distinct structural modifications in the form of water-reservoirs. Parts of them become fleshy and store up water. Fleshy leaves occur in ferns, orchids, and Gesneraceæ; bulbs in Amaryllidaceæ; tubers in ferns, and epiphytic Utricularias; fleshy roots in Vacciniaceæ, and the curious melastomaceous *Pachycentria*.

This water may be stored up in intercellular spaces, in ordinary parenchymatous cells, or in specialised cells ("aqueous tissue," or "storage tracheides").

The leaves of certain Gesneraceæ have, between the upper superficial layer and the green assimilatory tissue, aqueous tissue in the form of colourless cells. This tissue increases in size late in the life of the leaf, long after all other morphological changes have been accomplished. Isolating old leaves for four weeks, they diminish in thickness only to a very slight degree; but if whole branches be isolated for the same period, the old leaves become much thinner (and gradually die), because the aqueous tissue has parted with water, and has shrunk; but the young leaves are scarcely any thinner than at the commencement. These observations show that the aqueous tissue is a reservoir on which the younger leaves can draw, and that it takes on its peculiar function late in the life of the leaf.

It is, however, among aroids and orchids that the most interesting arrangements occur.

In the Araceæ, aqueous tissue is not known. *Philodendron cannifolium* may be chosen as a type of aroid belonging to this group of Epiphytes. It is a large herb with its leaves forming a rosette more than three feet in height. The stem is short and gives off numerous roots. Each leaf has a tongue-like lamina and a spindle-like petiole. The leaf possesses spongy tissue with large and conspicuous intercellular spaces. In the dry season the spaces are occupied by air, but when the wet season comes on, the intercellular system being lined with mucilage, absorbs water which gradually replaces the air. The swollen petiole is a water-reservoir calculated to meet the demands of the lamina. This is proved by the following experiment: If a whole leaf be isolated and left to dry for fifteen days it remains unchanged in appearance; but if the lamina alone be treated thus, it fades and shrinks in a few days.

Among the Orchidaceæ, the arrangements for the storage of water are varied and characteristic. Some possess leaves with well developed broad tracheides, the function of which is to store water. In others, the water is retained in pseudo-bulbs. Experiments similar to those on *Philodendron* prove that the latter are water-reservoirs. For this reason, orchids with pseudo-bulbs usually have thin leaves; whereas, orchids with fleshy leaves are devoid of pseudo-bulbs. Only a few orchids have thin leaves and no pseudo-bulbs, but, to compensate, they have fleshy roots (*Isochilus*). Attention has already been directed to the orchid *Stenoptera* which has no water reservoir, as it grows in damp, gloomy spots.

The most curious structural peculiarity which is common to all epiphytic orchids (almost without exception) is the velamen possessed by the roots. Typically this envelope is made up of a number of layers of tracheides with perforated walls. These absorb water deposited as dew or flowing down the surface of the host; but it is quite a mistake to suppose that they have any appreciable power of condensing moisture from the atmosphere. The velamen is not always an absorptive layer. In *Bromheadia alticola* (6) it is a sheath to prevent the evaporation of water which is absorbed by root-hairs which attach the plant to the substratum.

The question arises, "Is the velamen really adaptive?" I am of opinion that the answer must be given in the affirmative, in spite of the cautious scepticism of Schimper on the subject. With the single exception of one species of *Stenoptera*, all epiphytic orchids have a velamen. All other known species of *Stenoptera* are terrestrial, and this species only grows in damp, moss-laden, furrowed bark in the lower regions of forests; moreover, its structure is purely terrestrial. We must conclude that this *Stenoptera* has only recently adopted its epiphytic mode of existence (6). On the other hand, with the exception of two species, all terrestrial orchids known are devoid of a velamen. *Epidendrum cinnabarinum* is one of these exceptions, and has a velamen. Now it is almost always a terrestrial orchid, but Fritz Müller[1] has found it as an Epiphyte. It is a renegade Epiphyte which has become terrestrial, as is confirmed by the occurrence on it of useless aërial roots, and by the fact that its relatives are Epiphytes. The other exception is the purely terrestrial *Bromheadia palustris* (6). It undoubtedly has been derived from an epiphytic form. It possesses a curious impermeable velamen like that of the epiphytic *B. alticola*; but this velamen is exceed-

[1] Communicated by letter to Professor Schimper.

ingly transitory and peels off almost simultaneously with the root-cap cells. The last reason for believing that the velamen is adaptive is afforded by its re-appearance in Epiphytes belonging to the widely-separated family, the Araceae. Thus Epiphytes acquired their velamen after they had deserted their terrestrial mode of life.

When young, the aërial roots of orchids usually contain chlorophyll; but their assimilatory function is insignificant. However, in *Æranthus* and in *Taeniophyllum* the leaves are reduced to scales, while the roots are large and green. The plant consists almost solely of root-system. In the roots of *Æranthus*, Schimper discovered that certain regions of the velamen are impermeable to water, but are easily penetrated by air. These regions obviously function as stomata do in leaves. In *A. fasciola* the leaf-like appearance of the roots is heightened by their dorsi-ventral structure.

Class I.—Group ii.

Members of the second group of Epiphytes possess roots of two sorts—they are hence heterorhizal, and exhibit an advance on the first class. When the seed germinates on the host-plant, some roots are produced which clasp the host like tendrils. These are purely organs of attachment. The plant also produces long nutritive roots which descend rapidly to the ground.

Such roots are possessed by *Clusia* and *Ficus*, among *Dicotyledons*: and by species of *Philodendron*, *Anthurium*, and *Carludovica*, among *Monocotyledons*.

The attaching roots are negatively heliotropic, but have no marked geotropic properties. They have a slow, definite growth, and coil closely round their support, giving off numerous root-hairs. They have no absorptive function. In them the xylem consists mainly of thick-walled lignified fibres with relatively few and narrow conducting constituents.

The nutritive roots, in some cases, possess negatively heliotropic characters, but they are always positively geotropic to an eminent degree. Their growth is rapid and indefinite, some roots attaining a length of more than a hundred feet. Till reaching the soil they remain thin and unbranched, but on dipping into the soil they give off numerous subterranean lateral roots. The nutritive materials from the soil have to travel over long distances by way of these roots. The conducting constituents of the xylem are accordingly numerous and prominent, whereas the fibres are feebly developed. The necessary amount of rigidity is ensured by the formation of peripheral sheaths of sclerenchyma or collenchyma.

Clusia and *Ficus* frequently "strangle" their hosts by investing their trunks with a fenestrated cylinder formed of the fused roots which embrace the trees on which these Epiphytes originally lodged.

Class I.—Group iii.

The members of the third group possess arrangements for the retention of decaying humus-giving bodies, and the roots frequently branch copiously in the air, some of them deserting the surface of the host.

In many epiphytic orchids there are attaching roots which are negatively heliotropic, and in addition clusters of erect negatively geotropic roots which are wholly absorptive in function. In orchid houses, potted specimens of these orchids frequently possess roots growing vertically upwards out of the soil (species of *Cymbidium*, *Eulophia*); but there is no reason to believe that these roots act thus in order to supply the subterranean parts with oxygen. They simply point upwards because, when the plants are growing in their natural position (as Epiphytes), the nutritive materials come from above, and are absorbed by these peculiar roots.

An aroid, *Anthurium Hügelii*, is a good type of an Epiphyte belonging to this group. Although a large plant, it is often to be found fastened to the smooth aërial roots of *Clusia*, or to the band-like stems of *Bauhinia*. It is fixed to its support by means of very long roots, which stretch along the surface of the host and give off root-hairs only on the side towards the latter. An extensive rounded complex of roots surrounds and overtops the dwarfed stem, and gives off numerous lateral roots among the sub-sessile leaves. The last-named form a large rosette enclosing a mass of vegetable fragments, which decompose into humus. The complex is composed mainly of much branched and hairy nutritive roots which are closely interlaced at their bases, but which possess apices freely protruding into the air or dipping into the humus.

In this plant, as in the rest of this group of Epiphytes, the structural differences between the attaching and nutritive roots are not so wide as in the second group. This is due to the fact that the attaching roots absorb water to a certain extent, and that the nutritive roots do not conduct the absorbed substances over long distances. Still, the nutritive roots of *Anthurium Hügelii* possess larger and more numerous vessels and fewer sclerenchyma-fibres than do the attaching-roots.

Some ferns have precisely similar arrangements, *e.g.*, *Polypodium phyllitidis*.

In other ferns the humus is collected by single leaves which form pockets against the surface of the host's stem, or against their own stems. In *Polypodium quercifolium* (3) the differentiation has gone so far that there are two sorts of leaves: in the first place, ordinary stalked pinnate leaves; secondly, sessile negatively geotropic leaves, which form pockets against the opposed surface. The latter assimilate only for a brief period, but persist as large, dead, yellow oak-like leaves. Thus when dead they do not drop off (as do the normal foliage leaves), but, being strongly ribbed, persist as receptacles for humus.

Another heterophyllous Epiphyte is the curious twining, pitchered

Dischidia rafflesiana. Some of its leaves are hollowed out into pitchers. It has roots of two sorts—attaching and nutritive. The latter are given off into the cavities of the pitchers, within which they ramify. Treub (7) was of opinion that these pitchers functioned solely, or mainly, as receptacles for rain-water and economisers of the water transpired within the pitchers. He showed that the epidermis lining the pitcher-cavity could not absorb water. But I have shown that very considerable amounts of humus-providing materials are collected in these pitchers (8), and that the roots within the pitchers absorb this food placed at their disposal. These pitchers have the same function as the rosettes of *Anthurium* or the "pocket-leaves" of ferns —they collect humus. However, they are able to collect more water than is possible to their simpler analogues. One curious fact about these pitchers is the constant presence of ants living within them. Probably a large portion of the food found in these pitchers is conveyed thither by the ants. The plant provides shelter for the ants, and they in return bring these materials (for the purpose of building nests).

Externally unlike *Dischidia rafflesiana*, but very similar in fundamental design, are two Rubiaceous Epiphytes—*Myrmecodia* and *Hydnophytum*. In each of these genera the base of the stem takes the form of a great swollen tuber, which is hollowed out by a series of galleries or canals continuous with one another, and communicating with the exterior by, mostly small, apertures. These galleries are always found to be occupied by innumerable ants belonging to a certain definite genus and species. In a typical case, lenticels occur in the walls of the galleries from which there protrude, more or less, distinct roots. In principle, then, this great tuber corresponds to a pitcher of *Dischidia*; the stomata of the epidermal lining of the pitcher correspond to the lenticels opening into the galleries; and the roots protrude into the galleries and the pitcher-cavity respectively. Rain-water can only pour into some of the pitchers of *Dischidia*, because their mouths sometimes point downwards; but rain-water can enter freely into the galleries in only one known species. In most cases, the external openings of the galleries are so small that water could only enter in the form of a thin capillary film. Formerly it was supposed that the ants constructed the galleries, and that these animals were essential to the existence of these plants; but Treub (9) showed that the galleries developed in the absence of ants, and that the plants could exist without the proper ants. The hypothesis that the galleries are respiratory passages is quite inadequate. The whole arrangement has been looked upon as a mechanism for the economy of water. This hypothesis is backed up by no experiments, and, at present, is only bolstered up by a few facts, and more or less distant analogies.

It is still possible that the ants have been the prime factors in the evolution of these excavated tubers. The development of the galleries is not out of harmony with this view. The observation that under cultivation these plants live without their ants, tells us naught of what would happen to them in their wild state under similar circumstances; and it is futile to object that the plants cannot be myrmecophilous (ant-loving) because they do not attract ants by means of honey or food. Ants are attracted to these plants. This is proved, first, by the constant presence of a certain definite species of ant in specimens growing wild; and, secondly, by the extreme difficulty in preventing ants crowding on cultivated seedlings of these plants, even when adjacent seedlings of other plants are neglected. It is possible that ants convey into the galleries materials which can be subsequently utilised by the roots therein. But at present *Myrmecodia* and *Hydnophytum* are living mysteries.

Class II.

The second class, represented by the Bromeliaceæ, stands apart from the types previously described in that food and water are absorbed by the leaves—and not to any appreciable extent by the roots.

Often the leaves together form a very close rosette—a funnel—which contains water with humus-giving bodies. By isolating some of these funnel-forming bromeliads, Schimper proved that the leaves absorbed nutritive solutions. Living specimens were deprived of their roots and the wounds were closed with Canada balsam. Water was poured into the funnels of some of these, but the rest were kept without water. The former remained fresh as long as the experiment lasted—ten to twelve weeks; but the latter died in the course of a few days or weeks. Water supplied only to the roots of isolated specimens neither restored faded plants nor appreciably retarded the death of freshly isolated individuals. This experiment shows that the roots absorb only a negligible amount of water—they are purely organs of attachment. Hence epiphytic bromeliads which have other means of fixation are devoid of roots.

Tillandsia usneoides is one of the commonest rootless Epiphytes belonging to the Bromeliaceæ. This plant forms silvery-grey, pendent masses, which resemble horses' tails or manes. These tufts attain lengths of more than ten feet. Each thread consists of a filamentous distichous shoot, which, at its base, twines round a branch of its host. Hanging down as it does, the plant possesses no external, funnel-like reservoir for water.

The water is absorbed by means of certain peculiar scaly hairs. In funnel-forming bromeliads these are confined to the basal parts of the leaves, thus they only occur in the region of the water-reservoirs; but in forms, like *Tillandsia usneoides*, having no external receptacle for water, the scaly hairs are distributed over the whole surface.

In *Tillandsia*, which may serve as a type, each scaly hair consists of: (i.) a stalk-like *base* sunk into the tissue; (ii.) a peltate upper part

—the *shield*—lying flat over the surface of the leaf. This shield has a central group of cells, and a thin extended margin composed of cells or cell-walls. Underneath the *base* is a group of three cells on top of one another; they are protoplasmic and have thin uncuticularised walls (except sometimes the lowest wall is thick but pitted). These three cells are "passage-cells."

If a drop of water be placed on a leaf of *Tillandsia usneoides*, the silvery-grey appearance is locally replaced by a green colour. Examination with a microscope reveals the fact that at first the central cells of the *shield* contained air, but on supplying water these cells become filled with it. Placing a drop of caustic potash or strong sodium chloride solution on the leaf, it can be seen that the colour changes or plasmolytic phenomena commence first in the epidermal cells nearest the scaly hairs, and gradually advance to more distant cells. Finally, the cells composing the sheets have their lower walls thin and uncuticularised, as also are the walls of the stalk and passage-cells; whereas the rest of the epidermal cells have thick cuticularised walls. Again, terrestrial bromeliads which have absorptive roots possess scaly hairs which are incapable of being wetted. These facts prove that the peculiar scaly hairs of the epiphytic bromeliads absorb nutritive solutions. The upper wall of the scaly hair is thick and heavily cuticularised. It functions as a mechanism to prevent evaporation of water. Accordingly when those hairs are sunken it is relatively thin, but in protruding hairs it is thick. When the central cells of the shield contain air their lateral walls are crumpled so that the shield lies close above the surface of the leaf (thus preventing loss of water by evaporation); but when these cells absorb water, their walls straighten and thus raise the shield above the general surface.

It is impossible to mention all Schimper's interesting observations on this part of the subject. However, he points out that some terrestrial bromeliads possess absorptive scale-hairs, and he makes out a good case for the view that the possession of these scaly hairs in epiphytic Bromeliaceæ is not adaptive, but that these plants become epiphytic because they already possessed these curious hairs.

General Conclusions.

It has been pointed out that in the first three groups forming Class I. the nutritive solution is absorbed by the roots. In the first group no provisions are made for the collection of water or humus otherwise than from the surface of the host-plant. This group commences with the simplest forms which are entirely terrestrial in structure, and only grow on the lower parts of trees in damp and ill-lighted spots; but the rest of the group are characterised by a great development of the surface of the root- and leaf-systems. The leaves have a thick cuticle, etc., to avoid excessive transpiration: and internal water-reservoirs are usually present. But owing to the poverty of the supply of water and humus, and to the simple measures adopted in utilising these, the plants in this group are, for the most part, merely small herbs.

In the next two groups the xerophytic structure is, on the whole, less pronounced; for these forms have advanced and have adopted means to ensure a richer supply of food and water. In the second group the roots have become differentiated into two sets—attaching and nutritive. The latter descend to the ground. These forms can be directly derived from the first group, and intermediate types occur in which the nutritive roots reach the ground rather by chance than in virtue of any strong geotropic properties (*e.g.*, some Cactaceæ).

The third group has for the most part roots differentiated into attaching roots and absorptive roots which desert the substratum and often are negatively geotropic. The roots or leaves, or both, make provisions to collect humus. In the highest types not only are there two sets of roots, but there is a differentiation in the leaves, some of which are set apart to collect humus (species of *Platycerium*, *Polypodium*, and *Dischidia rafflesiana*). This group, too, is derived directly from forms like the first group.

The second class—the Bromeliaceæ—have been derived directly from terrestrial bromeliads. They have become epiphytic in virtue of the power which their leaves already possessed of absorbing nutritive solutions. They have been formed utterly independently of, and have pursued a course quite different to, that of the first class. Yet it is striking to note how the effect of the environment has been stamped on forms belonging to the two classes; and to see how the physiological anatomy of two forms may be identical and yet attained in a totally different matter. For instance, comparing *Tillandsia usneoides* with *Æranthus*, both are formed of pendent, green, assimilatory organs. Yet the one is wholly made up a shoot-system, and the other, practically, entirely a root-system. In both there is a general external absorptive mechanism—numerous scales and velamen respectively. The nutritive solution reaches the assimilatory tissue after first passing through a layer cuticularised except at certain points—the "exodermis," with thin-walled passage cells, and the epidermis with the passage cells under each scaly hair. Here and there free communication exists between the atmosphere and the assimilatory tissue—by stomata in one, and by air-containing patches in the other. The assimilatory tissue lies within the cuticularised layer. Rigidity is ensured in both by sclerenchyma-fibres. In both there is a feebly-developed vascular system, corresponding to the fact that each part of the whole surface is capable of assimilating and of absorbing nutritive solutions, and that hence conduction over long distances is not necessary.

Distribution of Epiphytes within their own Region.

Moisture is the first great factor which controls the distribution of Epiphytes. This is true, not only of the distribution of Epiphytes in their own region, but also over the world as a whole. It is not temperature which determines the presence or absence of Epiphytes. They are absent from deserts which have a tropical temperature; whereas shrubby representatives of this class are to be found on the Himalaya, in regions where the average temperature is below that of England.

This demand for moisture is well illustrated by the distribution of Epiphytes in England. Out in the open they are confined to a few lichens, minute algæ, few mosses, and a couple of sorts of liverworts. But in woods, where the air is damper, the number of these epiphytic forms is much greater, and occasionally ferns are found epiphytic. Where dampness of the air is combined with good illumination, there is a still richer epiphytic flora; for instance, on pollard-willows lining a river-bank there are many Epiphytes, some of which attain the size of bushes (Rose, etc.).

So, in the tropics, the moisture-laden jungles and primæval forests abound in Epiphytes; on the other hand, the open savannahs have but few.

The second controlling factor is *light.* The demand for light drives Epiphytes to the tops of the trees in a tropical forest. In wandering through one of these forests it may seem as if there are but few Epiphytes, till one espies their great aërial roots descending to the soil and their fallen flowers and fruits dappling the ground.

To a certain extent, light and moisture are wants which the Epiphyte cannot obtain at the same time to the fullest extent. In general, one may say that shade involves a moister atmosphere and illumination a drier one; in addition, the stronger the light, the more rapidly does a leaf transpire. Hence, in a tropical (American) forest, one finds the Epiphytes ranged at different heights up the trees, and the same forms constantly occur in the same positions. Low down in the shade, on the trunks or thick lower branches, there are but few. These are moisture-loving and shade-bearing species, especially Hymenophyllaceæ, Lycopodiaceæ, delicate *Peperomias* and orchids (*Zygopetalum*), green bromeliads. On the tree-tops are forms which can endure strong light, and are constructed so as to withstand the danger arising from excessive transpiration (grey *Tillandsias*, thick-leaved orchids without pseudo-bulbs, leathery polypodies). Between these two extremes, on the branches below the tree-tops, the richest and most varied epiphytic flora prevails. It is at this level that the giants among Epiphytes reign, and countless orchids with pseudo-bulbs flourish.

Going out into the tropical American savannahs we find the Epiphytes far fewer. They are largely identical with those growing at the extreme tops of the forest-trees. Only a few forms from the middle stratum of Epiphytes occur, but none from the lowest stratum.

Corresponding to the above-mentioned facts, we find that densely-foliaged trees have few or no Epiphytes on them (Mango), unless indeed the tree have deciduous leaves (South Brazilian fig trees), in which case numerous Epiphytes occur.

The *nature of the surface* of a plant also determines the number and sort of Epiphytes which lodge on it. Only a few forms can exist on very smooth surfaces. These are chiefly Bromeliaceæ, which may be found on polished stems of palms, in glassy shoots of bamboos, on leaves, and on smooth aërial roots. They are the pioneers among Epiphytes, as they germinate on smooth spots unsuitable to other forms. Among their roots these more exacting plants settle, so that a colony of Epiphytes springs up round the originally solitary bromeliad.

On the other hand, there are forms which cannot live except on very uneven surfaces, on rough or mossy bark, in the persistent leaf-sheaths of palms (especially ferns).

Most Epiphytes avoid trees with a peeling or scaling bark.

In addition, the distribution of Epiphytes on plants depends on causes not yet understood. For instance, an epiphytic orchid in North Florida especially affects *Magnolias*. *Crescentia cujete* is a tropical American tree which is the greatest favourite with Epiphytes. Schimper suggests that this is due to the facility with which root-hairs penetrate its soft superficial cork.

The Evolution of Epiphytes.

We are now in a position to trace out the mode in which terrestrial plants became Epiphytes in tropical America, and similarly in other tropical regions.

In the primæval forests one sees terrestrial forms allied to the epiphytic species growing at different heights up the trees. This is not so in the open country, say in the savannahs, where the Epiphytes are allied to those occurring in the tops of the trees in the forests. This fact, and the general principle that moisture of the air largely determines the presence, or absence, of Epiphytes, leads us to the result that Epiphytes were evolved in the regions of moisture —the forests.

There are now species which grow in the forests as terrestrial plants or as Epiphytes, but in the latter case they invariably grow in moist gloomy spots on the lower part of trees; and, in general, shade-loving forms occupying positions low down on the trees are the simplest and least modified Epiphytes. They are the most recent recruits in many cases.

Thus to us comes the idea that when Epiphytes started their aërial mode of existence, they commenced in these shady moist positions not far above the ground. I may suggest that this view of Schimper's receives support when one particular group of Epiphytes is considered—that is, the group in which some of the roots descend to the ground, often having to be more than a hundred feet long to attain this. The evolution of such roots is absolutely inconceivable, unless we suppose that the epiphytic ancestors of the plants possessing them were once much nearer the ground than their present representatives are.

The plant, then, having become an Epiphyte occupying the lower regions of the forest, its next demand would be to get into more strongly-lighted places so as to assimilate at a greater pace. So the next step was for the Epiphytes to climb higher up the trees. With this arose the necessity for provision against excessive transpiration and for more elaborate organs for absorbing water. Gradually, then, the Epiphytes crept up the trees, becoming more adapted to their habitat as they struggled with one another (and with their hosts) in the fierce battle for light. In this manner they reached the higher branches of the trees—but below the tree-tops. This is the paradise of the Epiphytes; the light is not too intense, but filters through a veil of leaves.

To escape their many competitors, a few bolder spirits advanced almost into the full blaze of the sun on the tree-tops, taking measures, as they ascended, to avoid being parched. From these points of vantage, some of them, and a few from the middle stratum, winged their way to the savannahs, others climbed mountains and became tropical alpine forms living on the ground (1). Still others journeyed to the sea-coast, and descending to the ground became littoral forms with their roots dipping into the salt-laden soil (1). A few which have entirely, or almost entirely, deserted their epiphytic mode of life bear structural evidence of their former epiphytic mode of existence (6).

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Percy Groom.

II.

On the Relation of the Fauna and Flora of Australia to those of New Zealand.

THAT the flora of New Zealand should present so many features akin to that of Australia, and yet entirely want the distinctive botanical characters of its nearest continent, is a problem to which Hooker first drew attention. No solution was attempted by that writer, who limited his discussion of the question to a clear statement of the facts in a passage [1] so frequently quoted that it need not be here repeated.

The explanation offered by Wallace in "Island Life," and generally accepted, is: (1) commencing the biological history of Australia with the Cretaceous era, that Eastern and Western Australia were then totally severed; (2) that Eastern Australia was at that time quite devoid of a typical Australian terrestrial fauna and flora, which was then confined to Western Australia; (3) that a large area of what is now the floor of the Tasman Sea was upheaved, and nearly, or quite, connected New Zealand with Australia, whereby the flora and fauna, then existing in Eastern Australia, were enabled to colonise New Zealand; (4) that this hypothetical bridge then sank, isolating the New Zealand colonists, and afterwards dry land appeared between Eastern and Western Australia, upon which the characteristic Australian forms first crossed from west to east.

Apart from the difficulty they are supposed to satisfy, little or no evidence is advanced for the support of these propositions. That a Cretaceous sea separated Eastern from Western Australia may be granted, but that at any time Western Australia possessed a monopoly of characteristic Australian plants or animals is improbable. Recent discoveries show that the dry land representing Western Australia in Cretaceous times was much less than Wallace supposed, and than appears in the sketch map on p. 497 of the second edition of "Island Life." In the region explored by the Elder Expedition, "an area of Mesozoic formation extends as far as long. 123°." [2] The shallow inland Cretaceous sea was studded with islands, large and small, which served the fauna and flora as stepping-stones in their migrations from west to east and from east to west. Some of these are now

[1] J. D. Hooker, "Introductory Essay to the Flora of Tasmania," pp. lxxxviii.-ix.

[2] V. Streich, "Scientific Results of the Elder Exploring Expedition, Geology," p. 88.

represented in Central Australia by the Macdonnel, Musgrave, and Everard Ranges;[3] in Queensland by an extensive area of Palæozoic schists, extending almost from the Gulf of Carpentaria to beyond Cloncurry, reappearing in the south as the Grey and Stokes Ranges,[4] and yet again in New South Wales as the Barrier Range. If allowance be made for denudation, more islands probably then existed than geological maps now indicate. Tertiary fossils from Victoria, N.S.W., and Queensland show as marked an Australian facies as recent plants and animals. Such are the various species of *Banksia* from the Lower Miocene.[5] The very characteristically Australian remains preserved in the Darling Downs' deposits cannot, says De Vis, be considered as of later date than Early Pliocene.[6] Finally, many genera and species among recent plants and animals essentially Australian are confined to Eastern Australia. Wallace correctly remarks that West Australia is richer than the East in peculiarly Australian plants, but when, as described later, the archaic Queensland flora was submerged and almost extinguished by the Papuan invaders, great numbers of such peculiarly Australian plants must have perished in Eastern Australia.

More in need of confirmation than the preceding propositions stands the theory that a large tract of the Tasman Sea connecting Australia with New Zealand was upheaved, and when it had served its purpose conveniently, subsided at the close of the Cretaceous. Except about the twentieth parallel, where a bank no deeper than 1,300 fathoms connects the Great Barrier Reef with New Caledonia, the shores of Eastern Australia everywhere front deep water of over 2,000 fathoms, to the north the abyss of the Coral Sea, to the south that of the Tasman Sea. The geological history of this coast, as opposed to that of New Zealand, indicates great stability; such a vast elevation and subsequent depression as Wallace demands could scarcely have occurred without leaving signs decipherable by geologists. If the upheaval was local instead of general, it must, to satisfy the theory, have taken place between Bowen and Rockhampton, where the Caledonian Barrier Bank abuts on the Queensland coast. No consequent distortion of the strata appears to have been there observed by local geologists.

Upon detailed examination, it appears that although the fauna of New Zealand sometimes approximates to that of Queensland, yet it could not have been derived therefrom; that, in short, the relation is not that of mother and daughter, but that of sisters. An analysis of the Australian land shells shows the range of the operculated forms

[3] V. Streich, *Op. cit.*, p. 80.

[4] *Vide* sheets 3, 4, and 5 of the Geological Map issued with "The Geology and Palæontology of Queensland and New Guinea." By R. L. Jack and R. Etheridge, Jun.

[5] Ettingshausen, "Contributions to a Tertiary Flora of Australia," pp. 138-143.

[6] C. W. De Vis, *Proc. Linn. Soc. N.S.W.* (2), vol. vi., p. 456.

to be restricted to the coast of Queensland, and that, proceeding from south to north, the group becomes gradually richer in species and in genera. In the New Zealand fauna the operculates are represented by a tenth of the snail fauna as compared to none in Australia, except Queensland, to one-sixth in Queensland, and one-third in New Guinea. So far our investigation conforms to Wallace's theory, and as that demands an origin in Australia for the New Zealand fauna, so we expect the operculate land mollusca of New Zealand to be descendants of those of Queensland. Here, however, facts rebel against the theory, the New Zealand species being ranked under *Lagocheilus*, *Realia*, and *Hydrocena*, while the Queensland ones are contained in *Georissa*, *Truncatella*, *Pupina*, *Callia*, *Pupinella*, *Hedleya*, *Ditropis*, *Diplommatina*, *Leptopoma*, and *Helicina* ; than which two faunas could hardly be more distinct.

Most European writers who have touched on the zoo-geography of Australia have described the fauna and flora as falling into a temperate and a tropical division, which again subdivide into eastern and western sections. A little real experience proves these divisions to be quite artificial. The dry interior, for example, is everywhere inhabited by the same plants and animals, regardless of latitude or longitude; examples are, the great red kangaroo among mammals, the plain turkey, the maller hen and the rosy cockatoo among birds, the frog *Notaden bennetti*, the lizard *Lygosoma monotropis*, and the snakes *Pseudonaja nuchalis* and *Hoplocephalus nigriceps*, among plants the predominance of salt-bush, spinifex, gidya (acacia), and *Frenela*. On the eastern seaboard, the difference between north and south is that between a conquering and invading fauna and flora and the indigenous population where not exterminated or obscured by them. The types encountered by a traveller in tropical Queensland, or rather in that narrow belt of tropical Queensland hemmed in between the Cordillera and the Pacific, all wear a foreign aspect. Among butterflies the *Ornithoptera*, among reptiles the crocodile, among birds the cassowary and rifle bird, among mammals the tree kangaroo and the cuscus, and among plants the profusion of epiphytic orchids and of palms, all point to a northern origin. In the heart of a great Queensland "Scrub" a naturalist could scarcely answer from his surroundings whether he were in New Guinea or Australia.

Late in the Tertiary Epoch, as I read the record, Torres Straits, now only a few fathoms deep, was upheaved, and across this bridge there poured into Australia a stream of Papuan life. Between the coastal range and the sea as far as the tropics the irruption flowed in undiminished strength ; on reaching the border of N.S.W. the cooler climate diminished its vigour, and at the Clarence River, N.S.W., with few exceptions, it found its southern limit. Within this area grow side by side, like oil and water—touching yet not commingling, two distinct vegetations, the dense Papuan jungle called "Scrub" by the Queenslanders, which has usurped every rich volcanic upland and every fat alluvial plain, and the lightly timbered "Forest" of acacias and eucalypts confined to the inferior soils. As sharp a line is drawn between "Scrub" and "Forest" as a European may see between an Alpine meadow and a thick pine wood.

In considering the contour of the floor of the Western Pacific in relation to the distribution of species, we are greatly hampered by want of information. Too few and distant are the deep-sea soundings to determine the trend of the numerous banks and troughs of this region. Something can, however, be deduced from the "Challenger" discoveries. When that vessel ran a line of soundings across the Coral Sea, she found that the temperature of the soundings diminished down to 1,300 fathoms, below which depth it was stationary, proving that no cold stream from the depths of the Central Pacific or Antarctic Oceans could penetrate and chill this basin;[7] from which it is to be inferred that no gap deeper than 1,300 fathoms crosses the rampart extending from the Solomons to New Zealand. This region, in contrast to Australia, is one of great volcanic activity, and there are evidences of great fluctuation of levels in the past. If we consider that this track was ridged up by the contraction of the floor of the South Pacific, the fact would be accounted for that the various earth-waves of this plateau—for example, the Southern Alps of New Zealand—all curl, or have their steepest face towards Australia; and this argument, if granted, would render more probable the former existence of a continuous land-surface from the Solomons to New Zealand. It would explain the undisturbed condition of the Australian Tertiary and Mesozoic beds, which escaped the shocks of contraction, as compared with those of New Zealand which received them. Holmes and Hinde assume from the contained sponge remains that the siliceous beds of Oamaru, New Zealand, were formed at depths of not less that 6,000 to 9,000 feet in Eocene seas[8]; while Guppy considers that since Post-tertiary times the Solomon Archipelago has been upheaved at least 12,000 feet.[9] The vertebrate fauna of the Solomons evinces a recent descent from Papuan types, apart from which it shares with the New Hebrides and New Caledonia that remarkable poverty of mammalian and reptilian forms so distinctive a trait of New Zealand. Besides this negative evidence may be adduced the instructive distribution of *Placostylus*. This large Bulimoid snail forms one of the most striking components of the snail fauna of New Zealand, Lord Howe Island, New Caledonia, the New Hebrides, the Solomons, and the Fijis. This distribution appears to be in no way connected with trade winds or ocean currents. On the supposition that the remoter colonies of *Placostylus* were seaborne emigrants, it would be difficult to explain how, when Lord Howe was populated from New Zealand

[7] Voyage of H.M.S. "Challenger," narrative, vol. i., p. 519.

[8] *Journ. Linn. Soc.*, vol. xxiv., pp. 178 and 255.

[9] H. B. Guppy, "The Solomon Islands, their Geology, etc.," p. 126.

or New Calendonia, that the emigrants who had successfully traversed the wide and shallow portion of the Tasman Sea should be stayed by the narrower but profound waters from reaching Australia. Similarly, a narrow but a deep strait intervenes between Fiji with numerous species and Samoa with none, between the Solomons where the genus is well represented and the Louisiades where it is absent. The beaches of Queensland, as I can testify from personal observation, are strewn by a constant drift of pumice accompanied by coco-nuts and pearly nautilus shells. The coco-nuts might have floated from any tropical island in the Pacific, the nautilus shells are only derivable from the narrower limits of the Solomons, the Fijis, and the New Hebrides, while the pumice must almost certainly be the product of the active volcanoes of the New Hebridean group. It is therefore obvious that in the Western Pacific the chief route of drifting objects is that of the trade winds from N.E to S.W. To an objection that the range of *Placostylus* might be due to transmarine migration, it is a sufficient answer to observe that this genus extends beyond and across, but not parallel to, the direction of the principal agency of dispersal in this region. Both birds and plants contribute evidence of the homogeneity of the fauna and flora of these islands, and of their distinctness from those of Australia. Among the former may be quoted *Merula* and *Aplonis*, and especially the distribution, past and present, in Lord Howe, Norfolk Island, and New Zealand of. *Nestor*, *Platycercus*, *Notornis*, and *Ocydromus*.[10] Of the botany of Lord Howe Island it is especially to be remarked that, whereas on Wallace's theory it should, lying nearest to Australia, be most akin to that region, yet "those typical Australian families the Leguminosæ and the Myrtaceæ are barely represented, whilst the Proteaceæ are said to be wholly wanting."[11]

Upon these grounds I conjecture that an ancient continent, separated on the west from Australia by the abysses of the Coral and of the Tasman Seas, is represented by the Solomons, the Fijis, the New Hebrides, New Caledonia, Lord Howe, and New Zealand, with its outlying islands—an area that I have elsewhere[12] proposed to call the Melanesian Plateau, and upon part of which Forbes has more recently conferred the name of Antarctica.[13]

In conclusion, I would contend that New Zealand is associated with the Solomons and the New Hebrides, firstly, as a member of their volcanic system; secondly, by community of fauna and flora; whereas to Australia it is related not at all physically, and to a foreign and intrusive element biologically; and that a theory which derives the fauna and flora of New Zealand primarily from these archipelagoes and remotely from New Guinea, necessitates fewer unproved assumptions than that which derives them from Australia.

C. Hedley.

[10] R. Etheridge, Jun., "Lord Howe Island, General Zoology," p. 13.

[11] R. Etheridge, Jun., *op. cit.*, p. 108. [12] *Proc. Linn. Soc. N.S.W.* (2), vol. vii., [p. 335.

[13] See Natural Science, vol. iii., p. 54.

III.

Recent Researches on the Fauna and Flora of Madagascar.

THE remarkable faunal riches of this African island have been chiefly made known in M. Grandidier's volumes, but there is still much material at Paris lost to science for want of someone energetic enough to work it out. Particularly so is this the case with the Mammalia, and the delay in publication is not only vexatious to the zoologist, but allows others to describe as new, recent accessions, of which specimens are probably buried in Grandidier's collections.[1] The practice, too, of issuing volumes of plates unaccompanied by descriptions, may, perhaps, be useful when dealing with the larger animals, but is absolutely valueless (even to secure priority) for insects or plants in which so much depends on microscopic characters, and can only lead to endless confusion in nomenclature.

The latest information about the Madagascar fauna as a whole may be found in chapter xix. of Wallace's "Island Life" (ed. 2, 1892) ; but since that time, thanks to the liberal-handedness of Mr. Grose Smith and Mr. Walter Rothschild, material as remarkable for its abundance as for the peculiar value of the specimens has been received in this country ; nor must it be forgotten that other workers, such as the Rev. R. Baron and M. Gautier, are largely interesting themselves in the accumulation of material which, though less in quantity, is every bit as valuable as the collection made by Mr. Last. Indeed, one may safely say that any remains, whether fossil or recent, if properly localised and carefully packed, add new light to our imperfect knowledge of this great island.

Mammalia.—The remains of Hippopotamus from the Post-tertiary deposits of the island have been known since the time when Murchison exhibited to the Geological Society in 1833 a tusk and molar tooth from a conglomerate, some 30 miles from Antananarivo. Further remains of what may be considered to be the same species were described by Grandidier and Milne-Edwards in 1868 as *H. lemerlei.* Portions of the skull of another individual of possibly the same

[1] We understand that a monograph on the genus Lemur, by Milne-Edwards and Filhol may shortly be expected, accompanied by 60 anatomical plates, when we hope the plates issued in 1890 will be properly described.

species have recently been received in London, and will form the subject of a paper by Dr. Forsyth Major.

In June last, Dr. Major read before the Royal Society a paper on an extinct Lemuroid from Ambolisatra, a slightly imperfect skull—with a right and left mandibular ramus—of which has been sent to this country by Mr. Rothschild's collector, Mr. Last. This skull, belonging to an animal approximately three times the size of any described Lemuroid, has been named *Megaladapis madagascariensis.* It is characterised by an enormous lateral development of the anterior inter-orbital portion of the frontals; a comparatively narrow and elongated post-orbital frontal region, separated by a slight contraction from the equally narrow parietal region; a thick and flattened sagittal, and an equally strongly developed occipital crest. The zygomatic arch is high and projects moderately outwards. The general appearance of the skull and teeth indicate an old individual. The brain-case is comparatively small in size, and is viewed by Dr. Major as a degenerate feature, and he anticipates that in young specimens the cranium would be more rounded in the cerebral region, more voluminous, and the facial portion much shortened.

Evidence has also been forthcoming of the former existence of a ruminant, but the remains are too imperfect to permit of identification. The colossal hippopotamus and rhinoceros (six times the size of an elephant) of M. Hamelin (*Standard*, 25 July) are imaginative, and the reported destructive powers of the *Cryptoprocta*, which attains a size about equal to that of a lynx, is, we venture to think, somewhat exaggerated.

Aves.—Many remains of *Æpyornis* have been received during recent months, some of which—sent to Mr. Rothschild, and exhibited by him at the Zoological Society—far exceed in size any specimens previously recorded. An egg of this bird was sold in a London sale-room a few months ago in a perfect condition, realising the sum of sixty-seven pounds, which may be considered as quite a fair price. The most recent information on the genus will be found in Burckhardt's paper on some remains which have reached Berlin from Sirabé, and here the pelvis, the upper portion of the metatarsus, and the immature metatarsus have been made known for the first time. A metatarsal of a large eagle has been found with the remains of hippopotamus at Ambolisatra.

Reptilia.—In the May number of the *Geological Magazine*, Mr. R. B. Newton described and figured portions of a rostrum and mandible of a crocodile from the Jurassic rocks of Andranosamonta. This fossil, which was obtained by the Rev. R. Baron, has been named *Steneosaurus baroni.* It was associated with mollusca, referable to *Mytilus* (near *M. tigrensis* from Abyssinia), *Modiola*, *Perna*, and *Trochactæonina*; and since the occurrence of this genus was, in our imperfect knowledge, limited to European areas, the discovery is of the highest importance in questions of geographical distribution.

Remains of the huge land tortoises, named by Vaillant *Testudo grandidieri*, have been lately received, and two fine specimens have been placed in the Palæontological galleries of the British Museum.

In Mr. G. A. Boulenger's "Catalogue of Snakes in the British Museum" (Natural History), vol. i. (1893), information will be found regarding the Madagascar forms; and the book itself will be found invaluable to anyone studying Ophidia.

PISCES.—Dr. Sauvage published in 1891[2] the description of the fishes from Grandidier's collection, and this text, with the two volumes of plates issued in 1887 and 1888, completes our present knowledge of the group.

MOLLUSCA.—The Rev. A. H. Cooke, dealing with the distribution of the mollusca in the Malagasy Region, notes the great development of the carnivorous land mollusca, the occurrence of large numbers of true Helicidæ of great size and beauty, and the prominence of the genus *Cyclostoma*. The molluscan fauna of Madagascar, even as imperfectly known, appears to possess sufficient individuality to separate it off distinctly from that of the main-land. The Helicidæ are quite peculiar, not being found at all in the Mascarenes, Seychelles, or Comoros. They seem to be rather related to the *Acavi* of Ceylon and the *Pandæ* of N.E. Australia. Fifty-four species of *Cyclostoma* are known from the group, but are not confined to Madagascar, being distributed over Comoros, Seychelles, Mauritius, and Bourbon as well. The African *Bulimini* are represented by two species, but *Achatina* is scarce. Two groups of Bulimini (*Leucotænia* and *Clavator*) are peculiar. A single species of *Kaliella*, identical with a common Indian form, is also recorded. In the fresh-water forms unmistakable traces of Indian relationships are found. There are two species of *Paludomus*; *Bithynia* is recorded; several of the *Melaniæ* are of a type common in the Indo-Malay Region, while the *Melanatriæ*, quite peculiar to Madagascar, have their nearest affinities in Ceylon or East India. Not a single one of the characteristic African fresh-water bivalves has yet been found in Madagascar. Several African genera of gasteropoda occur, and indicate, in common with the land mollusca, as Mr. Cooke points out, that the land connection of Madagascar with Africa must have taken place, but that it occurred at an immeasurably remote period. References to recent literature are given in Mr. Cooke's paper.

INSECTA.—Since the publication of the Lepidoptera (1887) from Grandidier's collections, there have been issued three parts of his great work on Madagascar, devoted to the Hymenoptera. These contain both text and plates. A second series of plates of the Coleoptera was issued in 1890, but, in the absence of the text, the new species can only be considered in the light of manuscript names.

FLORA.—The latest information respecting the botany of Mada-

[2] NATURAL SCIENCE, vol. i., p. 152.

gascar will be found in the *Journal of the Linnean Society* for 1890. In this volume Mr. Baron gives a general sketch of the flora, dividing the island into regions, and treating of the geographical distribution. The affinities of the plants with those of America, the fruits, cereals, and vegetables, garden trees and shrubs, are all treated of by Mr. Baron, who also gives an appendix of introduced plants. Mr. J. G. Baker contributes to the same volume descriptions of 160 new species collected by Mr. Baron, of which four belong to new genera. A fourth part of Grandidier's great work, dealing with the plants, was issued in 1892, but the foolish practice of issuing plates without text makes this publication of little value at present.

The Mosses and Hepatics are treated of by Stephani, who publishes technical descriptions, with figures of new species.

Geology.—The geology of Madagascar is best known from the researches of Cortese and of Baron. Cortese supplies a map of the whole island, while Baron deals only with the northern half. Roughly speaking, a line drawn from north to south, and dividing the island longitudinally, shows a granitic and a volcanic area on the right side, and a Secondary, Tertiary, and Recent area on the left side. The crystalline area consists of granite, gneiss, syenite, diorites, amphibolites, and basalts; the sedimentary series, by their included fossils, representing the Jurassic, Upper and Middle Cretaceous, Nummulitic series, and Recent deposits. Much blown sand occurs round the coast, and numerous old lake basins are found, one of which, according to Mr. Baron's map, must have been nearly 300 miles along. It is to these old lake basins that we mainly look for evidences of the ancient fauna of Madagascar. The petrological characters of the rocks have been described by Cortese, while Hatch has dealt with those specimens brought to England by Baron. For general information on the fossil fauna, see Newton's two papers, quoted below, with the references therein contained; and a paper of Stanislas Meunier in *La Naturaliste*, for Aug. 1, 1893. In this last paper figures are given of several Cretaceous oysters from Mahamayo, recently received from M. Gautier, and some general notes on Malagasy geology are appended.

Geographical and Zoological Relationships.—The observations of Mr. H. O. Forbes, which include Madagascar, and of which the essence was given in this Journal for July, may be compared with those of Rütimeyer "Ueber die Herkunft unserer Thierwelt" (4to, Basel und Genf, 1867), and with those of Emile Blanchard, "Les preuves de l'effondrement d'un continent Austral pendant l'âge moderne de la terre" (*Comptes Rendus*, vol. xciv., 1882, pp. 386 and 395); while much general and particular information will be found in Dr. Blanford's address to the Geological Society, and in his review of Boulenger's "Catalogue of Snakes," in *Nature*, Aug. 3, 1893.

Hans Gadow, in the last number of Bronn's "Klassen und Ordnungen des Thierreichs" (vol. vi., pt. 4, 1893), has some observations on the geographical distribution of the Ratite and rail-like birds.

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C. Davies Sherborn.

IV.

The Interlocking of the Barbs of Feathers.

THE exquisitely beautiful, and remarkably modified epidermal products, which we know as feathers, have probably received more attention than any other form of clothing in the animal kingdom. Science, Commerce, Poetry, Art, and Fashion have each in turn laid them under contribution, a fact which, it will be readily admitted, has been beneficial to all but those most immediately concerned—their possessors, the birds.

Now we have just implied that feathers are structures with which the world in general is tolerably familiar. To a certain extent this is true; nevertheless, I have ventured to present the readers of NATURAL SCIENCE with what I hope will prove a concise account of the mechanism, by which the characteristic unity and elasticity of the vane of a typically perfect feather is secured, believing that this will be received not altogether as stale news. I wish it, however, to be distinctly understood that, for the most part, what I have to say will be found little else than a restatement of a tale already told, though in one or two somewhat minute points my claim to originality will, I believe, be allowed to pass unchallenged.

Before, however, proceeding to the more special subject of this article, it may be well to direct attention briefly to the accompanying diagram of a feather. (Fig. 1.) There may be noted (1) the strong pliant *scapus* or stem (*Sc.*), divided for convenience sake into two parts, (α) a *rhachis* (R_1) or distal, and (β) a *calamus* (C.)—commonly called the quill—or proximal portion, and (2) the *rami* or *barbs* (R^1), lanceolate processes, seated in two rows, one on either side of the rhachis, and pointing to the tip of the feather. The interstices between the rami, it will be observed, are filled by exceedingly numerous and very small processes, looking to the unaided eye in the natural feather more like bristles, perhaps, than anything else; these are the *radii* or *barbules* (R.), and it is mainly with these that we have to deal in explaining the structure of the *vexillum* (V.) of a feather. The vexillum, it must be remarked, is the compact, well-knit, elastic fringe or web ("beard" as it is called in German), composed of rami and radii interlocked in a manner to be described presently.

It has just been remarked that the interstices between the rami are filled up by exceedingly small processes called radii; of these, there will be found two rows to every ramus, one directed towards the tip of the feather—the distal series—and one looking towards its outer or inner edge, as the case may be, and neither to the tip, nor the base of the feather, as has been described. Now if we press a sharp knife along the edges of one of these barbs, we shall succeed in getting a number of radii of two quite distinct varieties, though they have this much in common, both may be likened to lamellæ which have undergone the process familiarly known as "warping," in a longitudinal direction. Those of the distal series (Fig. 4) are found to be lamellate for about half their length, when the lamella might be imagined to have been cut up into a number of long, slender, filiform processes, some of which are hooked at the tip. If hooked, they are called *hamuli*, otherwise they are commonly known as *cilia*. The hamuli are confined to the ventral edge of the radius, but the cilia may occur on both dorsal and ventral edges.

The radii of the proximal series will be found, in addition to the warping, to have their upper edges folded somewhat sharply over towards the hollow surface, as in Fig. 3. After rather more than half of its length, this edge gives place to a series of notches, which soon—rapidly diminishing in size—blend with the almost and often quite filiform tip, into which the distal moieties of these radii are produced. It must here be noticed that, in the region of the notched edge, the radii are bent at a slight angle upon themselves, the bent ends being closely approximated, as shown in the figure.

So much for the general conformation of the radii. What is the reason for this warping? and what is the actual method of interlocking? are questions that my readers will here naturally ask.

Probably a glance at Fig. 2 will explain more than could ever be done by mere description. Here we have a semi-diagrammatic representation of an oblique section across two rami (R), taken parallel with the distal radii, or what amounts to the same thing, parallel with the shaft of the feather (P). As I have hinted, the figure tells its own story, but I should like to draw attention to the following details.

It will be observed in Fig. 2 that the proximal halves of the distal radii appear as flattened, overlapping laminæ, which at about the middle of their length suffer a sudden twist in a vertical direction, then breaking up into the hooklets and cilia previously described. These hooklets, it will be seen, are thrust down between the scroll-like upper margins of the proximal series, between which there is only just room enough to allow the hooklets to pass. The proximal radii are somewhat puzzling, perhaps, because seen in section; this, however, is unavoidable, as a reference to the diagram, Fig. 1, will, I think, make clear. The true relation of this series having been grasped, there is little else to note for the moment, except the folded upper edge, whose function is obviously to afford a secure hold to the hooklets, just

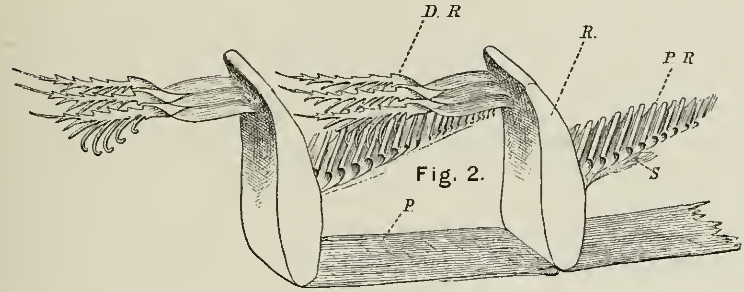


Fig. 2.

Section through 2 rows parallel to the distal Radii.

D. R. Distal Radii P. R Proximal Radii.

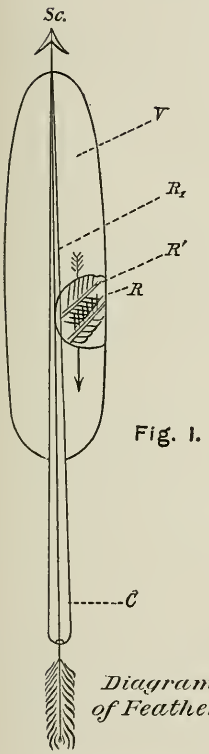


Fig. 1.

Diagram of Feather.

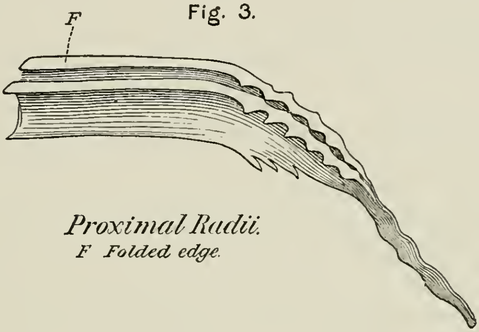


Fig. 3.

Proximal Radii.
F Folded edge.

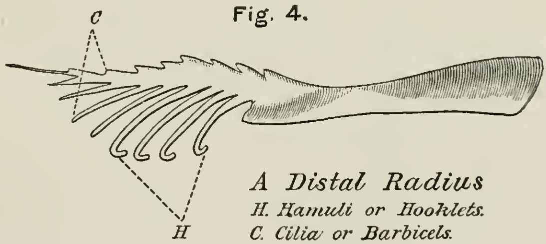
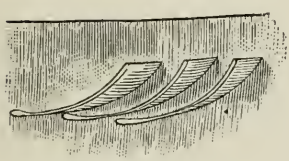


Fig. 4.

A Distal Radius
H. Hamuli or Hooklets.
C. Cilia or Barbicels.



Section through the bases of 3 Distal Radii

Fig. 5.

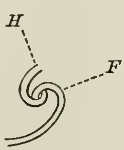


Fig. 6.

Magnified Section through a Hooklet and folded edge of a Proximal Radius.

F. Folded edge.
H. Hooklet.

DIAGRAMS OF FEATHER-BARBS.

described as being thrust down between them; and the formation of comparatively deep channels by the approximation of their lower edges. These channels are not maintained far, if at all, past the most proximal hooklet; from this point, it will be seen, these sectionised radii diminish in size, terminating close against the surface of the next ramus.[1] Now it only remains for the reader to draw upon his imagination, to see at once that a comparatively severe strain would have to be exerted upon the rami to pull them apart, and if he will only make the experiment on some large quill feather, he will be amply satisfied; then let him sever a few rami at their base, and see how easy it is to separate them by pulling the ends in opposite directions, the hooklets, of course, gliding along the grooved edges.

Now a word as to the "warping." The radii of both distal and proximal series are, it will be remembered, longitudinally curved, or "warped," as I have termed it, but in Fig. 2 the radii have the appearance of *flattened* not curved laminæ! How is this? Well, I have chosen to give the appearance they present under the microscope when seen *in situ*, rather than avail myself of the licence allowed me in adopting a semi-diagrammatic figure, for the sake of those who take up this work for the first time; for unless seen in section, it is difficult, if not impossible, to consider these anything but what they appear—flattened lamellæ, seated obliquely along the ramus, and even examination under high powers does not reveal the true state of things. Their actual form appears in section as in Fig. 5, which represents three radii cut off close to their base.

As touching the "channels" of the proximal series, what we have now to do is not so much to tender a reason for their formation—this being rather too presumptuous, as to offer a suggestion to that end. The first thing that will occur to the thoughtful reader is, if these edges were free, and directed straight downwards, might they not be a source of danger to the hooklets, inasmuch as the latter would perhaps frequently catch their lower instead of their upper edges; while the present arrangement suggests a twofold purpose: firstly, to afford a wind-proof surface for the downstroke of the wing (in the case of the remiges and flight feathers), thus preventing the displacement of the hooklets by the upward rush of air; and, secondly, to conduce to that marvellous elasticity of the feather we paused to admire in the earlier part of this paper, the hooklets gliding freely over the smooth, hollow surface, yet with difficulty letting go their hold. Surely, there seems to be good reason for these views. At any rate, just such an interpretation as this seems to find favour with so good an authority as Dr. Gadow. I find on reference to his most valuable work the following remarks, which, by the way, however,

[1] These diminished portions, it should be noted, are the distal ends of radii arising nearer the proximal end of the ramus. A moment's reflection will show—bearing in mind their oblique direction—how, that being placed one in front of the other, the sections will gradually decrease.

leave us in a little uncertainty as to whether the full extent of the curvature of the radii is appreciated or not. Thus (quoting Klee) he says, "Ihr *oberer Rand*[2] ist einfach umgeschlagen, wodurch längs dieses Randes eine vollkommene Rinne zu Stande kommt, die unter dem Mikroskop allerdings den Eindruck eines verdickten Randes macht." And again, speaking of the elasticity of the feather, he says, "Zugleich ist die Fahne in höchsten Grade elastisch, nicht nur durch die Elasticität aller einzelnen Federtheile, sondern weil die Hacken auf den glatten Strahlen *Kanten*[2] hin—und hergleichen können."

The relative position of the radii, like their form, varies. Both distal and proximal rows may arise from what would otherwise be the ridge of the ramus, or the distal radii may arise from the upper border of the ramus, while the proximal may run along quite near the lower border. Frequently the distal radii are overhung by a projecting ledge, formed by the much compressed ridge of the ramus, and between these every possible gradation may be found, according as the part examined be from the primaries, secondaries, or coverts, or from the inner or outer margins of the vexillum.

Along the lower edges of the rami in Fig. 2 will be noticed a thin transparent band (P) of a width corresponding to the section. This I recently found in the remiges of a Teal, *Querquedula crecca*. The free edge may be either closely approximated to, or even fused with, the lower edge of the next ramus. To the naked eye, this appears along the side of the shaft as a narrow band of silvery whiteness, and very smooth, but I am at a loss to account for its function, and do not care to hazard a guess. I have indicated the presence of a similar band under the proximal radii; it seems, however, to occur but rarely. Mr. R. S. Wray, to whom we shall have occasion to refer presently, makes mention of such a band in his paper, shortly to be noticed.

I propose now to present a sort of census of opinion as to how the "Interlocking of the barbs of a Feather" was obtained; but, to be brief, it will be well to confine attention to the descriptions of some three or four writers whom we may consider to have been more or less well-qualified to speak on this subject. This will carry us back some sixty years, to the time of the great Nitzsch, the father of the science of Pterylography. His description was fairly accurate; the wonder is it was not quite so. He perceived that the radii could be divided into two series—an anterior and a posterior, as he called them, but he failed in his endeavour to explain their mutual relations, considering that the hamuli of the anterior radii were for the purpose of catching into "little pits" prepared for their reception in the proximal radii. Burmeister, editing Nitzsch's work a few years later, exploded this theory, and gives essentially a correct explanation. He says, "I have noticed in all parts a strongly-thickened, superior margin of such a size that the hooklet can just grasp it. It is behind this margin . . . . that the hooklets take hold."

[2] The italics are mine.

Owen, writing some thirty years later, gives a much less truthful description. He says, "The barbules arising from the upper side of the barb, or that next the extremity of the feather, are curved downward or toward the internal surface of the shaft; those which arise from the underside of the barb are curved in the contrary direction; so that the two adjoining series of hooked barbules lock into one another in a manner which has been compared to the fastening of a latch of a door into the catch of a door-post."

Huxley's description is as follows:—"The barbules may be laterally serrated and terminated by little hooks which interlock with the *hooks*[3] of the opposed barbules."

The last and most authoritative account is that of the late Mr. R. S. Wray. Practically, his paper, short though it is, tells us all that is to be told; but the author presumably failed to appreciate, or missed entirely, one or two of those nice little differences that will so often escape the notice of the most painstaking workers, of whom he was one. Possibly "'tis a rule in Nature," in order that those who come after us may have something left to do.

As just stated, Wray's figures and descriptions may be held in all but a few details to be correct. Exception is taken to his interpretation of the proximal radii. These he describes as ". . . thin laminæ, with a thickened upper edge forming a small ledge or kind of flange"; and continues, "If a single barb be examined, the proximal barbules appear . . . at their proximal end to join on to an edge-piece running parallel to the barb . . . this edge is seen to be formed by the attenuated halves of these barbules overlapping one another, and being turned at a considerable angle upon the other half." On referring to his figures, however, we find these barbules represented as perfectly flat laminæ, vertically inserted by their base along the ramus, and with a slight "flange" running along the upper edge, but with no trace of the "attenuated halves overlapping one another." True, this is a diagram, but surely it would have been more correct to figure the specimen, since the essence indeed of a diagram is to give us *the* points of a structure, divested of all superfluous detail. So far as my experience goes, this edge-piece occurs but rarely, neither do I believe it is formed as Wray imagined it, since, in the only case in which I ever found it, the radii were normally related, "the edge-piece," though closely approximated to, being yet quite distinct from them. While this was in the Press I detected a precisely similar flap under the *distal* radii. Again, as touching the notches, these are accounted for by supposing that they are for the purpose of allowing the distal portion of the radius to be turned upon itself without "crumpling." Would it not seem more probable that these notches are—if we are to explain their presence at all—rather to keep the edges of these radii sufficiently far apart to allow not only of the passage of the hooklets, but to prevent their

[3] The italics are mine.

being injured by chafing when in position. The curvature does not seem sufficiently great to account for these notches in the first place, and in the second, seeing that the curve is not delayed till the radius has completed its development, and is then suddenly bent, but that the curvature is synchronous with growth, a far greater inflection could have taken place without any special arrangements, as witness a thousand instances in Nature. Lastly, I would point out that the proximal radii, instead of curving abruptly just at the point where the laminæ of the distal radii break up into hooklets, as shown in Wray's figure, are actually not only curved less, but, as already pointed out, extend right across to the next ramus. I have purposely dwelt upon the shortcomings of these figures, because they are rapidly making their appearance into the various text-books. Though this is a fact to be deplored, it may be readily understood. I suppose, nowadays, if one finds himself engaged in bookmaking—of the more honourable kind, I mean—he must add yet more drudgery to his labours, by verifying every statement to its minutest details, when probably, by the time he has finished the last chapter, he will have to begin again and bring his work "up-to-date."

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W. P. PYCRAFT.

V.

The Lucernarians as Degenerate Scyphomedusæ. A Note upon the Phylogeny of the Order.

PRELIMINARY.

BEFORE entering upon discussion of the problem of the descent of the Lucernariæ, it may be well to set out for the guidance of the general reader the more important facts already known concerning the class to which they belong.

The Hydrozoa, in which together with the Zoophytes, Jelly Fishes, and Siphonophores they find place, is conveniently grouped in two divisions (2). The first includes the Lucernarians themselves, together with those fleshy Medusæ or jelly fishes without velum (hence sometimes called "acraspedote medusæ") and with eight marginal sensory organs (tentaculocysts) so well-known under the forms of *Aurelia*, *Pelagia* and *Rhizostoma* (termed collectively Discomedusæ), and finally the Conomedusæ, a small group with four tentaculocysts and with a velum—*Charybdæa* being the type. Such sub-class comprising these orders constitutes the Scyphomedusæ, so called on account of the general presence of a particular stage in the life-history of typical species. Normally this is as follows :—A ciliated embryo (proceeding from the sexual or medusa stage) after a short period of free-swimming pelagic existence, settles down and becomes attached to some stationary object by one end. A mouth forms at the other, around which sixteen finger-like tentacles appear ; and at the same time the lower part of the body becomes drawn out into a stalk.

This elegant form, $\frac{1}{16}$ to $\frac{1}{8}$ inch in height, is the *scyphistoma* stage, and is what is sometimes termed the hydriform phase, from its general resemblance to the fresh-water polyp, *Hydra*. Soon, as growth goes on, food being captured by the transient tentacles, transverse constrictions—*strobilation*—cut the body into a rouleau of discs, appearing thus as a tiny pile of sculptured plates. Each disc has eight arms, bearing each a tiny tentaculocyst at the tip. This is the *strobila* condition, and the discs composing it, as the constrictions become deeper, are, one by one, broken off and sent out on a free-moving life. Known at this period as *ephyræ*, these organisms rapidly take on the form of ordinary jelly-fishes, develop sexual organs, and send out swarms of ciliated embryos to go through the same strange cycle of life.

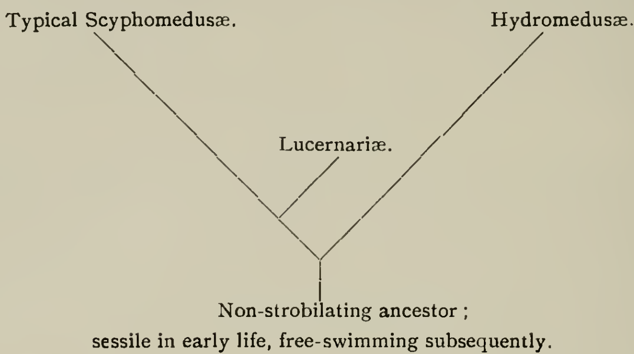
The second great division of the Hydrozoa, viz., the Hydromedusæ, is, by most authorities, made to include both those fixed colonial-living polyp forms, known generally as the Hydroid Zoophytes, as well as the highly-specialised and complex floating siphonophores exemplified by *Physalia*, the Portuguese man-o'-war. The former—the Hydroid Zoophytes—which alone we need mention here, have a life-cycle closely akin to that of the Scyphomedusæ, the main difference being that the fixed stage—the hydriform—does not give origin to medusæ by transverse constriction, but, by oft-repeated budding, produces usually large tree-like colonies. A medusiform sexual stage is, however, present, though its importance is overshadowed and masked by the great development of the hydriform. Thus such a typical Zoophyte as *Obelia gelatinosa* forms, by repeated budding from a ciliated embryo which has become fixed, a large colonial arborescent community of many hydra-like and purely nutritive polyps. Later on, certain modified organs produce, *by budding*, numerous tiny medusæ, which break loose, and float away as beautiful glassy, pulsating bells. These differ from the medusæ of the Scyphomedusæ in having a muscular membrane—*velum*—closing, all but for a central opening, the mouth of the bell, hence the term "craspedote medusæ." This stage is sexual, and gives rise to free ciliated embryos.

The Scyphomedusæ have internal or gastral filaments, and their hydriform stage is simple; the Hydromedusæ have no gastral filaments, and their hydriform stage is colonial and frequently complex. The Lucernarians possess the two former characters, and for these and some less easily stated reasons, their intimate relationship with typical Scyphomedusæ is universally admitted. They, however, differ from both divisions in that their life-cycle is not complicated. The adult hydriform animal gives rise directly to ciliated embryos, which settle down and become of the adult form without metamorphosis.

Several species of Lucernarians are found in British seas. The most common in the South (English Channel) is *Haliclystus octoradiatus*, a most elegant, short-stalked, bell-shaped species found almost invariably attached by an adhesive disc to blades of the sea-grass (*Zostera*) close to low-water mark. The edge of the bell is drawn out into eight points, each bearing a cluster of short clubbed tentacles, well provided with stinging cells. Midway between each two groups of tentacles is a hollow papilla, the "colleto-cystophore," usually believed to be the equivalent or homologue of the tentaculocysts of the medusæ. Normally, these are eight. A like number of genital bands extend from the mouth into each of the marginal points. In general outline *Haliclystus* resembles a shortly pedicelled inverted cone with scolloped margin worked into eight projecting tentacle-armed points. The size frequently reaches 1 in. in height, by 1¼ in. across.

NEW CONCLUSIONS.

The position assigned to the Lucernariæ (=Calycozoa) has long been that they represent, comparatively but slightly modified, a simply organised ancestral form of those Scyphomedusæ called by Haeckel and Lankester the Discomedusæ. This view was embodied by L. Agassiz in the comparison of the relation borne by the order to the Discomedusæ, to that which the stalked Crinoids (primitive form) bear to the free-moving Comatulidæ. This meaning is expressed in the following " tree," which gives the accepted view :—



Such pedigree has the excellent advantage of simplicity—a lowly-organised animal being placed as the common ancestor both of complex and of, presumably, little changed, simply organised descendants.

Quite recently some slight observations (4), which I recorded in the July number of NATURAL SCIENCE (p. 33), decided me to examine afresh into the truth of this conclusion, with the result that new evidence points distinctly to a completely different phylogenetic arrangement, whereby the Lucernariæ would appear to owe origin to an ancestor having as complex a life-cycle as, say, such a typical Discomedusid as *Aurelia* has at the present day, and *not* to a simple non-strobilating scyphistomatous type of animal.

This origin, I believe, came about by the premature development of genital products during the scyphistoma stage of the ancestral form. When such hastening of events took place—ova and sperm being formed and set free prior to strobilation—the latter phase would in consequence be rendered abortive, no ephyræ would be thrown off, and no medusid stage would occur. Such a change in the life-cycle would clearly be advantageous, as under sessile conditions of life competition with free-swimming medusæ would be avoided, and thus the variation resulting from premature sexual maturity would tend to be repeated, and finally to be perpetuated. This same avoidance of competition with the Discomedusæ accounts for the non-operation of that most general law of Natural Selection —that when through advantageous variation a new species is evolved, the old or stock species is forced from the field and becomes extinct, since it cannot compete for long *in the same sphere* with the better equipped varietal form.

The clue that led me to this conclusion as to the phylogeny of the Lucernariæ was the fact of finding great and remarkable variation or inconstancy in *Haliclystus octoradiatus* in those bodies termed colleto-cystophores, and which are generally supposed to be homologous with the marginal organs—tentaculocysts, etc.—of the medusæ. Such variation consists, as I have recorded elsewhere, (*a*) in duplication of the organ, (*b*) in the frequent suppression of one or more in the same animal, (*c*) in the still more frequent reversion in structure to that of the form of the normal papillæ or tentacles as seen in the marginal clusters of these appendages. This gives us the fixed fact of *great variability in the marginal bodies.* Now inconstant presence and frequent reversion are known to be the accompaniments of a vestigial organ; hence we conclude that these bodies are vestigial, consequently have been more important than they now are, and that the origin primitively was from marginal tentacles. As bearing upon this, it is important to remember that it is admitted generally that tentaculocysts arise from modification of ordinary tentacles.

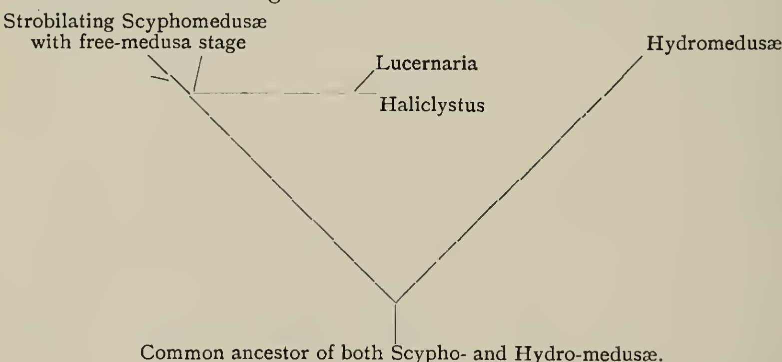
These facts bring us now to the conclusion that *Haliclystus* is descended from an animal possessing a stage wherein certain marginal tentacles had become modified into highly important sensory organs, approaching, if indeed not almost similar to, the tentaculocysts of medusæ. Further, organs of this complex description can scarcely be imagined as in any way serviceable to a sedentary animal unprovided with powers of locomotion, whatever use such may subserve in a free-swimming organism, whether we follow the old theories adducing auditory and visual functions to these sense-organs, or follow Dr. Hurst's more probable theory as put forward recently (3). Sessile animals of the description of the Lucernariæ can neither follow prey nor avoid capture, and hence do not require auditory and visual organs, much less automatically-actuated direction-controlling bodies.

This brings me back to my initial proposition, that among the ancestors of *Haliclystus* was a form having a strobilation stage throwing off ephyræ, which became acraspedote medusæ provided with eight marginal sensory organs and with numerous tentacles separating these, approaching indeed such a form as the present-day *Aurelia.* Then, when the premature development of sexual glands occurred in the scyphistoma stage, as already mentioned, it is reasonable to infer that the other organs belonging normally to the fully-developed adult form (acraspedote medusa) would also appear. Chief among these would be the marginal sensory bodies; but these would occupy a very different platform to the sexual glands. The latter are necessary to the existence of the animal; the former—under the changed conditions of *sexual maturity in sessile position*—would be of no more use, as pointed out above. The courses open to them must either be, (*a*) reversion to the original form, *i.e.*, that of marginal tentacles, (*b*) adoption of a new function, or, (*c*) disappearance. As we have seen, the first and third of these alternatives take place frequently in *Haliclystus*, which, perhaps, more than any other Lucernarian remains most closely related to the old form.

In some species, however, *e.g.*, *Lucernaria campanulata*, the third alternative has been unmistakably adopted, accompanied, too, by the very significant fact that, though absent in the adult, yet the marginal bodies do exist for a short period in an early stage of life. (It may be that the genus comprising this species is directly descended from *Haliclystus*.)

As to the second alternative—the adoption of a new function—probably in *Haliclystus* there is indeed a secondary one coming, or come, into operation. In this genus, the marginal bodies are used, apparently, as accessory to the ordinary tentacles in holding on to any prey in course of capture. Thus, if with a pencil point one of the marginal bodies be touched, it adheres strongly to the pencil, and with very perceptible power. Coincident with the loss of sensory organs, there has been decay of the whole nervous system, the purpose for which departed with the cessation of rhythmic motion and the lost utility of the tentaculocysts.

The view I have above put forward as to the relationship and descent of the Lucernariæ is expressed in "tree" form as follows, and shows clearly on comparison with the diagram given at the commencement wherein the divergence from the old view consists :—



Dr. Hurst has arrived independently and simultaneously at conclusions which, I believe, are practically the same as above put forward, but as I know but the outlines of his reasoning, the reader is referred to his article on the subject following this (pp. 209–217).

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JAMES HORNELL.

VI.

Biological Theories.

VI.—THE PHYLOGENY OF LUCERNARIANS.

THE following speculation is now published, not only on account of the intrinsic interest of the question suggested by the title, but also as an illustration of the application of various views, already published in this series of essays, to the solution of one definite problem; and I have deliberately chosen a case in which the conclusion arrived at is the direct opposite (or converse) of the view to be found in most modern zoological works which treat of the subject. I have made this selection in order that anyone who is prepared to give my views a fair test may do so by comparison of my result with that reached by the usual methods and on the usual assumptions.

As it is necessary, in the course of the argument, to make use of at least six views not as yet familiar to zoologists in general, I will begin by enumerating those views, giving references to the papers in the earlier volumes of NATURAL SCIENCE where fuller accounts of them may be found.

1. Heredity is a mere similarity or likeness among corresponding individuals belonging to several generations of one species. It is not a force or other objective influence, simple or complex, capable of producing effects. The likeness between father and son is not the effect of heredity; it is heredity itself (vol. i., p. 502).

2. This similarity among individuals, or if we prefer to call it so, this constancy of structure in successive generations, is the result of Natural Selection. It is not simply an effect produced once for all, but is dependent upon the continued action of Natural (or other) Selection for its maintenance. Natural Selection under constant environmental conditions tends steadily towards the production of a constant structure, which, when it has become almost constant, we speak of as the specific structure, and the constancy of which may be called "heredity," though the abuse of this term makes it safer to use only the term "constancy" (vol. i., p. 578).

3. Natural Selection is therefore, in a certain sense, independent

of "heredity," and is older than "heredity," and has led to the evolution of "heredity" from "variability," or to adopt the safer terms, the evolution of constancy from inconstancy. Inconstancy is the older: constancy which is never complete is only inconstancy kept within narrow limits by the continued action of Natural Selection. The assumption that "heredity" occurred *ab initio*, and that "variation" has been produced by various external agents—Lamarckian and others—is a mistake (vol. i., p. 580).

4. The ontogeny of an individual, though in some cases it presents a marked resemblance to the real or supposed phylogeny of the species, with reference to certain characters, usually shows only a very slight such resemblance, and may show none at all. We are, therefore, not justified in relying upon it as a "record" of the phylogeny, even though we admit that the supposed record is highly imperfect. Von Baer's law is readily verified in some cases at least; and while it appears to contain all that is true in the "Recapitulation" Theory, it is contradictory to the rest of that theory (vol. ii., pp. 197 and 365, and especially 368).

5. The resemblance between an adult animal on one hand and the larva of another species on the other hand, while indicating a close relationship between the two species, does not show that the second is descended from the first. A stalked crinoid may resemble the immature form of a free-swimming one, and the latter may be actually descended from a form very like the former, but the resemblance in question does not *alone* justify this conclusion, inasmuch as the same resemblance might have arisen from a relationship the direct reverse of that supposed (*Locc. citt.*) The following essay treats of a case in which this reverse relationship appears to be very near the true one.

6. Tentaculocysts cannot subserve an auditory function; and such a function would be useless to a medusa if they could (vol. ii., p. 353, last paragraph); but they may and probably do serve a function of the utmost importance in the preservation of jelly fishes from destruction by oceanic waves (vol. ii., p. 421).

While writing the essay last referred to, the following question occurred to me with almost maddening persistency:—*Haliclystus has tentaculocysts or something of the kind and does not swim: can it then be that the function suggested is the true one, seeing that such organs are present also in an animal, to which such a function would be useless?*

It was not till I had answered that question in my own mind that I ventured to publish the above-mentioned view on the "true function of tentaculocysts." The reason for withholding my answer till now will appear later.

The structure of the marginal bodies of *Haliclystus* (*Lucernaria*) *octoradiatus* shows that they are unfitted for the performance of the function which I have ascribed to tentaculocysts. In position, however, these bodies correspond exactly with the rudiments[1] of the tentaculocysts in a scyphistoma.

The function of the tentaculocyst-rudiment, supposing it to have only one function of importance, is to develop into a tentaculocyst. The development in such forms as I have examined (and have failed to identify!) is direct, and exhibits nothing which justifies the assumption that they have any specially "larval" function, or function not connected with the production of the adult organ: and though we may be unable to say at what precise stage they come to be capable of performing the "tentaculocyst-function," I think we are justified in believing that the efficient performance of that function after the organ is fully developed may reasonably be regarded as the chief factor which has determined the evolution of these organs under the guidance of Natural Selection.

Considering the resemblance in form, and, to a certain extent, in structure, and the identity in position of these rudiments on one hand, and of the marginal bodies of *Haliclystus* on the other, a community of origin is hardly doubtful; in other words, the influences (or some of them) which have determined, or helped to determine, the presence and the structure of either may reasonably be supposed to have played a part in the case of the other also.

According to the usual view, the Lucernarians are the unmodified, or little-modified, descendants of the common ancestor of Scyphomedusæ in general. This view further suggests that *Lucernaria* is the "most primitive," and that *Haliclystus* is "higher" or more modified; and that all free-swimming medusæ (Scyphomedusæ only, I mean, of course) are descendants of forms which, like *Haliclystus*, exhibited no "alternation of generations"—no strobilation, while remoter ancestors still were very similar indeed to *Lucernaria*. Such a view is an almost necessary consequence of the Recapitulation Theory.

For convenience, I will now use the names *Lucernaria* and *Haliclystus* a little loosely, so as to include all hypothetical ancestors possessing the distinctive characters of these genera.

The orthodox view just set forth brings us face to face with a problem which presents, I believe, insurmountable difficulties, *i.e.*, how to account for the evolution in *Lucernaria* of marginal bodies so as to give rise to *Haliclystus*.

We must, in attacking the problem, assume either—(1) that the

[1] I use the term "rudiment" in its true sense, and as equivalent to the German "Anlage." Use may justify a change in the meaning of a word, but the persistent misuse of the terms "rudiment" and "rudimentary," when "vestige" and "vestigial" are meant does not appear to me to justify the change in this case, especially as we have, so far as I know, no other words to put in their place; for the German word "Anlage" is so utterly "un-English" that its adoption in English (or even American!) writings appears to be open to serious objection. I deem it best, therefore, to use "rudiment" for what it really means, and to ignore the prevalent misuse of the term, in the hope that it will soon die out.

bodies in question are useful, or (2) that the evolution has been independent of Natural Selection. Few will be willing to accept the second hypothesis—I least of all, for I believe more thoroughly in the adequacy of Natural Selection than even Mr. Wallace, to whom, with Darwin, we owe the theory itself.

If, however, we adopt the first hypothesis (provisionally), we must further suppose that a portion of the genus *Lucernaria* has been isolated—geographically or otherwise—and so protected from the action of the selection; otherwise the evolution of the new genus (*Haliclystus*) from the old one (*Lucernaria*), under the direction of Natural Selection, would, of course, involve the extinction of the old one.

Lucernaria and *Haliclystus* are, however, found in the same seas, though their geographical ranges may differ considerably. As to the geographical distribution of any one species of *Haliclystus* or of *Lucernaria*, I will not venture to speak, for I am not sure that we can yet say with certainty what are, and what are not, distinct species (or even genera) in this family. For present purposes it is sufficient that forms which have been called *Haliclystus*, and others which have been called *Lucernaria*, are found on both east and west coasts of the North Atlantic, as well as elsewhere, and that some zoologists consider that some of the American specimens in both genera are specifically identical with some of the European ones; and since the mode of life is the same in both, and both live on *Zostera*, and on red sea-weeds, just below low-water mark, there arises a very serious difficulty in the way of the assumption of isolation, either geographically or otherwise. Though the marginal bodies are, perhaps, not *very* complex bodies, yet, in comparison with the whole body of the animal, they are sufficiently complex to justify the assumption that Natural Selection could only determine their evolution during a very long and very severe struggle for existence and for multiplication.

The organs may give some slight advantage, or even a great advantage, to *Haliclystus*. They may serve, for instance, as "anchors" (Clark) (1); but that is not enough. If Natural Selection has caused the evolution of animals possessing those organs (*Haliclystus*) from animals not possessing them (*Lucernaria*), that means that the organs have at every stage in their evolution conferred such an advantage on their possessors as to enable them to vanquish, to starve out, to render extinct, the forms not possessing them, or possessing them only in a less perfect state of development. It means that the evolution of *Haliclystus* from *Lucernaria* would have exterminated the genus *Lucernaria* in the seas where this evolution occurred; and this the geographical distribution of the two genera emphatically contradicts.

We may therefore regard—nay, we are compelled to regard, the evolution of *Haliclystus* from *Lucernaria* as disproved.

From what, then, is *Haliclystus* descended?

I believe the clue to the solution of this problem is to be found in a consideration of the evolution of tentacles, and of tentaculocysts, and of the marginal bodies of *Haliclystus*.

The evolution of the marginal bodies in *Lucernaria* offers the difficulties already considered, and, therefore, we may not take these bodies as the starting-point in speculating as to the origin of tentaculocysts. Before this procedure would be allowable, we must account for the origin of the marginal bodies themselves. As this origin is not obvious, we may consider the other organs, tentacles and tentaculocysts, first; and we will begin with tentacles, as these are the simpler. In doing so, however, it is necessary to bear constantly in mind that the more complex is not necessarily a modification of the simpler—the simpler may just as well be a modification of the more complex, or the two kinds of organs may even have been independently evolved.

The tentacle in its simplest form is a mere outgrowth of the body-wall, identical in histological structure with the adjacent portions of that body-wall. Almost in this simple form tentacles may be seen in *Hydra*, though *Hydra* is probably far from being a primitive form. A mere projection of the wall of the animal, provided it be contractile and suitably placed with reference to the "mouth," may well be regarded as useful in the prehension of food. Its usefulness would, moreover, be greater, within certain limits, the greater the length of the process. Every individual possessing such outgrowths would, therefore, be favoured thereby in the struggle for existence and multiplication; and this advantage would, moreover, always rest more especially with the individuals in which the processes were, within limits, the longest. Hence the evolution of tentacles from mere irregularities of the body-wall can be explained without any assumption beyond that of the operation of Natural Selection.

How a free-swimming medusa was evolved we are not now considering, and fortunately it is not necessary that we should answer the question at present. Suffice it to say that free-swimming medusæ exist, and that the evolution of their tentacles in the above-described way offers no difficulty.

With tentaculocysts the case is different. If we suppose them to be modified tentacles the difficulties vanish; otherwise the difficulties are, so far as I can see, insurmountable. The slow transformation of a *few* tentacles *out of many* so as to better perform a special function, performed, at best, only badly by a tentacle, leaving the remainder to carry on the original chief function of these organs, appears possible; for the chief changes required are the formation of a depression around the base of the tentacle and subsequently a shortening of the tentacle itself, and these offer no difficulty. Every stage of such an evolution provided only that the medusæ are fairly large, and becoming larger, fulfils all the requirements of the theory of Natural Selection; that is, the transformation is a gradual one, and every stage in it is an advance upon the previous one; or in other words, every supposed change in the evolution is a minute one; is one which may well be supposed to occur "fortuitously"; and is one which confers an advantage upon the species undergoing the transformation—however numerous these species may be.

It is not necessary to go more into detail in this question, as the view here urged is, I believe, adopted almost unanimously: difference of opinion, if there be any, will probably be confined to the question of the possibility, or perhaps probability, of such a transformation occurring independently in several species.

There are, however, not a few who have only adopted this view as to the origin of tentaculocysts at a moment when they were not thinking of their homologues in a supposed *Haliclystus*-like ancestor. These, no doubt, will require that any hypothesis or conclusion as to the course of evolution of tentaculocysts shall regard the marginal bodies of *Haliclystus* (or something very like them) as having served as a stage in the evolution. To such I commend the following considerations.

(1) The evolution of marginal bodies in a Lucernarian *ab initio* offers difficulties already considered, and apparently insurmountable.

(2) The evolution of these bodies *directly from tentacles* in ancestors of Lucernarians offers difficulties no less formidable. Natural Selection will not on this assumption account for the evolution of two genera *Lucernaria* and *Haliclystus* in the same seas, and in like positions in those seas. If the marginal bodies are advantageous, then their evolution from tentacles would be accounted for easily enough, provided we had *Haliclystus* alone to deal with. The evolution of the two side by side negatives any such hypothesis.

We may therefore turn in another direction for the solution of our problem—there is only one left! The marginal bodies may be *vestiges* of the tentaculocyst-rudiments of ancestral scyphistomata.

This involves, of course, the supposition that *Haliclystus* is descended, not merely from scyphistomata, but from free-swimming medusæ.

The evolution of tentaculocysts in free-swimming medusæ I have explained. In that explanation is involved an explanation of the evolution of their rudiments in the young. The evolution of strobilating from non-strobilating medusan types appears to me to offer no serious difficulty, and I leave this portion of my argument (which seems almost obvious) to be filled in by my readers. If it should be called for I will publish it.

Given, then, that there has been evolved a type of medusa in which the formation of a scyphistoma, strobilation, and the separation of *Ephyræ* provided with rudimentary tentaculocysts have been phases in the life-history, the origin of Lucernarians from such a type offers no further difficulty of great magnitude. (*Re* the meaning of "rudimentary," *see* footnote, p. 211.)

On this hypothesis the community of origin or "homogeny" (as Professor Lankester has well called it) of the marginal bodies and the tentaculocysts is not a mere community of origin; the marginal bodies are vestiges of the rudiments of tentaculocysts, *i.e.*, they are vestiges of tentaculocysts themselves; but what is their function? Are they *mere* vestiges, or are they organs which have undergone a change of function?

Again, the coexistence of two genera in the same localities and with the same mode of life shows the way to the answer. They are mere vestiges. The ancestral function is impossible: they cannot serve as rudiments whose function is to develop into organs (tentaculocysts) useful in a free-swimming phase, because there is no such phase in the life-history. They cannot serve a function of sufficient importance to determine their preservation through Natural Selection, for *Lucernaria* thrives abundantly without them living in the same way and in the same places. We are therefore driven to the conclusion that these bodies, if not functionless, are at least of so little value to their possessors that the chief competitor in the struggle for existence (*Lucernaria*) survives although devoid of them. If this be true, then (*see* view 2 at commencement of this essay) they *must* be inconstant ("variable") in structure, or at least must tend to become so.

Now this point seemed to be so easy to put to the test of observation that I delayed to publish this essay till I should have an opportunity of applying the test.

I had not yet had the opportunity of so testing my conclusion when the July number of NATURAL SCIENCE appeared, and in it a record of observations made by my friend, Mr. Hornell, on this very point (vol. iii., p. 33). No communication on the subject had passed between us. His observations were made in ignorance of my conclusions, and hence the element of bias is happily absent.

I should have been sanguine indeed to expect so striking a confirmation of my conclusion! Out of one hundred and eighteen specimens of *Haliclystus* taken in June last in Jersey, seventy-eight (that is, two-thirds of the whole number) exhibited a very marked abnormality in the structure of these organs, *i.e.*, some of the organs were "crowned with tentacles." Of specimens gathered earlier in the year (that is, older specimens which had lived through the winter) a small proportion possessed less than the normal number of these bodies. None were observed to be "crowned with tentacles." Either the tentacle-crowns had been absorbed, or the individuals possessing them had been worsted in the struggle for existence during the past winter.

Further observation is necessary before we can say which is the true explanation. The fact that many—about 15 per cent. in a lot I received from Messrs. Sinel and Hornell in the spring of the present year—have survived through the winter with less than the normal number of these bodies, shows at least that the possession of the full number of these bodies does not give a very great advantage, if any, to their possessors.

Some years ago I obtained from Mr. Bolton a large number—probably nearly two hundred—of specimens of so-called *Lucernaria*, which, he informed me, had been collected on the coast of Cornwall, and they were forwarded alive on some red algæ. They were all small—it was autumn—and I then noticed that they were of two kinds—*Haliclystus* and *Lucernaria*—mostly *Haliclystus*. I had not considered this problem then, and simply took them to be two genera mixed, and thought no more about it. I did not examine them closely, and hence cannot say whether intermediate forms with less than the full number of marginal bodies were present or not. The naturalists at Plymouth may well take that problem in hand and decide for us whether *Lucernaria* and *Haliclystus* are generically—to say nothing of specifically—distinct or not.

Whatever answer may be given, Mr. Hornell's description of *Haliclystus* must be enough to convince any non-evolutionist—if such a zoologist still exists—of the mutability of species (or genera). It is a simple description of a species in the very middle of its transformation—*Haliclystus* is in the very process of being transformed into *Lucernaria*, there, on the coast of Jersey!

On receiving my friend's paper I at once wrote to him, giving him an outline of the above argument, and pointing out the remarkable way in which his observations fitted in with my conclusions, and was not a little surprised to receive, by return of post, a long letter, in which he showed how he had, by an altogether different line of reasoning, come to the same conclusion as my own, viz., that *Haliclystus* is a "degenerate" scyphomedusan—an "arrested scyphistoma"—and that *Lucernaria* has gone further in its "degeneracy."

If, however, such a "degeneration" or "arrest" may occur once, it seems probable that it may occur often. There may be a well-established genus, *Lucernaria*, to which new species are now being added, not by the modification of the old one or ones, but by new "degenerations" of scyphomedusæ, or new "arrestments" of scyphistomata of various species of medusæ; and hence existing well-established species of *Lucernaria* (supposing there *are* such) may have arisen, not from a common ancestral form of *Lucernaria* at all, but by a series of separate "degenerations" of scyphomedusans through a *Haliclystus* stage. Or, on the other hand, it may be that *Lucernaria* and *Haliclystus* are only names for different forms of "monstrous" scyphomedusa, arrested in the scyphistoma stage—that is, arrested so far as strobilation is concerned, but developing the reproductive organs.

I would submit, therefore, the following conclusions for criticism:—

(1.) That *Haliclystus* is a degenerate or arrested scyphistoma (*cf.* axolotl!);

(2.) That its marginal bodies are *arrested rudiments* (*i.e.*, vestiges) of tentaculocysts;

(3.) That *Lucernaria* is a still further modified (" degenerate ") descendant of some free-swimming medusæ of one or more species ;

(4.) That the degeneration and disappearance of the tentaculocysts, when these ceased to be of use for the function I ascribed to them, supports the view I have expressed in a former essay on the subject of their function.

REFERENCE.

1. **Clark, H. J.**—Lucernariæ and their Allies. Smithsonian Contributions to Knowledge, vol. xxiii., 1881.

C. Herbert Hurst.

VII.

Notes on Pipernoid Structure of Igneous Rocks.

MOST geologists are acquainted with a peculiar rock, much employed as a building stone in Naples, that occurs near that town at a village called Pinaura, and known by the name of "Piperno." What the origin of this name may be, I do not pretend to know, but it much resembles the word *peperino*, a true though peculiar tuff, derived from a sub-lacustrine eruption of the Colli Albani, near Rome, but entirely different in character.

For all practical purposes, this rock may be called a rather basic trachyte, but not uniform in structure. It is of a clear grey colour, interrupted by innumerable flackers,[1] when seen in section lying parallel to the surface on which the rock bed rests. These, when isolated, are seen to be flattened cakes of an original pasty material, rather longer in one definite axis with striæ parallel to the same, and of such a nature as to show that shearing occurred in the viscous material between the upper and lower surface of each cake. Chemically and microscopically, these two constituents are identical, except that the black flackers, or cakes, are more compact, exhibit true fluxion structure in harmony with their planes of shearing, and are much more vitreous; in fact, they present all the characters of a lava. The grey material is very spongy, but does not present true vesicular structure, extending beyond the minute particles of which it is composed, and, in fact, exhibits all the characters of a tuff—that is, a consolidated volcanic dust.

Piperno occurs in one or more beds in the neighbourhood of Pianura, Soccava, and Monte Spina. Of late years I have paid much attention to this rock, and I have been successful in working out the stratigraphy and physical history in detail of the Phlegrean Fields and the Campanian plain. The principal conclusions that I have come to have been published in the Reports Brit. Assoc. since 1884. In these researches I was able to show that we could trace the piperno into the grey tuff which covers several hundreds of square miles of the

[1] This word is used by cabinet-makers to denote the markings due to the cutting the medullary rays in an oblique manner, which resemble the graining of piperno.

Campanian plain, and even remnants of which are found filling depressions in the limestone as far as Bari. This tuff much resembles the piperno, except that the grey interstitial matter gets finer and lighter the further we extend outwards radially from the true piperno. Coincident with this, we find that the blacker cakes become more and more spongy, not at first rapidly decreasing in size, but decreasing in bulk specific gravity. After a certain distance, we find they also decrease in size, until at distances of 20 to 30 kilometres they are almost invisible. The flattening out of the cakes, parallel to bedding, soon disappears as the black inclusions get lighter and more scoriaceous.

All geologists who have attempted to explain these *principal* peculiar characters, have utterly failed to do so, and had I space to enumerate many minor ones, the difficulty would be still greater. Unfortunately, most of these conclusions have been jumped at, as the result of that useful instrument though unfortunate misleader of geology, the microscope, which has caused investigators to forget that it is only one means to an end, and that field investigation is of far greater importance.

Some have supposed it to be a metamorphosed tuff, but I have shown that subjacent beds of pumice are as fresh almost as the day they fell; others, have imagined it metamorphosed by infiltration or some internal reaction; others, again, maintain it to be a lava. It was only after years of careful investigation that the true explanation suggested itself to me.

In my researches on the Geology of Monte Somma and Vesuvius,[2] I was able to bring forward conclusive evidence that the volcanic paste that occupied the duct of a volcano was nearly, if not entirely, vapour-free, and that it dissolved H_2O out of the upper stratum of aquiferous rocks of the earth crust. I showed that in any explosive eruption, the uppermost part of the paste in the canal was the most gaseous (H_2O), and that, as the eruption progressed, the pumice ejected became more compact, and cooled slower by a less conversion of its potential into kinetic heat for the evolution of less gaseous contents.

The Geological Society published my facts, refusing to do so with my conclusions, but the Royal Dublin Society was more charitable and put them before the public.[3] Those facts and conclusions have never been assailed, but have carefully been passed over by subsequent writers on volcanoes, although they are, I maintain, the only scientific and generally applicable explanation of volcanic action offered to the public—not a romantic hypothesis, but one borne out in every region I have yet examined.

Now, what appears to have occurred with the *piperno* eruption is

[2] *Q. J. Geol. Soc.*, 1884, vol. xl., pp. 35–112.

[3] *Q. J. Geol. Soc.*, 1885, vol. xli., pp. 103–106; and *Scientif. Proc. Roy. Dublin Soc.*, vol. v., n.s., pp. 112–156.

as follows: A trachytic paste occupied a volcanic duct towards the surface. During a considerable period of repose the upper portion absorbed much water, its temperature fell, and its tension rose in proportion; lower down, less change of this nature had taken place. When the final outburst occurred, the vent must have been of tremendous size, as proved by the enormous quantity of material ejected. In consequence, the lower part rose almost coincident with the upper, as shown by the arrangement of the deposit. The upper cooler, more aquiferous part, was soon reduced to a solid dust by vesiculation, and fell as such, while the hotter, lesser aquiferous, was only separated into cakes. The heaviest and most compact of these would fall amid the dust as hot masses, close to the vent where they would spread out by their own weight and by the pressure of other material that immediately buried them. They were squeezed out by a movement hardly amounting to flow, but just sufficient to elongate them where they rested on an inclined surface. The greater the inclination, the more are they pulled out, so that in the buttress under Camaldoli, which shows the bed fairly inclined, the flackers are reduced to that of the thinnest paper.

The top and bottom of each piperno bed are less compact, and have less of the enclosed flackers of lava, for very obvious reasons, too lengthy to detail here. Even in a single bed of piperno, the variation in size and numbers of the flackers varies greatly with the horizon, but such variations are continuous over considerable distances. These differences of structure really record the fluctuations in the force of the eruption and the temporary predominance of one or other of the two kinds of ejecta.

Near the vent little dust fell, because it reached great altitudes and was carried away by the wind. The heavier lava cakes, however, fell by preference near the vent, especially the more compact and heavier ones; more and more scoriaceous, and therefore lighter were they, the farther they travelled, and the colder were they when they fell, so that at a short distance they were too hard to flatten out. At still greater distances, however light and scoriaceous they were, the large, then smaller and smaller ones, fell, until finally the deposit is practically composed of the dusty part only.

The *piperno* at Pianura consists of several beds intercepted by brecciated fragments of trachyte of the same composition, which, no doubt, mark an interval during which a crust solidified, to be re-broken up by the fresh outburst that formed the next bed.

I have spoken of one vent, but I do not deny the possible existence of a long fissure that reached the surface at several points. Neither should I dare to attribute certain pipernoid tuffs, south of and near Rome, to the vent, or the main one if several, that opened between Monte Spina and Camaldoli. At Monte Spina the blobs of lava were so large and so slow in cooling that they actually flowed as a stream.

In Ischia, on the east coast of the island, is a lava in which two magmas seem to have issued and flowed together, almost reproducing that kind of structure of earthenware, of mottled design, made by working together different coloured clay. From that we pass to the banded trachytes of Palmarola and the so-called felsites (because they are old) of the neighbourhood of Dresden, or the liparites of Basiluzzo.

The vitreous crust of some of the flackers, the peculiar conformation around foreign enclosures, the nature of these latter, and many other points might be discussed as confirming this explanation. At any rate, I hope to have cleared up a knotty point in lithology by showing that my theory of volcanic action is able to explain even an intricate rock structure, such as that which I have called "pipernoid," from its remarkably perfect illustration in the *piperno*, and which I have frequently seen exhibited in rocks from Iceland to Sicily.

H. J. Johnston-Lavis.

VIII.

The Evolution of Life.[1]

PROFESSOR BROOKS has printed in advance from his memoir on the genus *Salpa*, chap. vii., on "Salpa in its Relation to the Evolution of Life," and chap. viii., on the "Origin of the Chordata considered in its Relation to Pelagic Influences." Salpa is distinctively a pelagic animal borne hither and thither on the surface of the ocean as currents and the wind direct, living entirely on the minute micro-organisms and, unlike so many of its nearest relations, spending no part of its life attached to rocks, or weeds, or mud. Professor Brooks has been studying its life-history and its structure minutely, and has come to some biological conclusions of very general interest, conclusions which, perhaps unnecessarily, he apologises for as heterodox; but we hope that morphology so far is free from the doctrine of authority. Its generalisations tread so closely on the heels of observed facts that, granted the necessary skill in manipulation, no morphologist need accept much on the authority of a memoir, or a text-book, that he cannot verify with the scalpel and microscope.

Dr. Brooks leads up to his results by consideration of the contrast between terrestrial life and marine life. The surface of the land is more or less completely covered by verdure. Forest trees, and herbs and grasses, ferns and mosses stretch their green expanses into the air, and the chlorophyll of the cells in the presence of sunlight is constantly building up from the inorganic elements of the earth and air rich supplies of protoplasmic food; and so on the surface of the earth the vegetable eater, as insect, or bird, or mammal, abounds not only in species but in individuals, while carnivorous forms are found in smaller numbers, and obviously in immediate dependence on the vegetarian world.

The ocean waste is very different. Here and there are floating tufts of sargassum, but save round the coasts the vegetable world is meagre, and the richly-coloured gardens of the bottom are nearly all animal forms. So, too, in the sea, the conspicuous animals are nearly all carnivorous. The seals live upon fish; the sea elephants and walruses on lamellibranchs; whales, dolphins, and porpoises

[1] Studies from the Biological Laboratory of Johns Hopkins University, vol. v., No. 3. "Salpa in its Relation to the Evolution of Life." By Professor W. K. Brooks, Ph.D., Baltimore, May, 1893.

live upon sea animals. The enormous numbers of terns, gulls, petrels, cormorants, and so forth, are all carnivorous. Among lower forms, practically only a few molluscs, echinoderms, and annelids are plant-eaters, and they play an inconspicuous part in the economy of the ocean.

If a small amount of *débris* coming from the land is left out of count, the source of the food-supply of the ocean is to be found in the minute organisms of the surface. *Trichodesmium*, *Pyrocistis*, *Protococcus*, and the coccospheres, rhabdospheres, and diatoms form the lowest link in the chain. These and the globigerinæ and radiolarians that feed on them, are so abundant and prolific as to supply practically all the food for the animals of the ocean. Such a simple pelagic food-supply Dr. Brooks believes to be, not only the fundamental, but the primæval supply—the supply which has determined the course of evolution of marine life, and secondarily of all life. The conditions of pelagic life are so simple and so universal, that the dawn of life appeared there. There is no fierce competition, and very little stimulus for the production of diversity of habit. All the metazoa have pelagic larvæ, or else embryonic stages recalling such larvæ, and best interpreted as the degenerated vestiges of a pelagic habit. During the long period in the history of the earth when there was practically only pelagic life, the pelagic ancestors of the great groups of the metazoa were evolved.

These advanced little till multiplication in the number of individuals drove some to the bottom or to the shore. In the strain and stress of the new conditions with their more varied mutable environment, increase of size and complexity, appearance of shells and skeletons began. All this happened before the earliest fossiliferous strata, for these show abundance of complicated types, of which their minute primæval pelagic ancestors left no traces in the sedimentary rocks. Secondarily, a number of forms returned from the bottom to the surface just as the Cetacea and many sea-birds returned from the land to the sea, and the improved wanderers rapidly produced changes in their ancestral pelagic home; but none the less a number of surface forms Dr. Brooks believes to retain the simplest possible structure, and to resemble closely the primæval ancestors of their groups.

Among these are notably the ancestors of the Chordata. Dr. Brooks criticises very closely Dohrn's famous degeneration theory of the Ascidians and finds in *Appendicularia* a little modified ancestral pelagic chordate.

One difficulty in the way is the possible importance of metamerism; but Dr. Brooks makes light of this, and many morphologists will be ready to agree with him. Metameric segmentation is a feature which occurs in widely-separated groups: which appears again and again in cases where there can be no possibility of community of descent. It may, in fact, be said that metamerism is so characteristic of life that it is more likely to disguise than to reveal kinships. Dr. Brooks considers it a corollary of growth by cell-reproduction, but to whatever biological cause it may be due, too much importance has been attached to it in the framing of phylogeny.

The ancestor of *Appendicularia* he pictures as a "simple, minute, unsegmented chordate animal, leading a free locomotor pelagic life, and subsisting on the micro-organisms of the ocean. It had an elongated, unsegmented body stiffened by an axial, unpaired, unsegmented notochord like that of *Amphioxus*, *Appendicularia*, and the ascidian larva: a simple elongated dorsal nervous system: and an elongated, ventral digestive tube without pharyngeal clefts." This tube was distended and ciliated, and the water with the micro-organisms in it was swept through by ciliary motion, not by contractions of the body-wall.

In process of time there came slime cells to retain the food, and Natural Selection preserved and accentuated the slime cells in the anterior region. The free current of water passing through the animal at first swept in new organisms, and swept even any partially digested organisms. However it might have appeared, a pharyngeal cleft that would allow this free stream of water to pass out, leaving behind the slime-bound food, would be an advantage too great not to be selected, and one gill cleft being established, Dr. Brooks believes that by a "law of growth" it would become paired.

Thus he finds in the primitive gill clefts mechanical advantages for nutrition quite unconnected with respiratory or excretory functions. The velum, the endostyle, and the peripharyngeal bands similarly were food-catching mechanisms.

He traces out stages by which fixed Ascidians might easily have been derived from a free-living form like *Appendicularia*.

In the next section Dr. Brooks criticises the Annelidan theory very closely; but we imagine that, however fertile they have been in suggestions, and full as they are of brilliant morphological detail, not many naturalists support the main thesis of Dr. Dohrn's well-known studies; and for that reason it is hardly necessary to refer in detail to the present criticism. Still less to the associated criticism of the hypothesis that the Tunicates are degenerate Selachians.

What is more interesting is the view Dr. Brooks takes of the coelome. Since Hertwig first insisted on the importance of this method of development of the body-cavity, it has been accepted as final that the chordate body-cavity is an enterocoele—that is to say, that it is developed from series of pairs of gut-pouches, and that the simplicity of *Appendicularia* cannot be primitive, as its ancestors once possessed a segmented enterocoele. Seeliger gives a minute account of the origin of the mesoderm in *Clavelina*, and states that it comes from two totally unsegmented rows of cells, while the body-cavity is really a primary segmentation cavity. Davidoff, working on *Distaplia*, agrees with him, and Dr. Brooks, from the development of Salpa, coroborates both. Practically, his conclusion is that the Chordate ancestor came straight from a simple pelagic form of which *Appendicularia* is a near living kinsman. The relations between the Chordates and other groups are to be looked for only among the characters common to such simple pelagic forms, and all chimæras, like the Arachnid or Crustacean ancestry of the Chordates, are relegated to the same oblivion as the more useful, if not more satisfactory, Annelidan hypothesis. It must, however, be pointed out that while Dr. Brooks received Willey's new work on Ascidians and *Amphioxus* in time to criticise it sharply, he had apparently not received, and so has not yet tried to consider, the bearings of the newly-discovered nephridial organs of *Amphioxus*.

SOME NEW BOOKS.

Birds in a Village. By W. H. Hudson, C.M.Z.S. 8vo. Pp. 232. London: Chapman and Hall, 1893. Price 7s. 6d.

We congratulate Mr. Hudson upon the publication of this pleasant volume, which is sure to receive a hearty welcome from all who like graceful essays about country life. "Birds in a Village" contains nothing absolutely new to us, nor is it intended in any sense as an ornithological treatise. Its function is to convey to us in a singularly felicitous style the impressions which the author has gathered from his rambles beside English brooks and across our home commons. Therein Mr. Hudson has found an agreeable variation from city life. Accordingly, he introduces us to simple village folk, and interprets for us their thoughts and instincts; or sketches the picturesque details of local scenery, following out his observations with such obvious faithfulness of narrative that we seem to see with his eyes and to hear with his ears.

There is, it must be confessed, a large element of "twaddle" in these pages; but *that* is such a constant ingredient of "popular" books upon Natural History, that it would be absurd to quarrel with it. The general public will perhaps prefer it to the enunciation of scientific truth; if it does not do much good, assuredly it is impotent to do anyone an injury. We know that Mr. Hudson is at heart as devoted to scientific ornithology as could be desired, and that when he romances or tells a pretty fairy tale, it is only because he prefers to be amusing rather than to edify us. Slight as he admits his knowledge of English birds to be, his diffidence cannot conceal the fact that with many of our homely birds he is thoroughly familiar, and his descriptions of their idiosyncrasies are pleasant to glance over. Not that Mr. Hudson discovers new points in their life-history; but because, being perfectly in correspondence with his environment, he plays the part of a faithful interpreter of their emotions and sensations, whether occupied in studying the movements of a jay amid the leafy foliage of some quiet copse, or comparing the song of the hedgerow nightingale with the richer notes of the white-banded mocking bird of his native country. Mr. Hudson is always certain to enlist our sympathy in his patient study of the most commonplace incidents.

We differ from Mr. Hudson in one particular. We refer to his tirade against the supposed enormity of keeping song-birds in captivity. The most that he can say against this custom, which exists all the world over, is that Mr. Rennell Rodd has protested against the practice in the dainty verse which our representative at Zanzibar so well understands how to frame. We have ourselves derived a vast amount of pleasure from our caged pets, and having enjoyed in this way the friendship of such rare and interesting forest birds as the Nutcracker, or the Great Spotted Woodpecker, may here register our dissent from Mr. Hudson's perfervid sentiments.

H. A. Macpherson.

Handbook of Palæontology. [Handbuch der Palæontologie.] By K. A. von Zittel. Vol. iv., pt. ii. Pp. 305–592. Munich and Leipsic: R. Oldenbourg, 1893. Price 10 marks 50 pf.

As briefly mentioned in our last number, where we gave a notice of the preliminary issue of the Introduction, this important work is now fast approaching completion, and we have the pleasure of welcoming the second fasciculus of the volume devoted to Mammals.

This part, which fully maintains the high standard of its predecessors, contains the greater portion of the Ungulates, which are concluded, together with the Rodents, Insectivores, Bats, and the commencement of the Creodont Carnivores. We are glad to observe that among the Ungulates special attention is devoted to the numerous South American forms which have been described of late years; and we may safely say that nowhere else can the student obtain so much information on a very difficult subject in a very short space. In regard to the arrangement of the Ungulates, the author, while following in the main the divisions into sub-orders adopted by most modern writers, makes a new departure in respect of the limits of families. He takes, for instance, the Suidæ to include genera so widely different from the typical forms as *Achænodon* and *Chœropotamus*, while the Anoplotheriidæ embraces the Cænotheres, Dichobunes, and Dichodons, and under the Cervidæ are ranged, not only the Deer, but likewise the Giraffes and Sivatheres. We confess that this arrangement is not to our liking, as it involves the introduction of a number of sub-family names; while the inclusion of the Giraffe and its allies in the same family as the Deer tends to obscure their manifest affinities in some respects with the Bovidæ. In discarding the sections Tragulinæ and Pecora, we are, however, persuaded that, from a palæontological standpoint, the author is correct, since, as we ourselves have previously urged, it is impossible to draw any line of demarcation between the Tertiary Deer and Chevrotains. That the Professor should have seen reason to employ family names in such a wide sense is the more surprising to us, seeing that among the Ungulates he splits up many groups, such as the Elephants, Oxen, and Uintatheres, into so-called genera, which it has been the tendency of modern writers to ignore. We are pleased to notice that the author fully confirms the view that the three-toed Eurytheriums of the Vaucluse Oligocene are not generically separated from the typical Anoplotheres of the Paris Basin; but we should like to be more fully assured that the latter were likewise tridactylate. So far, indeed, as the casts of Cuvier's typical specimens in the British Museum admit of forming a judgment, the Paris specimens certainly appear to have been didactylate; and an important difference in the form of the astragalus in the specimens from the two localities has been noticed by several writers. As a minor criticism, we may mention that we cannot assent to the inclusion of the Hartebeests in the same group as the Gemsbok and Sable antelope, the cheek-teeth of the two being so essentially different; while the writer of this notice may take the opportunity of disclaiming the position of sponsor to the genus *Helicophorus* assigned to him on page 418.

As the treatment of the other orders described in this fasciculus does not call for any special notice, we may conclude with the hope that we shall soon have the opportunity of congratulating the learned author and his able colleagues on the completion of their arduous and monumental task.

R. L.

Chemical and Micro-Mineralogical Researches on the Upper Cretaceous Zones of the South of England. By William Fraser Hume, D.Sc., etc. 8vo. Pp. 104. 3 Tables. London: [Printer—Whitehead, Morris & Co., 9 Fenchurch Street], 1893.

This publication is, we believe (though it is not so stated), the author's thesis for the degree of D.Sc. of London. It is the most interesting contribution that has appeared for some time on the Cretaceous rocks, and bears evidence of a vast amount of laborious and painstaking research. Commencing with the Chalk Marl, the author gives chemical and mineralogical analyses of the washings of samples of 80 grammes average weight of each of the zones of the Chalk, from the Chalk Marl to the *Belemnitella mucronata* zone, and also discusses the micro-zoological remains which occurred in the residues. The samples were mostly taken from the immediate neighbourhood of some distinctive fossil, so as to be typically characteristic of the zone to be examined. Each zone is treated separately and in detail, the chemical analysis coming first, then the organisms, thirdly, the secondary minerals, and, lastly, the detrital minerals.

The author has wisely taken into his confidence specialists in their own departments, and has thus not fallen into the fatal error of describing everything that came into his hands. Thus, Dr. Hinde has looked over the sponge spicules, and Mr. Chapman has examined the Foraminifera. Towards the end of the paper we find a summary of results, and here Dr. Hume discusses the secondary silicification of Foraminifera in a clear and exhaustive manner. He says, "there seems little reason to doubt that considerable replacement of the calcareous constituents by colloid silica has taken place," and he notes that the most delicate interior divisions are in a fine state of preservation, after the residue has been treated with acid. So complete has this replacement been in many cases that, in the process of solution by acid, the bubbles issue from the mouth only of these microscopic tests. Following these mineralogical notes, Dr. Hume mentions a comparative identity between the foraminiferal fauna of the Chalk Marl and the Gault, and commenting on the depths at which many forms live in recent seas, inclines to the view that the Chalk Marl was deposited at a depth of from 350–500 fathoms. No new light has been thrown by Dr. Hume's researches upon the distribution of the sponges in Cretaceous times, nor has he met with remains of Radiolaria or Diatomaceæ in the rock-specimens examined.

In the general summary devoted to mineralogy, the author notes the predominance of quartz in angular fragments, with occasional crystals, worn and clear, in the lower Cenomanian beds. Inclusions in quartz of apatite, rutile, tourmaline, ferruginous oxides, liquids, and gases are recorded, and other forms of silica, as jasper and chalcedony, have been detected. Felspar occurs in Chalk Marl at Folkestone and in certain of the Turonian and Senonian beds of other places, but it is much altered. Hornblende, augite, tourmaline (widely distributed), zircon, rutile, garnet, glauconite, limonite, and pyrite are also recorded in the paper. The glauconite abounds in the Chalk Marl, but rapidly diminishes in the higher Cenomanian. It reappears as an important constituent in the Chalk rock, and continues to form a considerable portion of the residue in all the lower Senonian zones. Preference is given to the theory, in accordance with Cayeux's views, that glauconite may be formed independently of the presence of organisms. According to the author's observations, the greater the thickness of the beds the more calcareous do they become, and he does not accept Cayeux's view of the terrigenous origin of Chalk as applicable to the Chalk as a whole, but considers that "such evidence as is forthcoming from faunal considerations points to an ocean which, if not abysmal, at least possessed depths far exceeding those of many prominent marine areas."

Space prevents any further notice of this interesting contribution to Cretaceous history, the researches for which have been carried out in the laboratory of the Royal College of Science; and while congratulating Professor Judd on the success of his choice of demonstrators, we thank Dr. Hume for providing so much food for reflection to the students of oceanic deposits, micro-zoology, and geology.

Lessons in Elementary Biology. By T. Jeffery Parker, D.Sc., F.R.S. With 88 illustrations. Second Edition. London: Macmillan & Co., 1893. Price 10s. 6d.

Professor Parker's well-known text-book is designed for reading—a complement to laboratory work with a text-book like the "Practical Biology." It aims at teaching the ideas rather than the details of the science of Biology—these ideas being studied in connection with concrete types of animals and plants. It is hardly necessary to do more than welcome the second edition of a volume so familiar and so satisfactory. It has been revised; several new figures have been introduced; and the lessons dealing especially with cell-structure and sexual cells (vi. and xxiv.) have been practically re-written. It is hoped that the few notes now to be made will be taken, not as criticisms directed against the book, but as footnotes which may be of some use.

On page 7 Professor Parker writes that "the theory is that protoplasm has a slightly acid reaction." This is very doubtful; it is practically certain that the enchylema is slightly alkaline—and it is doubtful if there is acid reaction in living protoplasm except in the secretions in food vacuoles.

On page 16 the author distinguishes between "true excreta" and the solid excreta, or, "more correctly, *fæces*, of Amœba—the rejection of which latter is no more a process of excretion than the spitting out of a cherry-stone." But certainly the "fæces" of the higher animals are largely composed—not merely of undigested substances, but of matter actually excreted by the cells lining the digestive tract.

On page 21, where the author is discussing the effects of salt on Amœba, he conveys the impression, although he does not definitely assert, that Amœba is confined to fresh-water—whereas it is quite common along the shore.

In the section on Diatoms no mention is made of Bütschli's account of the cause of their movement. (See Natural Science, vol. i., p. 537.)

Subterranean Picardy [La Picardie Souterraine. Album Composé de dix Planches en Chromolithographie reproduisant des Armes, Outils et autres Utensiles en Silex trouvés dans les Alluvions Quaternaires de la Vallée de la Somme (Époques Chelléene et Moustérienne). Dessinées et Lithographiées en couleur par M. Pilloy, officier d'Académie [etc.]; et imprimées par M. Bourbier, Rue Saint-Jacques, 9, Saint-Quentin.] 4to. Pp. 10, pls. x. Saint-Quentin: Poette, 1892.

Under this lengthy title we gladly chronicle a book of ten pages and ten plates, relating chiefly to flint implements. The text is very brief, is dated May, 1892, and is mainly descriptive of the plates which follow. These plates are so remarkable in execution, and so true in colouring, that it is scarcely an exaggeration to say that they are as good as the implements themselves. The extraordinary markings, so familiar on broken flints, are here produced in a most perfect and striking manner; indeed, we had scarcely hoped that chromolithography had reached so high a degree of excellence. MM. Pilloy and Bourbier, for their names must be linked together in this beautiful production, have evidently worked for the love of their subject, and whether we regard these plates from the point of view of draughtsmanship or from that of artistic skill in printing, they have given us results of which they may be justly proud, and which, instead of being buried in our bookshelves, should rather find a place upon our walls.

Republication of Conrad's Fossil Shells of the Tertiary Formations of North America. By Gilbert Dennison Harris. 8vo. Washington: May, 1893. Price 3 dols. (after November 1, 3.50 dols.)

Closely following on Dr. Dall's collation of Conrad's works, Mr. Harris, who has devoted much time and labour to the collation of these rare publications, has issued an exact reprint of the text, and facsimile of the plates, of one of them. Mr. Harris was largely responsible for the facts in Dall's work, and has received full acknowledgment in the preface. Now that we are in possession of Conrad's paper, many doubtful points in the identification of species of American Tertiary fossils will be cleared up, and without entering into controversy as to the actual dates of publication of the parts of this work, we feel that the thanks of all palæontologists are due to Mr. Harris, in reproducing for us a book so rare, that it could only be compiled from fragmentary and incomplete copies of the original.

Catalogue of South Australian Minerals, with the Mines and other Localities where found; and brief remarks on the mode of occurrence of some of the principal metals and ores. By H. Y. L. Brown. 8vo. Pp. 36. Adelaide: 1893.

This Catalogue supplants that of Goyder issued in 1883, and is intended to supply miners, prospectors, and others, with a record of mineral discoveries for convenient reference. The author does not pretend that it is exhaustive, as many private collections which possibly contain minerals are unavailable. The Catalogue is arranged in alphabetical order, and in each case consists of the name of the mineral, followed by a list of the localities.

We understand that Messrs. Cassell & Co. have resolved to discontinue the issue of the *Year Book of Science*. Only two annual volumes (1891, 1892) have been published.

OBITUARY.

JOHN RAE, F.R.S.

Born 1813. Died July, 1893.

As briefly noticed last month, this veteran Arctic explorer has passed away. He was a native of the Orkneys, and at the age of 16 entered the medical school of the University of Edinburgh. In 1833 he was appointed surgeon on one of the vessels of the Hudson's Bay Company, and in 1846 he undertook an important survey of the North-east Coast of North America. Two years later Dr. Rae accompanied Sir John Richardson in searching for the missing Franklin expedition; and the next six years were occupied almost continually in exploring the Arctic regions. He first proved King William's Land to be an island, and his detailed surveys have placed him in the front rank of the pioneers in Arctic exploration. Besides numerous smaller memoirs, Dr. Rae published a "Narrative of an Expedition to the Shores of the Arctic Sea in 1846 and 1847" (1850) and in 1852 he was awarded the gold medal of the Royal Geographical Society.

JAMES WILLIAM DAVIS.

Born April 15, 1846. Died July 21, 1893.

As briefly announced last month, a sad breach has been made in the ranks of amateur naturalists by the death of Mr. James W. Davis, of Halifax, at the early age of 47. Born at Leeds in 1846, and educated at the local grammar school, he and a brother succeeded to the cloth-dyeing business of his father; and his active pursuit of scientific investigation was carried on only in such leisure as he could secure from business and from the numerous political, municipal, and educational movements in which he was a leading figure. The patronage of Art, Literature, and Science, and researches in Geology, Palæontology, and Archæology, formed Mr. Davis's means of recreation; and it is astonishing how much he was able to accomplish in the limited time at his command.

Though a prominent worker in the Yorkshire Societies from his earliest youth, Mr. Davis did not enter the scientific world as an original investigator until 1873, when he became a Fellow of the Geological Society and a member of the British Association. At first he confined himself to the Geology of Yorkshire, and in 1878 he co-operated with the botanist, Mr. F. A. Lees, in publishing the well-known volume on "West Yorkshire." He had always felt especially interested in the fish-remains discovered in the Yorkshire Coal-measures, and on making the acquaintance of the late Earl of Enniskillen and Sir Philip Egerton, at the meeting of the British Association at Belfast in 1874, his attention was definitely directed to Fossil Ichthyology. He published many small papers, chiefly relating to the Carboniferous fishes, until 1883, when, at the suggestion of the Earl of Enniskillen, he began to contribute a series of large illustrated memoirs to the *Transactions of the Royal Dublin Society*. His first memoir dealt with the fossil teeth and spines of the Carboniferous limestone, chiefly based upon the Enniskillen collection. The second memoir related to Professor Lewis's great collection of Cretaceous fishes from Mount Lebanon. Subsequently there were memoirs on the Cretaceo-Tertiary Fish-remains of New Zealand and on the Cretaceous Fish-remains of Scandinavia ; while only last year the first part of a contemplated series of memoirs on the Coal-measure Fishes of the British Islands appeared in the same form. It is a matter of profound regret that Mr. Davis's untimely death should thus abruptly end the work.

Mr. Davis, however, was not merely an indefatigable palæontologist—one who has left a permanent impression upon Fossil Ichthyology ; he was also a keen man of business, whose advice was eagerly sought in all manner of scientific undertakings. He had travelled extensively, and was on intimate terms with the leading men of his time in many walks of life. His extensive knowledge of the world, his genial disposition, and his enthusiasm in watching the advancement of every department of learning, gave him a position in British Science that none but his intimate friends could estimate ; and it may truly be said that the demise of so valued a patron and trusted an adviser is a great and almost irreparable loss.

GEORGE BROOK.

BORN 1857. DIED AUGUST 12, 1893.

THE death of Mr. Davis in the prime of life was an unexpected shock, but still sadder is the sudden demise of his intimate friend and fellow-Yorkshireman, Mr. George Brook. While out shooting on the moors in Northumberland on August 12, Mr. Brook was paralysed by sunstroke and died immediately, though just before apparently in the best of health. Mr. Brook was born at Huddersfield 36 years ago, and entered his father's well-known cotton manufactory in that town. His interest in scientific matters, however, soon estranged him from business, and he trained himself in scientific work until he became an accomplished zoologist. His earliest observations were made in a private aquarium he established at home, and some of his results were issued in pamphlet form. In 1884 he was appointed Assistant-Naturalist under the Scottish Fishery Board, and in the next year he became Lecturer on Embryology in the University of Edinburgh—a position he held until his death. Mr. Brook's first great work was his memoir on the Antipatharian Corals, published in the "Challenger" Reports in 1890; and for the last three years he had been occupied in cataloguing the corals in the British Museum. The first part of this catalogue, a fine quarto volume, dealing with genus *Madrepora*, and exquisitely illustrated with Mr. Brook's own photographs, has only just been issued. Now, alas! the work is suspended, and must be relegated to other hands. Mr. Brook was a member of Council of the Linnean Society, and the loss of his genial presence will be mourned by a large circle of friends.

GEORGE WILLIAM SHRUBSOLE.

BORN 1827. DIED JULY 22, 1893.

CHESTER has lost a valued citizen, and Science an enthusiastic worker, in the person of Mr. G. W. Shrubsole, F.G.S., the geologist and antiquary. He belonged to the Kentish family of the same name, being a brother of the well-known geologists, Messrs. W. H. and O. A. Shrubsole, and settled in Chester some forty years ago. Commencing as a chemist's assistant, in the course of a few years he started a business of his own in Market Square, which is now in the hands of his two sons. In addition to his retail trade, he had a considerable connection as an analytical chemist among mine-owners in North Wales. He was Honorary Curator to the Grosvenor Museum, a member of the Chester Archæological Society, and a painstaking student of the antiquities of his own city. Mr. Shrubsole was one of the founders, with Charles Kingsley, of the Chester Society of Natural Science, and held the office of Chairman of the Geological Section for some years. He contributed numerous papers on local matters to the Chester societies, and published a handbook to Chester antiquities on the occasion of the visit of the British Archæological Association. In 1879 and 1880 he contributed papers on the Carboniferous Polyzoa to the *Quarterly Journal of the Geological Society of London*, and in 1883 was awarded the Kingsley Medal of the Chester Society of Natural Science in recognition of the value of his researches.

MAXIMILIAN VON HANTKEN.

GEOLOGY in Hungary has sustained a loss by the death last June of Maximilian von Hantken von Prudnick. This venerable naturalist was Professor of Palæontology in the University of Budapest, Director of the Hungarian Geological Society, and a

Counsellor of the Hungarian Ministry, his chief care being, we believe, that of Education. V. Hantken's chief work was "A Magyar Korona országainak széntepelei és szénbányászata," 1878, a translation of which appeared about the same time in German as "Die Kohlenflötze und der Kohlenbergbau in den Ländern der Ungarischen Krone." He was the author of many papers on the Tertiary beds of his country, and paid special attention to the Foraminifera and Ostracoda therein contained: his papers on the former group occupying two and a half pages of Sherborn's "Bibliography." His "Clavulina Szabói regetck Faunája," with its sixteen beautiful plates, was really a monograph on the Foraminifera of this horizon, and his studies on the Nummulites have been of considerable value to others working in the same field. V. Hantken visited this country for the last time on the occasion of the International Geological Congress, 1888, when his amiability, courtly polish, and enthusiasm endeared him to many friends.

WE also have to record the death, at the advanced age of 87, of Miss Anne Pratt (Mrs. Pearless), authoress of the well-known "Flowering Plants and Ferns of Great Britain." Miss Pratt's first work on "Flowers and their Associations" appeared in 1826, and she published many small volumes on Natural History, chiefly Botany. She revised the last edition of her "Flowering Plants" so recently as 1880.

THE deaths are announced of the geologists, George Robert Vine and D. Homfray, the former well known for his researches on fossil Polyzoa, the latter a successful collector of Cambro-Silurian fossils in Wales; also of the Norwegian bryologist, Fantz C. Kiaer.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

We accidentally overlooked the name of Mr. William Fraser Hume in the list of successful candidates for the degree of D.Sc. in the University of London this year. We review Mr. Hume's thesis elsewhere.

The honorary degree of Ph.D. has been conferred upon Mr. Arthur Gardiner Butler, Assistant-Keeper of the Department of Zoology in the British Museum, by the Western University of Pennsylvania, U.S.A.

Dr. E. P. Ramsay, the curator of the Australian Museum, Sydney, is at present incapacitated for work, and is not expected to return for three months. During his absence the duties of the post are being performed by Mr. R. Etheridge, Junior.

Mr. T. D. A. Cockerell, whose resignation of the Curatorship of the Jamaica Museum we noticed some months ago, has been appointed Professor of Entomology, Zoology, and Physiology in the New Mexico Agricultural College, Las Cruces, New Mexico, U.S.A.

Professor W. J. Sollas has been appointed on the Geological Survey of Ireland, to carry on the petrological work, in the room of Mr. W. W. Watts, who has been transferred to the Geological Survey department in London. Mr. Watts now continues the examination and description of the Volcanic rocks of Great Britain.

Dr. A. Dendy, who has for the last four years been assistant to Professor W. B. Spencer at Melbourne, has recently been appointed Professor of Biology in Canterbury College, New Zealand. At present, during Professor Spencer's visit to England, Dr. Dendy is taking charge of all the biological work at Melbourne University, but after February next his address will be Christchurch, N.Z.

Mr. D. T. Macdougal, assistant in Botany at Purdue University, has been appointed instructor in Plant Physiology at the University of Minnesota. Mis Alice Eastwood has succeeded Mrs. Katherine Brandegree as curator of the Herbarium of the California Academy of Sciences and as acting editor of *Zoe*. Dr. J. M. Coulter, one of the editors of the *Botanical Gazette*, has left the Indiana University to become President of Lake Forest University, Illinois, and has taken with him his large herbarium, of which Mr. E. Uline has been appointed curator. Dr. Hans Schinz has been appointed to succeed Dr. Cramer as Director of the Botanic Gardens at Zurich; and Dr. F. Kaunhowen becomes Assistant in the Section of Fossil Botany in the Royal Prussian Geological Survey, Berlin.

The Smithsonian Institution has secured a table at the Naples Zoological Station for the use of American investigators, in response to a memorial signed by nearly 200 working biologists.

According to *Erythea*, Messrs Sandberg and Leiberg are at present engaged, under the auspices of the Department of Agriculture at Washington, in botanical exploration on the Columbia plains. The late summer and early autumn they expect to give to the eastern slope of the Cascades and the desert plains beyond. Mr. John Macoun is devoting the season of 1893 to the botany of Vancouver Island.

We understand that a new zoological quarterly will shortly be issued by the Tring Museum, containing memoirs on Mr. Rothschild's collections, partly by himself and his curators, partly by other specialists.

A new part of the "Notes from the Leyden Museum" (vol. xv., no. 3, July, 1893) has been issued. The contributions are technical papers on systematic zoology, the only one of general interest being a Comparative List of the Birds of Holland and England, by F. E. Blaauw. It appears that Holland is the regular home or resort of 221 species of birds, while the larger area of the British Isles has only 211 species—of course excluding casual visitors.

The collection of Italian fossils made by the late Baron A. de Zigno, of Padua, has been acquired by the University of that city. It comprises many type-specimens of Vertebrata described in the Baron's memoirs, and will form an important addition to the fine Geological Museum in the University building.

A new hall has just been opened at the Industrial and Art Museum of Melbourne. It is a fine large room, with a wide gallery running round, and is lighted by sky-lights. It is devoted chiefly to Practical Geology and Mining, and, besides containing numerous models and examples of mining machinery, has a fine collection of rocks and minerals, at present being excellently arranged by Mr. Walcott.

Owing to the prevailing depression the public scientific institutions of Australia are in a bad way. The vote for the Australian Museum, Sydney, has been reduced by a half, which involves the dismissal of over a third of the staff, and considerable lessening of the salaries of those who remain. A large new hall has recently been built at this Museum, chiefly for the reception of the geological collections. It is, however, impossible to exhibit the specimens, as no money is forthcoming to provide fittings for the cases.

Similar tales of woe reach us from other places. The collections of the Queensland Geological Survey were only a year ago moved from Townsville to Brisbane, where they have been displayed in appropriate quarters by Mr. Jack and his assistants, Messrs. Rands and Maitland. Now, since the Government can no longer pay the rent for the building, they must be shifted into rooms of little more than half the size.

The Ethnological Collection belonging to the Museum at Auckland, N.Z., has recently been moved into a new hall, where it has been admirably arranged by the curator, Mr. T. F. Cheeseman, F.L.S., who has drawn up and printed very useful descriptive labels. The collection is especially rich in Maori curios, but contains also many valuable specimens from the South Sea Islands.

THE American Museum of Natural History has despatched another expedition this summer to the western territories for the discovery of fossil mammalia. This is the third annual expedition of the kind, and the Museum is already beginning to exhibit a remarkable collection as the result of Dr. J. L. Wortman's former explorations. In 1891 only a small collection was made in the Wahsatch beds of the Big Horn Mountains. In 1892 the Puerco Eocene formations of New Mexico, the White River Miocene of South Dakota, and the Cretaceous Laramie rocks were examined. About five hundred specimens were obtained from the first-mentioned horizon, many fine fossils were discovered in the Miocene, and a very large collection of the minute mammalian teeth was made in the Laramie. The recent writings of Professor H. F. Osborn give some idea of the value of the investigation, on which the American Museum is to be congratulated.

A NEW scientific society, named the "Società tra i cultori delle Scienze mediche e naturali in Sardegna," has been founded at Cagliari, Sardinia.

THE new Malacological Society of London has decided to issue Proceedings at undetermined intervals. The publication will be edited by Mr. B. B. Woodward, and the first part may be expected shortly.

WE are glad to observe that the "Société belge de Géologie" has now succeeded in bringing its *Bulletin* up to date. Two parts have lately appeared, the one completing the volume for 1892, the other being the first part for the current year, The Treasurer is able to report that the deficit, which for several years has crippled the Society, is steadily disappearing, and that the year 1892 showed a small surplus.

THE Bristol Naturalists' Society has issued a new part of its *Proceedings* (n.s. vol. vii., pt. ii., 1892–93), containing several papers of much value in reference to the Bristol district. There are records of Phenological and Meteorological observations, Mr. A. J. Heath and Professor Lloyd Morgan publish a concise account of the fossil fish-teeth and spines found in the Carboniferous Limestone, with some original observations; Dr. A. R. Prowse discusses the ancient British remains on Clifton Down, with a map; and Mr. C. Druitt has an interesting original paper on the Green Woodpecker, based on his own observations in the neighbourhood.

THE Croydon Microscopical Club has just issued its *Proceedings and Transactions* for Feb., 1892–June, 1893. The most original paper is that of Mr. Murton Holmes, who discusses "The Microscopical Structure of Hearthstone from Detchworth, Surrey." Foraminifera and casts of the same in glauconite. sponge spicules, coccoliths and rhabdoliths, are recorded, with abundant scales of mica. It is gratifying to learn that the loss on the last soirée was only £10; for the Croydon Club soirées are excellent as a rule, both in exhibitions and arrangement.

CORRESPONDENCE.

PHYLOGENY *v.* ONTOGENY.

A CHANCE has enabled me to see Dr. Hurst's reply to my article on "The Recapitulation Theory in Palæontology." Without reference to his original paper it is impossible for me adequately to examine his argument; but, that I may not be thought to ignore his defence, I venture to send these lines.

I am not so sure that I have misunderstood Dr. Hurst's position, despite his assertion that none of my missiles hit it. At any rate, it is clear enough now: he does not deny that, in many cases, individual development may follow the same lines as the evolution of the race; he merely denies that the past history of its race has any influence on the growth-stages of an individual. Considering the impossibility of producing experimental evidence, such a position is, in strict logic, impregnable. For, let us suppose that the ontogeny of every individual was an epitome of its phylogeny, it would still be open to anyone to deny any *causal* relation between the two. But, if I remember rightly, Mr. Hurst, in his original paper, maintained that the development of any individual was a regular progress from the embryo to the adult; modifications of early stages, adaptations to larval environment, might of course occur, but, on the whole, each growth-stage represented an approximation to the adult. Consequently, when race-history consisted in a regular advance from the primitive ancestor to the specialised descendant, then the individual development must normally repeat it; ontogeny and phylogeny would necessarily move in the same direction along parallel lines, but there would be no need to suppose any causal relation. But if the history of a race were no such regular advance; if, on the contrary, it could be clearly shown by the evidence of fossils that the race had ascended to an acme and then descended; if a structure could be seen to appear and disappear during this history; then it is equally clear that, on Mr. Hurst's hypothesis, ontogeny would not correspond with phylogeny; it is clear that vanished structures or forms would not temporarily make their appearance during the growth of an individual, unless indeed they were produced by some other cause, as adaptations to a temporary environment. This appeared to me to be Mr. Hurst's position, and the following was the nature of my attack. I adduced instances of structures or forms appearing in the earlier stages of individual development, and then disappearing; these characters were not such as could be supposed due to larval adaptation or to any modifying causes of a temporary nature; but these characters could be shown by palæontological evidence to have arisen and disappeared in just the same way in the history of the race. Now, I will merely ask Mr. Hurst to tell me what cause can have produced these deviations of ontogeny from the path of simple development. Unless some plausible explanation can be found, it seems reasonable to suppose that they are due to heredity, and are, in fact, as well as seeming, recapitulations of ancestral history. In Mr. Hurst's reply I see no attempt to meet these difficulties. He merely denies that difficulties exist, and asserts that my shots have all passed by him. Despite his explanations it seems to me that the only reason he has not felt my attack is, because he is encased in some bullet-proof cloth or other, which has a little dulled his perceptions.

It is needless to reply to Mr. Hurst's other remarks while the above question remains unanswered. Only with regard to the figures illustrating the ontogeny and phylogeny of *Antedon* which he suggests should accompany a fuller account in a future number of NATURAL SCIENCE, it seems enough to refer those interested to the classical description by Dr. W. B. Carpenter (*Phil. Trans.*, 1879), and to the second of my own papers on British Fossil Crinoids (*Ann. Mag. Nat. Hist.*, 1890).

Townsville, Queensland, F. A. BATHER.
20 June, 1893.

"RECAPITULATION" AND "EARLIER INHERITANCE."

IN reply to Mr. Buckman's remarks on pp. 138 and 139, let me beg him to be more careful in future.

I have never expressed any opinion whatever as to what views he had adopted either "as creeds" or otherwise, nor as to the nature of the "inspiration" under which he has written, and I have not either on p. 198 (vol. ii.) or elsewhere denied that there is "any" causal relation between ontogeny and phylogeny. P. 197 (vol. ii.) treats, as there distinctly stated, not of changes in individuals but of changes "in average constitution of successive generations" leading to production of new species. The word "structure" was used deliberately on p. 197 (vol. ii.), and in its strictest sense. I did not mean "character," I meant that complex abstract quality made up of *all* the characters together.

The "law of earlier inheritance" is on a par with the law of the north wind. Because characters are in *rare and exceptional cases* (*see* Darwin, "Origin," 6th edition, p. 10) "inherited" earlier in the offspring than in the parent, are we to have a "law" enunciated to the effect that the rare and exceptional is not rare or exceptional but universal? The "law" of the north wind states that all winds at all times and in all places blow from the north. Many authors might be quoted to prove that a north wind had occasionally been observed in some places. The "law" stands on as good a basis as does that of "earlier inheritance."

I used the term "late stages of development" believing that it would be clear from the context that I meant stages which, though late relatively, are passed through before the adult stages are reached.

The "contention" concerning loss of teeth and other senile changes does not affect me. It is at most a matter of words, and probably everybody agrees with both Mr. Buckman and myself that the words may properly be used in the way he has "contended" for.

By "acceleration of development" I mean a change in the average constitution of successive generations of a species by virtue of which the descendants of to-day pass more rapidly through some or all of their ontogenetic changes than did their ancestors. Is another meaning for the term in use?

Physiologically, of course, the term is applied to the shortening of the period of development by the artificial application of heat, etc., *e.g.*, in Weismann's experiments upon seasonal dimorphism ("Studies in the Theory of Descent," vol. i., p. 120, last paragraph).

I regret that the *Proc. Cotteswold Nat. Soc.* are not accessible to me, and I have therefore not read Mr. Buckman's "Some Laws of Heredity." I hope I may yet have an opportunity of doing so.

C. HERBERT HURST.

THE CLASSIFICATION OF ARACHNIDS.

IN a paper on some recent memoirs on "The Classification of Arachnids," by Mr. G. H. Carpenter (NATURAL SCIENCE, no. 16, June, 1893), it seems to me that the author has sadly neglected the work of at least one writer on the same subject; leading the unwary reader, if I am not mistaken, to conclude that many of the theories mentioned in that review are novelties brought forth for the first time, within the last year or two, by Mr. Pocock, whereas they are really the views published twelve years ago by Professor E. Ray Lankester in a classical paper not even mentioned, namely "Limulus an Arachnid" (*Quart. Journ. Micr. Sci.*, vol xxi., 1881).

Now, I venture to think that in a review of that kind, intended presumably to be read by students and others not intimately acquainted with the literature of the subject, the greatest care should be taken to avoid conveying, by implication or by direct statement, an erroneous idea as to who was the discoverer of any new fact of importance or the originator of a theory. For instance (I am speaking, of course, of the review, and not of Mr. Pocock's valuable paper), it is stated, page 448, that "On these grounds he [Pocock] considers the scorpions as the lowest living branch of the arachnid stem, in opposition to the views of Thorell and others, who have regarded them as the highest. There can be no doubt that Pocock's opinion will meet with general acceptance among biologists. Deriving the arachnid orders immediately from an ancestor with a long abdomen of twelve segments and a telson, Pocock indicates the modifications which have probably occurred in each group . . . nearly related to the Pedipalpi . . . are the Solifugæ . . . , on a higher branch . . . come the harvest men (Phalangida) . . , from these the mites (Acari) . . . a degraded offshoot." Turning to Lankester (*loc. cit.*, p. 644) we find, "the Scorpions, having once been developed, appear to have given rise to the whole series of living Arachnida, to the Pedipalpi first, and through these to the Araneina, and through the Araneina to the Acarina . . . we have to admit a very extensive process of degeneration . . . leading from the Scorpion to such Acarina as Demodeæ . . ."

In the discussion (p. 450) on the light thrown by palæontology "upon the question whether the lung-bearing or tracheate orders of arachnids are the more primitive," no notice whatever is taken of the mass of evidence collected by Professor Lankester showing the relationship between the Scorpions and the extinct Eurypterina and Trilobita.

On page 449, we are led to believe that Mr. H. M. Bernard originated the view that the "spinning-glands, coxal glands, and poison-glands" were derived from the "ventral setiparous glands of an annelid ancestor," a theory put forth by Eisig in his famous Monograph of the Capitellidæ.

The Museum, Oxford. E. S. Goodrich.

"Unilateral" Sleep in Animals.

The late Sir Emerson Tennent, in his "Natural History of Ceylon" (1861) at page 279 observed that each eye of the Chameleon *(Chameleo vulgaris*, Daud.) had not only a separate action quite independent of the other eye, but that also one side of its body appeared sometimes to be asleep while the other side was vigilant and active.

Hence, while one side of the Chameleon was green, the other was red.

It is said that the Chameleon is unable to swim because the muscles of its two sides cannot act in concert. Is this true?

Similarly some other accurate observers, including Matthias Dunn of Mevagissey, suspect that certain fish can secure partial sleep with one eye at a time.

I should be grateful for further information and references as to whether other animals have been observed in this alleged condition of one-sided sleep while the other side of their body continues awake.

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NOTES AND COMMENTS.

The Organisation of Science.

ONE of the most gratifying announcements of the month in British Science, is a statement by Lord Rayleigh to the effect that the Royal Society of London has lately appointed a committee to consider, in an entirely impartial manner, whether or not the procedure of that body can be altered with advantage. His lordship's speech was made before Section A of the British Association, in the course of a discussion on the publication of scientific papers. The physicists have been much excited of late on the publication question, and it was thought that a debate at Nottingham might do something further towards a solution of difficulties.

The anonymous author of a little pamphlet "On the Organisation of Science," which we reviewed in June, 1892, seems to have been the first, in recent years, to issue a reasoned plea for a more systematic method of publication of scientific papers, and the centralisation of the publishing authority; but as we pointed out at the time, and as Lord Rayleigh remarked at Nottingham, there is the great difficulty of censorship and the fear of a dominating clique. One of the essentials for progress is absolute freedom; and so long as there are competitive journals and rival societies, there is little danger of any novel views being suppressed merely on account of their unorthodoxy. The physicists and mathematicians of the British Association seem to have been agreed that there is no real necessity for interference with the "disorganisation" of present arrangements; but that there ought to be some recognised central authority for the periodical issue of volumes of abstracts relating to each branch of Science. This is the work that Professor Rücker and some others would like to see undertaken, or at any rate supervised, by the Royal Society; and the committee will certainly confer a great boon upon workers if they are able to make any suggestions for rendering records of progress more systematic and elaborate than they are at present.

Scientific Linguistics.

When a layman asks a naturalist why he invents and employs such a multitude of incomprehensible technical terms, the common reply is that exact ideas necessitate a precise and universally-understood nomenclature. We wonder how this explanation would apply to the terms of "Auxology" or "Bioplastology" just discussed again by Professor A. Hyatt in the *Zoologischer Anzeiger* (concluded August 28). We should like to know how much scientific precision there is in the determination of the nepionic, metaneonic, gerontic, paragerontic, etc., stages of any organism; and what grain of solid fact, as compared with mere speculation, in the so-called definition of the phylonepionic, phyloneanic, phylogerontic, etc., phases of development in any group of animals. We may be enslaved by some prejudice, and our patience may have been ruffled in the attempt to decipher some recent writings of American authors on fossil shells; but we cannot help uttering a protest against the clothing of a tissue of hypothetical fabrications in the garb of a precisely-defined scientific nomenclature. It is, of course, a matter of everyday knowledge that each organism passes through several marked stages in its individual development; and most naturalists will admit that a good deal of evidence is constantly being discovered as to the evolution of races, genera, and species in course of time. Nevertheless, there is as yet nothing very exact in this knowledge; and until that exactitude is reached, the invention and application of scientific terms is a delusion and a snare, and a veritable hindrance to progress.

The embryologists need not be considered in the matter; for they do not seem to have taken kindly to the classification of stages of development by those whose practical acquaintance with the study of embryos is, for the most part, probably *nil*. The latest work on the subject (Marshall's "Vertebrate Embryology") does not even mention that any such classification has been attempted. When they begin to discuss protembryos, mesembryos, metembryos, etc., it will be time to return to the subject, and examine the cogency of their reasoning.

It is with the speculative palæontologists that we have to deal. It is they who arrange ammonites and brachiopods in rows, and unfold to us, with surprising confidence, the history of a genus, a species, or a race. The manner in which they are studying growth, variation, and the stratigraphical sequence of forms, by the examination of enormous collections, is one of the most gratifying signs of the times. This is certainly the only method by which the modern ideas of Biology can be advanced. It ought, however, to suffice at present to record clearly and simply the facts, carefully distinguishing all legitimate suggestions as to their likely explanation; and, to our mind, it is one of the greatest misfortunes to entomb and obscure these plain facts in an array of would-be scientific expressions, which are nothing more than the outcome of the guesswork of each individual author.

If there is any logic in this attempt at a nomenclature, why do not Messrs. Hyatt, Beecher, Buckman, Bather, and its other advocates complete its definition in general terms? Why do they not show how to correlate the growth-stages, for example, of a brachiopod with those of a mollusc, and these, again, with those of a mammal? How do they determine which period in the life of a snail is scientifically equivalent to one of a crab? If no reasoned answer to these questions can be given, then, assuredly, the time has not yet arrived for applying technical terms to the various periods of the life-cycle.

In certain quarters, indeed, Biology seems to have reached a phase equivalent to that in which the originators of the Geological Society of London found the "Theory of the Earth" in 1807. That Society was established to combat the spirit of the day, "to multiply and record observations, and patiently to await the result"; and it was at first their favourite maxim "that the time was not yet come for a general system of geology, but that all must be content for many years to be exclusively engaged in furnishing materials for future generalisations." The older Societies dealing with Biology in Europe are wise enough still to withhold their patronage from any such reckless developments of "Auxology" and "Bioplastology" as we find in some magazines and transactions elsewhere; and the sooner the false appearance of this "whited sepulchre"—this clothing of pure hypothesis in a garment of precise terminology—is demolished, the more conducive will it be to sound progress.

Lake Superior.

An important geological memoir bearing upon the question of the local instability of the earth's crust has been issued by the Survey of Minnesota, U.S.A.[1] The evidence of changes of level on oceanic coast-lines is, of course, easily enough observed; but nearly everywhere inland very little progress has hitherto been made in detecting such movements. When, however, the area is large enough, recent unequal changes of level can usually be discovered by the study of the topography, such as the nature of stream-erosion, the distribution of sediment, and the deformation of the abandoned shore-lines of lakes. It is from this point of view that Professor Lawson has studied the old beaches to the north of Lake Superior.

It is now generally recognised that streams are very sensitive to any change in the slope of their trenches or of any portion of them.

[1] A. C. Lawson, "Sketch of the Coastal Topography of the north side of Lake Superior, with special reference to the Abandoned Strands of Lake Warren (the greatest of the late Quaternary Lakes of North America)," *Geol. & Nat. Hist. Surv. Minnesota*, 20*th Annual Report*, year 1891 (1893), pp. 181–289, with illustrations.

There is a uniform minimum slope which they constantly seek to establish and maintain. Any movement of the land leaves its record in a change in the intensity of the action of the stream, whether it be cutting or depositing; and in non-glaciated regions streams are now systematically examined by geologists for the purpose of ascertaining whether the country traversed by them has been uplifted or depressed, or has maintained a fairly constant altitude. Evidence of this kind, however, is not always available, and it is necessary to turn to other features. For instance, there are some regions in which the shores of ancient lakes can be traced for many miles—in the centre of North America sometimes for hundreds of miles. These old beaches must, of course, have been originally horizontal, and if, therefore, any one of them appears now to be displaced, variously inclined, and at somewhat different levels in different parts, there is clear evidence of movements of the crust since the lake dwindled or disappeared. Such evidence of earth-movements has been known for some years in North America on either side of the Lake Superior basin,[1] and Professor A. C. Lawson's investigation of the old beaches to the north of the present Lake Superior was undertaken with the object of ascertaining how far the effect of movements could be traced in that region. A beautifully illustrated detailed report leads to the conclusion that there once existed an enormous "Lake Warren" (to use Professor Spencer's term) probably at least twice as great as the combined areas of the present lakes Superior, Michigan, and Huron, or about 150,000 square miles. In some places as many as nineteen distinct beach-lines can be traced along the northern shore of Lake Superior, and Professor Lawson believes there is evidence of at least thirty-two definite stages in the recession of the lake. It seems probable that the lowering of the water was gradual, though perhaps varying in amount at different times; and it is hardly possible to believe that the existence of ice barriers had any connection with the phenomena. It is likely, therefore, that the local warping of the earth's crust, in some region far removed from Lake Superior, is accountable for the lowering of the land barrier which held back the waters of "Lake Warren." The outlet may even have been at different places at different times in consequence of the unequal continental warping of which there is evidence elsewhere. Professor Lawson enumerates the possible situation of some of these outlets, and makes many suggestions that will be of much service to future observers, both in this and other regions.

The Feeding of Fishes.

It is well-known that the sense of smell in fishes is very keen, and that all use it more or less in feeding, whether or not sight aids

[1] *See* "Great Lakes," by Clement Reid, Nat. Sci., vol. i., p. 117.

them in the process. Some further experiments on the subject have been made by Mr. Gregg Wilson in the Plymouth Marine Biological Association, and the following observations from his recent report to the British Association will be read with especial interest :—

"So far as I could determine, fish that are not *very hungry* habitually smell food before taking it. The pollack seems usually to be ready for a meal, and on almost all occasions when anything eatable is thrown into the tank in which it is swimming, it rushes towards it and bolts it. It does not hesitate to take stale food or food that has been steeped in strong smelling fluids; and time after time I have been amused to see its too-late repentance, after it had swallowed clams that had been saturated with alcohol, chloroform, turpentine, etc. It is only when it is satiated with fresh food or disgusted with what is nauseous that it takes the precaution to smell before eating. On the other hand, various fish that are equally keen-sighted, and habitually recognise their food by the use of their eyes, are more prudent. The whiting (*Gadus merlangus*), for instance, appears to pay much more attention to smell, and, as a rule, turns about and withdraws on approaching within a few inches of high-smelling objects that the pollack would take without hesitation. Even whiting, however, cease to be delicate if they are very hungry, and if other fish are present to compete for the food that is thrown to them. In such circumstances bait that is very distasteful may be taken by even the most cautious of sight-feeders; and likewise, in such circumstances, a quite smell-less artificial bait may be successfully employed. Where large shoals of fish are, there are likely to be many that are very hungry, and the consequent keen competition will lead to hasty feeding by sight alone; and hence it is, probably, that lead-baits are successfully employed in cod fishing in the Moray Firth and off the Northern Islands, while they are of no avail among the scanty fish further south.

"It may be said that in these cases the fish actually *search* for their food by sight alone, and merely test the quality of what they have found by smelling it; and Bateson quite recognised this. But more is possible: *habitual sight-feeders can be induced to hunt by smell alone.* The pollack, which is such a pronounced sight-feeder that it will take a hook baited with a white feather or a little bit of flannel and trolled along the surface, is yet able, when blinded, to get his food with great ease. Several blind specimens in the Plymouth tanks were carefully watched by me; and I had no difficulty in deciding that it was by smell alone that they found their food. Their conduct was exactly such as was seen in the smell-feeders, to which I shall presently refer.

"Again, the cod (*Gadus morrhua*), which Bateson puts among the sight-feeders, is generally believed—and with good reason, I think—to feed more by night than by day; which suggests that it, too, not only tests its food, but actually hunts by smell.

"Lastly, in this connection I would state the results of my experiments. I worked with a number of fish, and always with the same success; but I shall here only refer to one case—that of the dabs (*Pleuronectes limanda*). That they were sight-feeders was evidenced by their behaviour when I lowered a closed tube full of water, and with a worm in the middle of it, into their tank; time after time they bumped their noses against the glass at the very spot where the worm was situated. That they could also recognise the smell of food, apart from seeing it, was demonstrated in various ways. First, if instead of a closed tube, as in the last-mentioned experiment, one open at the bottom was used, after a short interval the nosing at the part where the worm was seen ceased, and *the lower end of the tube*, from which, doubtless, worm-juice was diffusing, was vigorously nosed. If, again, instead of putting worms into a tube I placed a number of them in a closed wooden box with minute apertures to let water pass in and out, there was a similar excitement produced, and the dabs hunted eagerly in every direction. When water in which many worms had lain for some time was simply poured into the tank through a tube that had been in position for several days, and by a person who was out of sight of the dabs, the results were most marked. In a few seconds hunting began, and in their excitement the dabs frequently leapt out of the water, apparently at air-bubbles, and on one occasion one even cleared the side of the tank, which was about two inches above the water, and fell on to the floor of the aquarium. Yet there was nothing visible to stimulate this quest."

The Thyroid Gland.

In the August number of our excellent contemporary, the *Medical Magazine*, Mr. Lorrain Smith has an interesting paper on the thyroid gland, which, as everyone knows, is the fashionable subject in current Pathology. It has been shown recently that many glands in the mammalian body have, at least, a double function: they form their special secretion as the liver, for instance, forms bile, or the kidneys remove nitrogenous material from the blood; but in addition they have some part to play in the general economy of the organism. Their loss by disease or excision not only prevents the formation of the special secretion, but disturbs the general economy of the body. The thyroid is a gland limited apparently to the second form of activity. This activity is at present being sought for: some results go to connect it with the regulation of heat; others with the general metabolism of the body. So far physiologists have not paid much attention to the comparative anatomy and development of the thyroid, but, without question, this side of the subject will raise problems of very general interest. It is, for instance, most probable that the thyroid was originally a slime-secreting organ correlated with the mode of life of pelagic forms: it secreted slime to gather together the small organisms in the intaken sea-water. The gradual change of function of any organ is a biological problem of vast interest, and here seems a case to be dealt with by the morphologist and the physiologist hand in hand.

The Polynesian Islands.

Under the title of *Flora of French Polynesia*, M. Drake del Castillo gives a systematic account of the Flowering Plants and Ferns which grow spontaneously or are generally cultivated in the Society, Marquesas, Pomoton, Gambier, and Wallis Islands. The Society Islands are by far the most important, whether we consider extent, population, and productions, or flora, and of these Tahiti holds the first rank.

With but slight differences, the climate of French Polynesia is the same as that of the other Polynesian islands. In the Society Islands, the mean annual temperature is a little above 75° F. The year is divided into two seasons of almost equal duration, determined by the direction of the wind. From August to October a S.E. wind, called *Maarama* by the natives, prevails, and the weather is rather dry, though storms are not rare. A N.E. wind rules from November to March, and rains are frequent. However, the difference between the two seasons is not strongly marked, and it rains more or less during the whole year. The climate is moister in the high valleys than near the shore, and there is a corresponding difference between the vegetation of the two regions, while the eastern side being more exposed to the storms brought by the S.E. winds, the limit of the humid zone is lower than on the western side.

The Marquesas are drier than the Society Islands. These conditions of climate necessarily produce a luxuriant vegetation, which is, however, more brilliant than varied, and remarkable more for the development in number of individuals than species. The poverty of the flora chiefly consists in a want of special forms, a characteristic of small islands. Annuals are poorly represented, almost two-thirds of the whole vegetation consisting of small perennial shrubs, while trees and large shrubs constitute another third. This predominance of shrubby plants is explained by the nature of the soil. On the abrupt sides of the valleys, woody plants with short stems and vigorous roots, and ferns with creeping rhizomes, are almost the only kinds which can profit by the scanty support afforded by the soil. Tall trees occur for the most part only in the lower valleys or in the ridges. Feebler plants can only get on by the help of their neighbours, which they use as supports or hosts; hence it comes that 15 per cent. of the vascular plants are climbers, parasites, or pseudo-parasites. The few herbs live on the borders of streams or dry hills. Owing to its rocky nature and steep slopes in the mountainous islands, the soil retains but little of the large amount of water it receives, while the narrow deep valleys keep the air constantly charged with moisture. Thus, if the roots absorb little, the leaves exhale but little, a fact which explains the chartaceous or coriaceous nature of the foliage of many species.

M. Castillo remarks that the flora of French Polynesia is a poor one, containing, according to our present knowledge, only 588 species of vascular plants distributed among 79 families, and representing 262 genera. They are classed under three headings. First, species confined to the islands, numbering 161; secondly, species distributed throughout Oceania, 123; and thirdly, comprising a half of the flora, or 297 species, those which are also found in the Indo-Malayan region. The predominating families are, Ferns, Leguminosæ, Orchids, Rubiaceæ, Grasses, Cyperaceæ, Euphorbiaceæ and Urticaceæ. The predominance of ferns—with their light and easily-carried spores—over the less easily dispersed flowering plants is a well-known characteristic of insular floras, consequently it is not surprising to find this group represented by as many as 142 species, or nearly one-fourth of the whole. There are 37 species of Leguminosæ, 34 Orchids, 31 Rubiaceæ, and 30 Grasses. A *Freycinetia* is specially mentioned as very plentiful and covering large areas, and along with the plaintain (*Musa paradisiaca*), a plant of a very different type, recalling the vegetation of a Malay or East Indian jungle.

Lake Vegetation.

In two recent numbers (nos. 54 and 55) of the *Revue Générale de Botanique*, A. Magnin discusses the vegetation of the lakes of Jura. After exploring a number of lakes, the regularity with which the plants are distributed becomes very striking. Passing from the shore to the centre, distinct zones are noticed somewhat as follows.

1. A littoral zone of reeds (*Phragmites communis*) and bulrushes (*Scirpus lacustris*), which stand out of the water. Sometimes the two plants are more or less intermingled, but usually the reeds alone line the shore, forming the *Phragmitetum*, extending to a depth of 2–2½ metres, while the bulrushes form a second ring, or *Scirpetum*, to a depth of 3 metres, a mixed zone uniting the two. Sometimes the order is inverted through the formation of a bar beyond the shore, the reeds installing themselves in the shallower water. Besides these, where the water is shallow, sedges, *Phellandrium*, *Equisetum limosum*, and others are observed, as well as some plants with swimming leaves, like *Polygonum amphibium*, the white water-lily (*Nymphæ aalba*), and *Potamogeton natans*.

2. Beyond the bulrushes plants with stems standing out of the water are no longer found, but only those with swimming or floating leaves. Especially characteristic of this zone is the yellow water-lily (*Nuphar luteum*), which occurs in almost all the lakes of the Jura, frequently forming a large continuous girdle, or great patches, from the limit of the scirpetum to the edge of the first slope to a depth of 3–5,

usually 4, metres, and constituting the *Nupharetum*. Sometimes it is associated with species of the preceding zone, chiefly *Potamogeton natans*, while the water milfoil (*Myriophyllum*), *Potamogeton perfoliatus*, and others of the following zone begin to appear.

3. The third zone, or *Potamogetonetum*, is formed by plants different according to the lake; sometimes pondweeds, chiefly *Potamogeton perfoliatus*, then *P. lucens*, or *P. crispus*, less often *Myriophyllum spicatum*, or still more rarely the mare's-tail (*Hippuris vulgaris*), all bearing their leafy and flowering branches at or near the surface, on stems 4 to 6 metres long. Their rootstocks occupy the edge of the slope, below the yellow water-lily, at a depth of 4–5 metres, descending to a depth of 6 and 7 metres. Here also lives the hornwort, *Ceratophyllum demersum*, which, after wintering in depths of 3 to 6 or even 8 metres, breaks loose and becomes quite free, floating like the bladder-wort (*Utricularia vulgaris*).

4. The fourth or deep zone, the *Characetum*, is formed by plants which never reach the surface, but remain always fixed to the bottom at a depth of 8, 10, and 12 metres. Such are the numerous Characeæ, *Naias major*, and the mosses *Fontinalis antipyretica* and *Hypnum giganteum*.

In deep lakes with rocky borders, plant-life is absent or represented only at points where erosion or a falling has occurred by tufts of reeds or bulrushes, associated or isolated, and sometimes followed inside by the yellow water-lily and *Potamogeton perfoliatus*. The turf-lakes with abrupt, sometimes sharply-sloping margins differ in having a very narrow littoral zone, consisting of the Bog-bean, (*Menyanthes*), the reed mace (*Typha*), *Cladium*, the reed, and the bulrush, while the yellow water-lily reaches almost to the edge. The floor is generally carpeted, to a great extent, with myriophyllums and charas, together with, in some lakes, the hornwort, the two mosses above mentioned, *Nitella* and others.

Shallower lakes with muddy or marshy banks with but a slight incline, and alternately swamped or exposed, show a very variable vegetation. In some can still be seen the littoral zone, the yellow water-lily zone, and a bottom covered with myriophyllums and charas; in others mare's-tail and water milfoil clothe the whole lake, while scattered patches of *Nuphar*, *Polygonum amphibium*, and others are seen at the surface.

Classification of Conifers.

The most recent issue of the Linnean Society's *Journal* (vol. xxx., no. 205) contains some valuable notes by Dr. Masters on the genera of the two large orders of Gymnosperms, Taxaceæ and Coniferæ. The observations are the outcome of a comparative examination of the morphological characters of all the genera as far as was possible in living plants, while the available museum and herbarium specimens have been studied as well as the literature. Dr. Masters is well-known as an authority on the subject, and the notes are a valuable addition to the work he has already published on Conifers and Taxads. The orders, tribes, and genera are carefully defined, and the history of each genus as well as the morphology of the vegetative and reproductive organs is discussed. Constant reference is made to the schemes of arrangement of the older writers, and, more recently, of Eichler, Van Tieghem, and others, but, in the main, the author has followed Bentham's plan, as elaborated in the "Genera Plantarum." Several deviations from the last arrangement claim to be "more in harmony with ascertained facts, or with more recently-acquired information." Thus, the Oriental genus *Cephalotaxus*, which Bentham places among the Taxodineæ, forms with *Gingko* and *Torreya* a distinct tribe, Salisburineæ, of Taxads. The drupe-like seed, and the germination, recall those of *Gingko*, and another point which separates both from all other Conifers is the presence of a resin-canal in the centre of the pith, observed by Van Tieghem. That author is also responsible for the genus *Stachycarpus*, a name originally proposed by Endlicher for a section of *Podocarpus*, to include those species where the peduncle does not become fleshy. The characteristic of the new genus is the arrangement of the fruits on a loose spike, the axis of which does not become fleshy. There are also distinctions in minute structure.

An important deviation from Bentham's arrangement is the splitting of the genus *Callitris*. In the "Genera Plantarum" this includes a species from North Africa, a few from Southern and Tropical Africa, and a number from Australia. Dr. Masters separates the North African plant as a new genus *Tetraclinis*, distinguished by its flattened (not triangular) Salicornia-like branches, and its solitary female cones, the axis of which is not prolonged beyond the four scales. Endlicher's *Widdringtonia* is revived for the other African and Madagascar species with angular branches, panicled female cones, and four unequal scales, while *Callitris* is reserved for the Australian species, the genus *Frenela* of Mirbel differing from *Widdringtonia* in having six scales in the female cone. Another genus of Endlicher, *Glyptostrobus*, included by Bentham, following Brongniart, in *Taxodium*, is restored.

In the tribe Abietineæ, which includes the spruces, cedar, larches, firs, pines, and their allies, Dr. Masters recognises the same genera as Bentham, except that Carrière's *Keteleeria* is revived on the ground of Professor Pirotta's recent observations on the male flowers and other remarkable characteristics. It contains, besides the original *Abies Fortunei*, a few other Chinese species.

The paper concludes with an account of the geographical distribution of the genera. China and Japan are remarkable for the number and variety of their Taxads and Conifers; two genera *Pseudolarix* and *Keteleeria* are confined to China, *Cunninghamia* extends to Cochin China, while *Gingko* is of Chinese origin, but widely cultivated in Japan. *Sciadopitys* is exclusively

Japanese. In North America the rocky mountains form a marked dividing line, the species to the west and east being, though closely allied, for the most part different. No true Conifers are found on the Andes, though the range forms, as it were, a continuation of the Rocky Mountains; their place is taken by Podocarps and Taxads working up from the south. *Juniperus*, *Libocedrus*, and *Pinus* are the only true Conifers found both north and south of the Equator, while among Taxaceæ, *Podocarpus* and *Dacrydium* occur in both subdivisions. Junipers, cypresses, spruces, firs, and pines spread across the northern hemisphere from west to east, but there is no such continuity in the south among true Conifers, though *Araucaria* is common to Eastern South America, Chile, Australia, New Zealand, and some of the South Sea Islands.

In the recent issue of the *Trans. and Proc. New Zealand Inst.* (vol. xxv., 1892) T. Kirk describes the heterostyly, or differences in length of the style or stamens of flowers in relation to reciprocal fertilisation in the New Zealand Fuchsias. Each of the three species, *Fuchsia excorticata*, *F. Colensoi*, and *F. procumbens*, is trimorphic, having a long-styled, mid-styled, and short-styled form. The arrangements for cross-fertilisation are much less complicated than in our own purple Loosestrife (*Lythrum salicaria*) as the long-styled flowers are, at any rate in the first two species, practically female, the pollen-grains being abortive.

The three forms of *Fuchsia excorticata* grow intermixed, usually in about the same proportion, wherever the plant is plentiful, the long-styled form producing the largest quantity of fruit, a fact which, it is suggested, is largely due to the application of pollen from both the mid- and short-styled forms. This assumption is supported by the great reduction in the quantity of fruit on the long-styled form in the few observed cases where one of the others appeared to be absent. The author knows of no instance of the different forms of *F. procumbens* growing intermixed; only one being found in any one locality. This, he says, may well account for the fact that the handsome fruit of this species has not been seen in the wild state. The mid- and short-styled forms are often cultivated, but were never seen in the same garden. Under these conditions although most frequently sterile, they do occasionally fruit. Hence it is inferred that each form of flower is almost sterile with its own pollen.

Dr. E. v. Halácsy spent last June in a botanical exploration of some of the North Peloponnesian mountains, and his collections may be expected to yield results of interest. The characteristic elements of the different plant formations were noted, and the upper and lower limits of the vegetative zones determined. Of special interest is the discovery of a Berberidaceous plant on the northern slope of Panachaion, most nearly allied to *Lemtice Altaica* from the Altai mountains, and probably representing a new genus. On Mt. Olenos Dr. Halácsy found above the fir region a girdle of large trees of *Juniperus fœtidissima*. The high mountain flora at the edge of the snow-fields made a gorgeous display, especially in the heights of Chelmos, where *Ficaria Peloponnesiaca*, *Anemone olanda*, Crocuses, Scillas, the endemic violet (*Viola Chelmiani*), and others, formed brilliant carpets, but nowhere were the species found which characterise the Austrian alpine flora.

BIOLOGISTS, as a rule, are apt to overlook the physical aspect of the facts and phenomena with which they deal, and it is therefore gratifying to observe the occasional incursion of the Physicist into the Biologist's domain, such as has just been made by Professor Johnstone Stoney. The Professor asks (*Sci. Proc. Roy. Dublin Soc.*, n.s., vol. viii., pp. 154–156): whence comes the vital energy of the innumerable bacilli which are excluded from the direct influence of sunlight, and why are these organisms all extremely minute? There are some bacilli, *e.g.*, the nitrifying bacilli of the soil, which seem to thrive entirely upon mineral food, and, not only so, but perform their functions while completely removed from the influence of the sun's rays. The manufacture of protoplasm and other complex compounds from inorganic materials involves a considerable amount of energy, and the bacilli must somehow obtain this from the surrounding gases and liquids. Professor Stoney regards it as conceivable that the energy may be imparted to the organisms directly by the impact of the more swiftly moving molecules of these gases and liquids; and if that be the case, then the necessity for the excessive minuteness of the bacilli—scarcely more than molecules—is explained.

OUR knowledge of the more minute structure of the Protozoa has considerably increased of late, as the result of improved methods and apparatus for study; and the protoplasm of an *Amœba*, for example, now appears as something far more than mere jelly and granules. It is now possible even to cut sections of these minute organisms and to stain them in such various ways as to distinguish a most elaborate arrangement of parts in many cases. Last year Dr. F. Schütt published in Berlin the result of some studies of *Peridinia*, in which he not only demonstrated the existence of remarkable spaces in the protoplasm, but a true excretory system in connection with them; and another investigator (Professor Greef) supposes that in sections of *Amœba* he can see striations denoting the arrangement to which the contractility of the animal is due. Mr. J. E. S. Moore is now following up these investigations in the Huxley Research Laboratory at South Kensington, and the last number of the Linnean Society's Journal

(*Zool.*, vol. xxiv., pp. 364–368, pl. xxvii.) contains some of his first results. Mr. Moore has examined sections of *Spirostomum* and *Paramœcium*, and detailed descriptions, with beautiful figures, are given. It would be premature at present to speculate as to the meaning of the various structures, but it is evident that a new line of research of great biological importance has been inaugurated.

Those who are interested in the progress of the American theories of mechanical evolution, ought not to overlook Professor John A. Ryder's papers on "Energy as a Factor in Organic Evolution," and "The Mechanical Genesis of the Form of the Fowl's Egg," in the last number of the *Proc. Amer. Phil. Soc.* (vol. xxxi., pp. 192-209). The arguments are ingenious and interesting, but we are by no means convinced that the phenomena of life are such simple, mechanical matters as Professor Ryder and his school suppose.

M. Marey is still making progress with his study of the locomotion of animals, by means of photography. He has just published figures of the gait of a snake, a gecko, and a scorpion (*Comptes Rendus*, vol. cxvii., pp. 355–359).

A red deer (*Cervus elaphus*) with ten points on each antler has lately been shot by Lord Burton in the forest of Glen Quoich, Scotland. This is apparently the most complex form of antler hitherto obtained in a Scottish specimen.

It has long been known that the Tapirs—which are curiously restricted at the present day to the Malay Archipelago and to Central and South America—once had a very wide range throughout Asia, Europe, and North America. There are traces of these marsh quadrupeds in the Miocene and Pliocene rocks of Germany, France, and England, and in the Pleistocene of China; and quite a long series of ancestral types has been discovered in North America. A brief synopsis of all the known extinct forms has just been contributed to the *Geological Magazine* (Dec. 3, vol. x., pp. 391–396) by Mr. Charles Earle, and all who are acquainted with the tooth-structure and foot-structure of Ungulates will read this account with great interest. So far as known, the Tapirs occur a little earlier in Europe than in America, and the teeth of all the extinct forms are somewhat simpler than those of the species now living.

When will Palæontologists cease to elaborate theories and try to amend long-respected "laws" on the evidence of mere scraps of animals? In a letter we publish elsewhere, Professor Hutton says that the supposed evidence of the occurrence of the New Zealand Moa in Queensland proves to be worthless—that it consists solely in a thigh bone which is much more like that of an Emu or a Cassowary than that of a Moa. "There is thus no longer any reason for supposing a late migration of Struthious birds between New Zealand and Australia." At the same time (*Proc. Linn. Soc. N. S. Wales* [2], vol. viii., pp. 7–10) Mr. De Vis discusses a second Queensland fossil—the upper jaw of Owen's great extinct wombat, *Phascolonus*—and points out how another guess has been exploded. Some have supposed that the huge broad chisel-like teeth named *Sceparnodon* by Owen were the upper incisors of this animal, but Mr. De Vis's discovery now disproves the supposition. *Phascolonus* has very stout upper incisors, and the animal to which the broad chisel-like teeth belong is as mysterious as ever. It is a distinct advantage to have accurate descriptions and figures of fragmentary fossils, and it is often desirable to assign to them provisional names; but literature is already sufficiently burdened, and may well be spared the additional incubus of elaborate guesswork and prosy prophecy.

In reference to general principles touched by this kind of Palæontology, we desire to add a word of warning to students who come across Professor H. G. Seeley's note on a supposed reptilian tooth with bifurcated root in the last number of the *Ann. and Mag. Nat. Hist.* (ser. 6, vol. xii., p. 227). It is a generally accepted principle that the teeth of reptiles are distinguished from those of mammals by having invariably a simple—not bifurcated—root. There is, it is true, not much difference between the longitudinally grooved root of certain reptiles and the double-rooted premolars of typical mammals; but there is no certain exception to the rule as yet on record. Now Professor Seeley describes what he terms an abnormal tooth of *Nuthetes*, an extinct reptile from the Purbeck formation; and if the determination be correct, the discovery is of great interest. We must, however, point out that there is no evidence whatever that the tooth described does belong to *Nuthetes* or to any other reptile; it was found isolated, and in a deposit where mammalian remains occur. Moreover, it appears to us much like the upper canine tooth (with bifurcated root) of the little insectivorous mammal *Triconodon* found at the same place. There is thus so much uncertainty in this determination that the good old law remains unaffected, and we only regret that such comparatively worthless fragments should add further to the literature of palæontological guesses.

In the same paper Professor Seeley incorporates an interesting observation which could not be suspected to occur under the published title, and we therefore extract it. He remarks how the little Purbeck reptiles, *Nuthetes* and *Echinodon*, seem to be dwarfed representatives respectively of the Megalosaurian (carnivorous) and Stegosaurian (herbivorous) Dinosaurs—animals which in other formations are many feet in length. Sir Richard Owen long ago pointed out how almost all the crocodiles from the division of the Purbeck Beds in question were dwarfs, and discussed the curious association of these predaceous creatures with the well-known dwarf mammals of the period. The further recognition of dwarf Dinosaurs existing at the same time is thus of especial interest. All of these are land- or freshwater-animals forming one fauna at one period in one and the same region (present Isle of Purbeck), and it seems very difficult to explain so remarkable an occurrence. The dwarfing of fishes is much more comprehensible from the circumstances of their environment; and it is curious to note that in one Purbeck stratum in Wiltshire all the fossil fishes are diminutive representatives of types that are comparatively large elsewhere.

It is curious to note how widely distributed are some of the great reptiles of the Jurassic period. The marine Ichthyosaurs, for instance, have been found in most parts of the world. Remains of one species were brought home by Sir E. Belcher from the islands between West Cornwall and North Devon in the Arctic Regions; and twenty years ago vertebræ and paddle bones of two other species were discovered by Baron Nordenskiöld in Spitzbergen. Throughout Europe and in North America remains are abundant; and detached vertebræ have been found near Mombasa in East Africa. Other evidence of these saurians has been discovered in Australia, and even in New Zealand as far south as the 45th parallel of south latitude; and now comes the welcome announcement of the occurrence of an Ichthyosaur at two localities in South America (W. Dames, *Zeitschr. deutsch. Geol. Gesell.*, 1893, pp. 23–33, pl. i.). Some dorsal vertebræ with associated fragments from the Tithonian Formation of Cienegita, in the Argentine Republic, are described by Dr. Dames as *Ichthyosaurus bodenbenderi*; and he records the discovery of two vertebræ and a paddle bone of the same genus from the Andes of Chili.

It is remarkable how new fossils are continually being discovered even in the most thoroughly explored strata. The fossil fishes of the Caithness flagstones have been diligently collected for seventy years, and yet it is only during the past summer that a specimen of the typically Lower Devonian genus *Cephalaspis* has been met with in these rocks in the neighbourhood of Thurso. The fossil was described by Dr. R. H. Traquair at the recent meeting of the British Association, and is now in the Edinburgh Museum. A full account of it is promised in a forthcoming volume of the *Proc. Roy. Phys. Soc. Edinburgh.*

It is interesting to note that two species of Ostracoda found in the Bear-River Formation (Upper Cretaceous) of Wyoming, have been identified, by Professor T. Rupert Jones, with forms discovered in this country—the one being *Cypris purbeckensis*, Forbes; the other a variety of *Cypridea tuberculata*, Sow., named *wyomingensis* by Professor Jones (*Geol. Mag.*, Sept., 1893).

A. Hollick claims to have discovered a fossil palm-leaf in the Cretaceous formation at Glen Cove, Long Island, a discovery of some significance, as "other fossil leaves associated with it show the geological horizon to be the equivalent of the Amboy Clays of New Jersey and other middle Cretaceous strata in America and Europe, from none of which have palms been definitely recorded." The leaf, which is small, fan-shaped, and about three inches across, is made to rank as a new genus, *Serenopsis*, the name indicating its supposed ancestral relation to the Southern Palmetto (*Serenæa*).

The genus was established in the April number of the Bulletin of the Torrey Botanical Club, and the August number of the same journal contains some further information obtained from a second specimen recently found, and in a better state of preservation. It may be well to mention that Professor Lester Ward, whose opinion on the specimens was asked, does not think it a palm, "but suggests that it is more likely to be allied to the organisms which have been called *Williamsonia*, whose affinities are exceedingly problematic."

It has long been uncertain whether the typical Carboniferous plant, *Lepidodendron*, occurs so low as the Devonian rocks in Australia. Messrs. Pittman and David now seem to have settled the question by its discovery on Mt. Lambie, New South Wales (*Proc. Linn. Soc. N.S. Wales* [2], vol. viii., p. 121).

The idea that there were high alps with glaciers in the neighbourhood of central France when the Coal-measures of that region were deposited, finds favour with another geologist who has been studying the curious breccias and barren strata in the series (A. Julien, *Comptes Rendus*, vol. cxvii., 1893, pp. 344–346). The result is very interesting, and the conclusions seem far from improbable; but we are pleased to observe that Mr. Mark Stirrup, in his latest paper on the boulders from the Lancashire Coal-measures, does not attempt to infer the occurrence of glacial conditions from the comparatively insignificant phenomena in his region (*Trans. Manchester Geol. Soc.*, vol. xxii., 1893, pp. 321–331). Mr. Stirrup confines himself to the careful observation of the facts of the case, and Professor Bonney has minutely examined many of the pebbles from the petrologist's point of view. Besides the well-known quartzites, Mr. Stirrup has now discovered examples of granite, gneiss, and probably felsite, but all these rocks are seen to be much decomposed when examined microscopically. Professor Bonney thinks they could all probably be "matched" in Scotland, though nothing definite can be stated as to their derivation. It is now evident that extraneous pebbles occur in the coal-seams and associated shales in many coal-fields both in Europe and America.

It seems to be proved that the Northern European sea of the Eocene period was separated from that of the Mediterranean area by a land barrier extending across France and Northern Germany. Hence it has been noticed that the fossil marine organisms to the south bear witness to highly favourable conditions for growth, while those in the north seem to have been stunted by the chilling influences of cold currents. This phenomenon is still further exemplified by a recent study of the British Eocene Bryozoa (J. W. Gregory, *Trans. Zool. Soc.*, vol. xiii., pp. 219–279, pls. xxix.–xxxii.), which are numerically small, both in species and individuals, compared with the wealth of forms that inhabited the contemporary seas of the Mediterranean basin. The northern species are also remarkably dwarfed. The land barrier seems to have been destroyed in middle Eocene times, but the conditions were not seriously modified until later. Incidentally, Dr. Gregory remarks that the species of Bryozoa are not so long-lived as some palæontologists suppose; he has no faith in the identification of Cretaceous species with any of those of existing seas.

The "parallel roads" of Glen Roy are still inspiring those who have the opportunity of studying glacial lakes, and Dr. Robert Munro last year kept the much-discussed Scottish phenomenon in mind while visiting Norway. He found a remarkable example of a glacier lake, formed by a branch of the Hardanger-Jökul, near Eidfiörd, and has now communicated a valuable detailed account of it to the Royal Society of Edinburgh. The paper appears in the last part of the Society's *Proceedings* (vol. xx., pp. 53–62), accompanied by a map. Dr. Munro concludes that the evidence of the existence of glacier lakes furnished by the so-called "parallel roads" of Glen Roy, corresponds, in its minutest details, with the facts observed in his study of the recent lake in Norway.

Various hypotheses have been started with reference to the Dartmoor Granite, but the old view of De la Beche, that it was intruded among the Devonian rocks and Culm-measures, into which it sent veins, is supported in a recent paper by Lieut.-General C. A. McMahon (*Quart. Journ. Geol. Soc.*, vol. xlix., pp. 385–395). Such veins have been observed by the author near Lydford, on the western flank of Dartmoor. He also drew attention to the pseudo-bedding of the granite, remarking that this conforms closely to the present slope of the surface of the hills. In his opinion it may be attributed to the action of the sun's heat and frosts, in rupturing the superficial portions of the granite, and thereby producing the pseudo-bedding and joints.

A new Cornish mineral has been determined by Mr. H. A. Miers (*Nature*, August 31). It is a hydrated sulphate and chloride of copper and aluminium, and it occurs in brilliant and translucent crystals of a deep emerald-green colour. In external characters and composition it is identical with Spangolite, of which only one example was previously known, and that was described in America, and obtained from a man living in Arizona. Mr. Miers hopes that search will be made among old collections, and especially upon copper ores from the St. Day Mine, near Redruth, for this is the locality whence this new specimen was obtained.

The report of the Director-General of the Geological Survey for the year 1892 occupies twenty-nine pages in the last Report of the Science and Art Department, and it presents a much fuller statement of the scientific work of the staff than has hitherto been made. Many new facts are thus for the first time announced, so that the volume is one to be noticed by compilers of geological records. There are fresh facts in reference to enormous boulders in the Drift, and on the occurrence of Eskers or Kames in the West of England. There are notes on the Hampshire Tertiary strata, on the Chalk and other Cretaceous rocks of England, and on the Jurassic rocks of England and Scotland. There is the latest official classification of the Devonian rocks, and many remarks on the volcanic and metamorphic rocks of Britain generally. Folds and thrust-planes are noticed, not only in the Highlands, but in the regions of Purbeck and Weymouth.

The Report of the Royal Commission on London Water Supply has been presented to the House of Commons. The Commissioners are strongly of opinion that the water, as supplied to the consumer in London, is of a very high standard of excellence and of purity, and that it is suitable in quality for all household purposes. With respect to the quantity of water which can be obtained within the watersheds of the Thames and Lea, they are of opinion that a sufficient supply to meet the wants of the metropolis, for a long time to come, may be found. From the River Lea, with adequate additions to the present storage, 52½ million gallons may be taken daily; and by the construction of storage-reservoirs in the Thames Valley, at no great distance above the intakes of the companies, it will be possible to obtain an average daily supply of 300 million gallons, without taking in any objectionable part of the flood-water. At present not half that amount is taken, but the scheme suggested can be carried out progressively to meet the increasing demands for water. A large supply of water might also be procured from the Chalk area in the basin of the Medway. In the opinion of the Commissioners a sufficient quantity of water to supply 35 gallons a head to a population of 12 millions could be obtained; and the population of Greater London is not likely to exceed that amount during the next 50 years. The Report will therefore be read with satisfaction by those interested in London Water Supply; it is signed by Lord Balfour of Burleigh (chairman), Sir George Bruce, Sir Archibald Geikie, Professor James Dewar, Mr. George H. Hill, Mr. James Mansergh, and Dr. W. Ogle.

We learn from the *Zanzibar Gazette* and recent letters that in his recent ascent of Mt. Kenia, Dr. J. W. Gregory was stopped at a point 2,000 feet below the summit by a cornice of snow, which could not be surmounted except by a properly-equipped party of climbers. He established a baggage camp at the foot of the mountain, a reserve camp at 11,000 feet, and an upper camp close to the snow-line. The greatest difficulty was experienced in passing through the zone of bamboos, where advantage had to be taken of elephant paths. The succeeding forests proved very dense, damp, and cold, and it was a relief to emerge into the higher pastures. Dr. Gregory collected plants and insects, examined some large glaciers and determined their former much greater extension, and mapped the S.W. side of the mountain.

We regret to hear that during his exploration of the now active volcano, Adjuma-yama, near Fukushima, in Japan, a member of the Japanese Geological Survey lost his life, and that of his assistant, owing to a sudden explosion. Mr. S. Miura, the deceased geologist, was formerly science master in the Normal School at Saga in S. Japan, and had only recently joined the Survey. The mountain can now be visited with safety, as the explosions occur at regular intervals, morning and evening.

One could scarcely accuse a writer so well-informed as Mr. Joseph Hatton of carelessness but that under the heading "Cigarette Papers" in the *People* of September 3 appeared an extraordinarily absurd account of a gigantic squid. This is referred to as an "up-to-date fish of the cephaloptera species," with "long scaly arms," or as the writer puts it in another paragraph, "forty-foot antennæ." The remarks are made with regard to an account of the appearance of one of these gigantic molluscs to some sailors in the Gulf of Mexico, and whether or not the nonsense occurs in the American newspaper account, a reference to any encyclopædia would have enabled the English writer to avoid repeating so strange a tissue of absurdities.

The Anthropological Institute issued in August an "Index to the Publications of the Anthropological Institute of Great Britain and Ireland (1843-1891), including the Journal and Transactions of the Ethnological Society of London (1843-1891): the Journal and Memoirs of the Anthropological Society of London (1863-1871): and the Anthropological Review." The volume consists of 302 pp., and is priced at ten shillings. In the main one alphabetical arrangement is followed, but why this is not carried out under the headings "Exhibitions," "Reviews of Books," etc., passes our comprehension. These items are apparently arranged in the order of publication, and the inconvenience of having to wade through 16 pages of index when hunting up the review of a book is obvious. The publication is extremely valuable, and is an example to several other London societies (*e.g.*, Geological and Geographical), whose publications have in part been indexed by Americans. The neglect to provide a general index for journals like the *Annals and Magazine of Natural History* and the *Geological Magazine* too, which has been running now since 1864, speaks little for the enterprise of publishers. In the last instance, we are informed, the work of preparation has been offered gratuitously, but the outlay on printing has been refused. The consequence is that many valuable papers are overlooked or forgotten, as a serious expenditure of time is entailed in searching through the indexes of thirty or forty volumes. They manage these things better abroad, for such publications as the *American Journal of Science* and the *Neues Jahrbuch für Mineralogie* are provided with general indexes at intervals.

An excellent portrait of the late Mr. James William Davis appears in the current number of the *Geological Magazine*.

The sixtieth anniversary of the birthday of Baron F. von Richthofen has been celebrated by the issue of a "Festschrift," a volume of 418 pages, illustrated with a portrait and maps. It consists of a series of essays, chiefly on geographical subjects, by the Baron's pupils. The seventieth anniversary of the birthday of Professor J. Victor Carus was marked by the issue of a beautiful portrait with the *Zoologischer Anzeiger* for August 14, 1893.

The *Mediterranean Naturalist* is now issued only at intervals of two months. The August number contains, among other matter, a plea for the establishment of a Museum of Natural History and Archæology for Malta.

We are always glad to note the success of any scheme that has for its object the infusion of a scientific spirit into collectors. It is thus a matter for gratification to observe that the small "monthly medium for collectors and students of Natural History" known as *The Naturalists' Journal* has just been enlarged. One enthusiast in the "exchange column" wants some Natural History books or specimens in exchange for a pile of literature of the "railway accident insurance" type. This looks like a conversion.

I.

The Effect of the Glacial Period on the Fauna and Flora of the British Isles.

THE question of the extent to which the flora and fauna of Britain was driven out of the country, or exterminated during the Glacial period is intimately connected with that of the southern limit of the ice. Geologists in general seem to favour the view that plants and animals were for the most part—if not entirely—expelled, and that our country had to be restocked from the continent. But there are several considerations which point to the possibility of the survival of some part at least of the pre-Glacial inhabitants of the land.

The ice during the Glacial period may have extended as far south as the latitude of London; very possibly it did not extend so far. Our opinion as to the survival or otherwise of our flora and fauna will be influenced by our view as to the southern extension of the ice. Let us take the former view first. This would leave the counties of Kent, Surrey, Sussex, Hants, Dorset, Somerset, Devon, Cornwall, and part of Wiltshire free from ice.

As far as *space*, then, is concerned, we have an area capable of affording an asylum to a considerable number of our plants and animals.

If, however, the Boulder Clay was not formed beneath the ice, then the latter probably did not extend so far south as the latitude of London, and the area fitted to form an asylum for our pre-Glacial flora and fauna increased.

As regards climate, again, it is to be remembered that ice-sheets and glaciers terminate in temperate latitudes when they do not reach the sea—if the latitudes were not temperate they would not terminate; and in connection with existing glaciers we find a temperate flora and fauna in close proximity to the ice, as in Switzerland, the Himalaya, and America. Further, the limits of the ice overlap the limits of temperate life; the latter ascends beyond the termination of the ice, while the former extends into the domains of the latter. Note, for example, how the lines marking the northern limit of forests and the southern limit of the ice overlap in North America.

Such facts lead us to admit the probability that the climate of the south of England during the Glacial period was such as to permit the continued existence of temperate life and also to enlarge our conception of the area free from ice.

There are other considerations which strengthen this view. The southern limit of the ice, when found, marks its extreme winter extension; during summer a very much larger area of country would be free from ice; for it is to be remembered that, whatever theory of glaciation we accept—since those involving an absolute diminution of solar heat are almost universally discarded—we have to reckon with a summer heat-supply as great as that which we receive to-day. Consider, then, what would be the effect of setting our present summer to work on an England in which the ice reached as far south as London. We may surely assume the melting back of the ice for a considerable distance.

During the most intense Glacial period possible, according to the astronomical theory, our present heat-supply was concentrated into 166 days, but this would probably affect very little its melting powers; and according to Sir Robert Ball's data, the average temperature of the Glacial summer would be 42° F. higher than at present, or about 102° F. Such a considerable retreat of the ice during summer would, of course, tend to make temperate life more possible in the country.

Again, there is evidence which appears to show that the Gulf Stream warmed our western and southern shores as it does to-day.

(1) It has been noted that more southern forms of shells occur in the Drift of the west of England and of Ireland than in that of the east of England.

(2) Glaciation extended further south in North America than in Western Europe.

Therefore, as at the present day there are places considerably north of London with milder climates, so during glaciation there may have been areas fitted to preserve temperate forms of life besides the southern counties before mentioned.

But do the fossils of our Glacial deposits favour the view of the survival of any pre-Glacial plants and animals in our country?

Intercalated with the Boulder Clay are certain sands and gravels containing occasionally organic remains, which those who accept the view of interglacial warm periods claim as evidence of such.

If, as I have endeavoured to show,[1] the evidence for such periods is inconclusive, these organic remains may indicate survival; and it is well-known that such mingling of arctic and southern forms does not necessarily indicate alternations of warm and cold periods. Professor Forbes pointed out an interesting example in his "Fauna and Flora of the British Islands." Cape Cod, in North America, he remarked, forms the line of demarcation between a fauna as northern as that of England during the Glacial period, and another of an aspect perhaps even more southern than that existing at present

[1] *Geological Magazine*, August and September, 1891.

on the coasts of Portugal; and he adds: "For a short space, and but a very short space, the two faunas intermingle."[2]

Take, again, certain parts of the Atlantic Ocean at the present day. A mingling of arctic with temperate forms of life must occur among the deposits there accumulating by means of currents flowing south from colder regions, and the Gulf Stream flowing north from temperate regions.

A similar mixture must occur in the seas north of Siberia, since the great rivers flowing from the far South will carry temperate forms of life to be interbedded with the natives of those icy seas.

An interesting example of the mingling of temperate and arctic forms is recorded from the New Siberian Islands. Here in one deposit were found bones of a long-haired variety of the tiger, along with those of the musk ox, mammoth, etc.[3]

Leaving, however, the doubtful evidence of the so-called "inter-glacial" beds, are there any indications of survival in undoubted Glacial deposits?

In the Glacial fresh-water beds which occur at the base of the Glacial series in Norfolk, *Salix polaris* and *Betula nana* occur along with *Planorbis complanatus*, *Succinea putris*, and *S. oblonga*. The first of these species of mollusca occurs everywhere in England, Wales, and Ireland; the second is fairly common, and the third rare and local *at the present day*. Thus the cold which brought *Salix polaris* did not drive away these temperate forms of mollusca.

In the Boulder Clay of Norfolk, again, the following species are recorded:—*Cardium edule*, *Tellina lata*,* *Cyprina islandica*, *Littorina littorea*, *Mya arenaria*, *Pleurotoma turricula*,* *Pholas crispata*. With the exception of those marked thus *, all occur in the succeeding Glacial gravel; they all, without exception, occur in the preceding Pliocene, while only *Tellina lata* is extinct as a British shell.

Cardium edule is now common round our coasts; *Cyprina islandica* ranges as far south as Boulonnais and Cherbourg; *Mya arenaria* occurring in South Greenland, ranges as far south as Rochelle; *Pholas crispata* extends to the North of France; *Littorina littorea* ranges between Greenland and Lisbon; *Pleurotoma turricula* occurs on every part of the British coasts.

As regards Mammalia, *Elephas primigenius* is recorded from the Boulder Clay of the same district, and doubtfully from the Glacial gravel; *Cervus elaphus* is doubtfully recorded from the Boulder Clay.

When we turn to the West Coast of England we find in the organic remains of the Boulder Clay still stronger indications of the possibility of survival. Thus the great majority of shells found in the Boulder Clay of Liverpool are such as are living in British seas to-day; only two, *Astarte borealis* and *Saxicava rugosa*, are distinctly

[2] *Mem. Geol. Surv.*, vol. i., p. 377.

[3] *Mém. Acad. Imp. Sci. St. Pétersbourg*, vol. xl., no. 1. Referred to in "Notes and Comments," NATURAL SCIENCE, March, 1893, p. 170.

arctic, while others—*Venus chione*, *Cardium tuberculatum*, and *Dentalium tarentinum*—are generally restricted to more southern regions.

As far, then, as these particular cases can be taken as typical, we seem to have indications that certain temperate forms of life were able to survive glaciation.

The present distribution of animals and plants in the British Islands has an interesting bearing on the question.

That England only possesses a portion, and Ireland a still smaller portion, of the animals and plants of the Continent, is held to indicate the gradual severance of the land connection by which these countries were restocked after glaciation. Ireland is supposed to have been severed from England while the latter was still connected with France and Belgium. It may possibly be, however, that the evidence of these facts of distribution is rather that England was not restocked by a land connection from the Continent after glaciation. If it can be shown that the missing forms are those most likely to have been exterminated by the cold, or least likely to cross the separating sea, and if, in addition, they are forms calculated to migrate as quickly as those which are common to our country, then there will be an argument against the restocking by a land connection.

The argument of the fifty absent species of mammalia is against the hypothesis of such a land connection, unless it can be shown that there was some other hindrance than lack of time; for the supposition that they had not time will not pass muster: such geographical phases as that implied by the union of the East Coast of England to the Continent by a great plain are usually of vast duration.

Supposing the connection to have lasted 10,000 years,[4] then, to accomplish the journey, they would only need to travel 15, 14, or 3 yards a year, according as they crossed from the Netherlands to Norfolk, from Belgium to Kent, or by the Straits of Dover. It is difficult to think of any species of mammal which, under favourable climatic and other conditions, would not be able to spread more than from 3 to 15 yards per annum. Individuals of most of the species might cross in less than a year. Nor is it easy to believe that any species of reptile would be unable to spread at a much quicker rate than 15 yards a year; and it is to be noted that if the process by which England was united to the Continent was a very gradual one—as such geographical changes usually are—then many species would be half-way across the dividing area by the time the connection was completely established.

With the plants the case is somewhat different, for they are not able to migrate as individuals like animals. Yet when we consider the numerous means at their disposal—rivers running from the Continent towards the central plain, seeds, like those of the dandelion

[4] According to Mr Jukes-Browne, "it lasted long enough for Palæolithic man to be supplanted by Neolithic man, and for a large number of mammalia to become extinct."—"The Building of the British Islands," p. 297.

and hawkweeds, wafted by the wind, others, like goose grass, burdock, and common Avens, adhering to the fur and wool of animals, others carried by frugivorous birds, or adhering to the beaks and feet of others, etc.—it would be strange if most of them did not migrate more than 15 yards a year.

There is another consideration. The plants which must have crossed the plain on the hypothesis of a restocking from the Continent after the Glacial epoch, include apparently some of the most slowly spreading forms.

While, then, the above considerations seem to indicate that our present flora and fauna did not necessarily migrate across an eastern plain after the Glacial period, they need not be taken as an argument against the existence of such a connection, if that is supported by other facts. There *may* have been such a connection, and yet our plants and animals may have existed pretty nearly as they are now before its existence. If it can be shown, on the one hand, that the absent species are such as ought to have migrated across a land connection, then the present distribution favours the view that our country was not restocked from the Continent after the Glacial period; if, on the other hand, they are species whose line of geographical distribution did not extend so far west as our east coast, or so far north as our south coast, then their absence is no argument either way; and if our country was not restocked from the Continent in post-Glacial times, then the pre-Glacial flora and fauna must have survived to a large extent.

The existence of several species of South European plants in the south-west of Ireland which are absent throughout the rest of Britain also furnishes an argument in favour of survival; for in the absence of a direct land connection between Ireland and the north of Spain since glaciation—for which there is no independent evidence—it is difficult to understand how they can have got there. It seems on the whole a more reasonable supposition that in the warm period preceding the Glacial they were widely dispersed over Britain, and that they were exterminated everywhere, except in the warmest corner, viz., the extreme south-west of Ireland, by the cold. Such was the opinion expressed by Professor Forbes.[5] At the same time the possibility of a post-Glacial migration by other means than a land connection—as, for example, by the carriage of seeds by migratory birds—must not be lost sight of.

A recent discovery which seems to hint that a part of our flora, at least, is a survival, is that of the seeds of *Naias marina* in the Cromer Forest Bed. The only British locality for this plant is Hickling Broad, Norfolk, and hitherto the Forest Bed is the only fossil locality. It would be, to say the least, a curious coincidence if it had been exterminated by the ice and had then re-migrated from the Continent

[5] *Mem. Geol. Survey*, vol. i.

to one spot in Norfolk only. The species is at present a native of temperate and tropical regions of the Old World.

A further indication of the possibility of survival is afforded by the flora of Cornwall and Devon. Like the Iberian element in the Irish flora, the Norman element in that of Cornwall and Devon may have existed there before the Glacial epoch. At least, the precarious foothold of many of the species, and the apparent dying out of some, are not suggestive of species migrating northwards in response to a continually ameliorating climate.

In a paper on "The Climate of Europe during the Glacial Epoch,"[6] Mr. Clement Reid discusses the question of the temperature of Britain during the Glacial Period. Some of his results seem to have an important bearing on the present enquiry. If, as he concludes, the temperature increased *rapidly* towards the south, there would be a greater possibility of survival south of the limits of glaciation. At the same time, a southern England bounded by an ice-foot similar to that of the Arctic regions, as pictured in the same article, does not appear a favourable shelter for any but somewhat hardy forms of plant and animal life. Mr. Reid also arrives at the conclusion that "extensive regions must have been quite uninhabitable for their present fauna and flora." Whether, however, this remark is to be applied to the South of England or not does not appear.

The solution of the problem which forms the subject of this article seems to me an important one, since almost the only evidence of a post-Glacial connection with the Continent is the *supposed* necessity of such to account for our present fauna and flora. To those geologists, indeed, who, on the strength of the evidence of a submerged forest, infer an elevation of the country *en masse*, such a connection—even repeated many times in accordance with the view of many Glacial and inter-Glacial periods—is no difficulty at all; they are as prodigal in respect of earth-movements as those naturalists who would "create a continent to account for the migration of a beetle." To those, however, who shrink from such assumptions, unless supported by strong evidence, and who feel that such oscillations of the land *en masse* are inconsistent with the physics of the earth's crust, the conclusion here indicated may perhaps be a welcome solution of a difficulty.

G. W. Bulman.

[6] Natural Science, vol. ii., August, 1892, p. 427.

II.

Some Recent Researches on the Habits of Ants, Wasps, and Bees.

BY most zoologists, the aculeate or sting-bearing Hymenoptera are accorded the highest place among insects; a position warranted not only by the extreme specialisation of their structure and development, but also by the perfection of their family and social instincts, which, from ancient times, have been held up for the admiration and imitation of mankind. Those families of the group in which the social life has been most completely adopted—the ants, and the social wasps and bees—are probably better known to persons not zoologists than are any other insects. Interest in their habits and economy has been greatly aroused by Sir J. Lubbock's well-known work (1), though the leading facts of these insect-commonwealths were long ago carefully observed by Huber.

The orderly and purposeful collective work of ants has often led observers to speculate upon the means of communication between the insects, and they have been believed to converse with each other by motions and contact of their antennæ. Experiments on this subject must be familiar to readers of Lubbock's book. He also investigated the ability of ants to make sounds and to hear. Although supposed organs of hearing have been found in the antennæ and tibiæ of ants, no impression could be produced upon the insects by any sound audible to human ears. Lubbock supposed, therefore, that they can only appreciate notes of a pitch too high to be heard by us. He suggested that certain ribbed areas on the abdominal segments of various species might be organs for producing sound, but he was unable to detect any sounds which these may have made.

These sound-producing areas have recently been specially studied by Dr. Sharp (2). He finds that some of the structures observed by Sir J. Lubbock and others are nothing but the ordinary sculpture of the surface of the segments. In other cases, however, he has no doubt that stridulating organs really exist. At the middle of the base of the dorsal surface of the third abdominal segment, in most species of the Myrmicides and Ponerides examined, he finds a special area traversed by excessively fine lines. In the Ponerides these are modified from the ordinary sculpture of the segment which, on these spots, becomes finer, closer, and more regular. Indeed, in comparison with the rest of the surface, the stridulating area looks smooth, and a high power and appropriate light are required to make the lines apparent. In the Myrmicides, the lines appear to be not always mere modifications of the general sculpture; in some cases they are described as being "developed on a glassy surface poured out on the ordinary surface." In a species of *Sima* from Australia, the area was found to be divided into two parts, the lines on the one part being much coarser than those on the other, and doubtless enabling the insect to modify the sound produced.

The delicate instrument formed by the fine sculpture is played upon by the specially modified hinder margin of the segment in front, which projects downwards to form a very sharp and even edge. This, drawn across the fine ridges, as the ant moves her abdominal segments, must give rise to an excessively high note. In some cases, at least, the sound is audible to human ears. By moving the appropriate parts, Dr. Sharp has produced sounds with species of leaf-cutting ants (*Atta*). Mr. Wroughton (3) has described the action of a colony of Indian ants (*Crematogaster*) when disturbed; they wag their abdomens, and emit a sound "as if a red hot cinder had been plunged into water." The Camponotides, to which most of our common British species of ants belong, do not appear to possess stridulating organs.

Leaf-cutting ants are familiar to readers of books of tropical travel, and the object of the insects in collecting leaves has been differently explained by various observers. The opinion advocated by Belt, that the ants tear up the leaves in order to make beds on which a crop of fungi may be grown, was confirmed by Mr. Cook, and has recently received more detailed confirmation from the researches of Herr Möller (4), who has studied the habits of these ants in Brazil. Within their nest is a soft, spongy mass, consisting of the remains of leaves, cut into excessively minute fragments and gathered into small heaps to serve as "mushroom gardens," in which a fungus (*Rozites gongylophora*) is cultivated to furnish food for the colony. Four species of ants (genus *Atta*) are found to grow the same fungus. In all cases a small space is left between the "mushroom garden" and the outer wall of the nest, and the beds are never formed in an exposed position. The material collected by the ants naturally contains spores of other fungi than their special kind, and in cultivations prepared by Möller these appeared and developed. In the nests, however, but the one kind is allowed to grow; the ants appear to weed out all but their special food. Over the upper surface of the "garden" are numerous small white bodies, formed by masses of swollen ends of fungus hyphæ, which the ants appear to produce by some special culture. These masses are what the ants feed upon, they never appear to eat the fragments of leaves, which form the fungus-beds. When they take a journey they carry with them the material of the "garden," even to the smallest fragments, and rebuild the structure in their new abode.

Herr Möller finds that ants of the genera *Apterostigma* and *Cyphomyrmex* are also "mushroom gardeners," but these do not cut leaves to form their fungus-beds. They use instead, wood, grain, or dung. Four species of ants of the former genus cultivate one fungus, and two species of the latter another. Although these fungi all belong to the same group, the ants, even when hungry, are found to refuse disdainfully the food of another genus. Even the species of ants of the same genus, which cultivate the same species of fungus, produce by different methods of culture a difference in the nature of their special food bodies. Those grown by the species of *Atta* are adjudged the prize by their patient observer in the fungicultural contest.

Observations by Herr Möller on another genus of ants (*Acanthognathus*) have been recently recorded by Professor Forel (5). Besides teeth at the extremity of their mandibles, the workers possess, at the base of each of those appendages, a long, curved tooth, directed inwards and downwards. When at work building, or carrying eggs, these ants have their mandibles widely apart, sticking out on either side at right angles to the body. When in this position, the ends of the basal curved teeth just touch each other, and are used by the insect to carry the egg, or a piece of earth for building. When frightened, the ant brings her mandibles sharply together, and the basal teeth are then, of course, crossed.

A most interesting summary of what is known of the various kinds of ants' nests has, also, lately been published by Professor Forel (6), in which he gives the result of observations by himself and other naturalists. Nests made in holes or under stones, nests dug in or built of earth, wood, etc., are described and illustrated from the work of European and exotic ants. The transition between nests built of a papery substance formed of fragments of earth, wood, etc., joined by a secreted cement, to those composed of fine threads drawn out from such cement, and used for binding leaves together, is traced through a succession of species.

Lubbock, in the work already referred to, remarked that it was strange that the exact manner in which new colonies of ants are founded should remain uncertain. He suggested that a young queen, after the nuptial flight, might join an old nest, or found a new colony, either by herself, or with the assistance of a number of workers. Professor Forel, in the paper just mentioned, considers it established that new colonies are founded by solitary females, or by several associated together. Mr. W. W. Smith (7) has lately studied the rise of ant-colonies in New Zealand. The nuptial flight there takes place in March, and the rise of societies was studied through the winter months following. A pair (male and female) or several pairs were found to be associated to form the new colonies; and these always selected sites, under stones or elsewhere, already occupied by aphids and coccids—the "domestic animals" of the ants. If, through any cause, the aphids left the place, the ants went away too.

To modern naturalists the stages by which the more perfectly social insect communities have arisen furnish a question of great interest. Some facts and suggestions towards the solution of this problem have recently been furnished by Herr Verhoeff (8). He points out that the economy of each social group must be compared with that of the "solitary" genera most nearly akin to its presumed ancestral stock. The affinities of the ants are considered by Verhoeff too doubtful for profitable consideration: he therefore confines his attention to the wasps and bees. The social wasps (Vespidæ) are believed to have arisen from the Eumenidæ, and these from a group of fossorial hymenoptera nearly allied to the Trypoxilidæ. The bees are supposed to have originated from some other fossorial family. Hence the study of the fossorial genera may be expected to throw much light upon the economy of the higher groups. From simply laying the egg in the body of some other insect—after the manner of modern ichneumon flies—was developed the present habit of the fossorial wasps of digging a hole, in which are placed both the egg, and the insect which serves as a prey for the grub when hatched. The habit of catching the prey, and then digging a hole to bury it in, must clearly have preceded that of first preparing the hole and then catching the prey to put in it. As in primitive man, the hunting habit was at first stronger than the home-making. But the simple digging of a hole, once adopted, led on by degrees to the construction of nests of a more and more complicated design. The simple unicellular nest was improved into the linear arrangement of cells, with a common opening, constructed either in the earth or in twigs which may now be observed in species of *Crabro*, *Hoplopus*, *Tropoxylon*, *Colletes*, *Osmia*, *Anthophora*, etc. From this latter, or from the simple cell, is derived the branched style of nest in which the cells open off from the entrance or from a passage leading thereto; such nests are made by many of the solitary wasps and bees, species of *Hoplopus*, *Halictus*, etc. In *Rhopalum clavipes*, which, making its nest in twigs, constructs some of the cells at a very slight angle with the main series, we see how the branched system could arise from the linear. A further advance is seen in the nests not placed in cavities but built of lime or sand upon rocks or walls. The highest forms of nests are the paper structures of the social wasps, and the waxen cells of the bees.

Herr Verhoeff does not believe that the social communities of the wasps and bees have arisen from casual colonies, such as are occasionally observed among solitary species; the bond of the community is to be found in the members being all the offspring of one mother. As in human society, the family has given rise to the state. For the development of a social community three conditions are necessary:—A nest large enough for a number of insects, a close grouping of the cells, and an association between the mother and her offspring in the perfect state (not simply as larvæ); the last condition will be brought about by the emergence of the older insects of the brood while the mother is still occupied with the younger larvæ or their cells. In a single species of solitary bee (*Halictus quadristrigatus*) these conditions are almost fulfilled, but the earliest young insects to appear are males, and when the females are developed the mother dies. For the formation of a community, of course, the mother must co-operate with her female offspring, and this *Halictus*, therefore, just fails to develop the social habit. It gives, however, a sufficiently strong clue to the origin of that habit.

The difference in habit between bees and wasps gives rise to another interesting suggestion by Herr Verhoeff. The bees, being vegetable-feeders, are more disposed to live peaceably in communities than the wasps. But there is no contact between the mother and larva; a supply of honey and pollen is placed in the cell, the egg laid, and the cell closed up. The wasps, on the other hand, are mostly insect-feeders, and might be expected, therefore, to be of too warlike a disposition to form well-ordered republics. But their mode of life is such as to develop family affection; the mother, after laying the egg, goes in search of prey, and from the fossorial habit of placing alongside the egg a paralysed insect to serve as food for the larva, and then closing the cell, has arisen the custom of feeding the larva throughout the preparatory stage, and only closing the cell during the pupal period.

The nests and habits of many species are described by Herr Verhoeff in detail. Some notes on the wintering of Hymenoptera are of special interest. The winter is passed by various species in all stages, except the egg and the feeding-larva. Curiously enough, insects of the social species are generally found singly in the winter, while individuals of the solitary species gather together in the cold season, as do also some ichneumon flies. A number of males and females of a species of *Ceratina* were found in a bramble stem which they had hollowed out to serve as winter quarters. A winter colony of females of *Halictus morio* (the males of this species die in autumn) was found in a forked tunnel opening under a large stone. In was in early spring that these insects were observed, and, while a number of the bees were gathered together at the end of one branch of their burrow, others were apparently making their way out of the winter retreat.

The habits of some bees (*Trigona*) at Trinidad have been lately described by Mr. Hart (9). These insects have no sting, and consequently adopt special means for defending their nest. The only entrance is by means of a tube which is constricted at intervals so that only one bee can pass. Each of these constrictions would serve as a fortification where a foe might be held at bay. Moreover, at night the bees entirely close up the tube with a sheet of wax, leaving only a few minute openings for ventilation.

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Geo. H. Carpenter.

III.

The Recent Plague of Wasps.

THE remarkable summer just past has been signalised by such a profusion of what is perhaps the most generally unpopular insect pest, that a brief review of the facts may be of interest. Of the seven British species of *Vespa*, no fewer than five appear to have been more than usually abundant. So early as the beginning of June both personal observation and reports from reliable witnesses showed *V. sylvestris* and *V. novegica* to be well established and in possession of strong nests. During the earlier portion of the summer these two tree-wasps were, in S.W. Surrey, far more abundant than the ground-wasps, *V. vulgaris* and *V. germanica*, which, however, were in full force by the middle of July, while *V. rufa* was found more commonly than in previous years, though it could not be said to be numerous, at least from my own information. It is much to be regretted that the letters and complaints in the daily newspapers contain no hint as to the species of wasps abounding in various parts of the country, though the context would in most cases point to ground-wasps. Nevertheless, Mr. Lowe (*Nature*, vol. 48, p. 437) speaks of "the tree-wasp" having had many nests in Monmouthshire, so that we may reasonably conclude that the season has been favourable to tree- and ground-wasps alike throughout England. Of the somewhat mysterious *V. arborea* I have heard nothing, excepting the capture of two females in Ireland (*Entom. Monthly Mag.*, July, 1893, p. 166). It is very singular the hornet, *V. crabro*, has hardly been reported abundant anywhere. I cannot help thinking that this species is decidedly on the wane and disappearing from our islands. Popular observation on the point is well-nigh worthless, for many so called "hornets" prove on inspection to be large female wasps. It would be of great interest to ascertain from all counties whether the "red hornet" has or has not become appreciably scarcer during the last fifteen or twenty years.

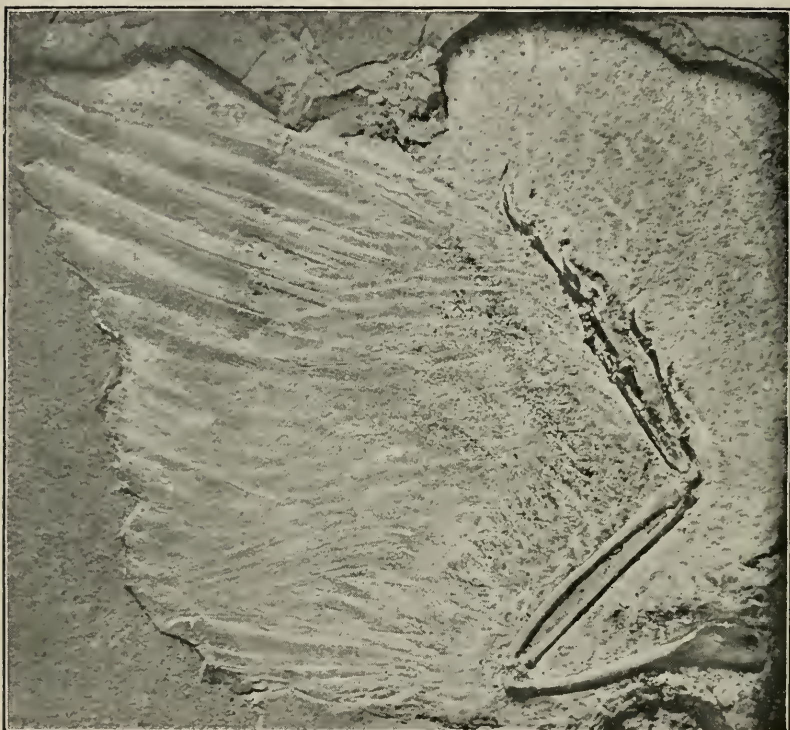
Turning now to the causes of this "plague," I have no hesitation in ascribing it almost entirely to a remarkable series of meteorological conditions advantageous to wasp life. Speaking of observations made by myself, which in the main agree with those taken elsewhere, it is noticeable that after March 23 there was no frost registered by the screened thermometer, excepting slight ones on the nights of April 13, 14, 15. Again, with regard to rainfall, from March 1 to June 30 the total amount registered was only 2·38 inches on 27 days, and on no one day throughout the four months did the fall exceed 0·34 inch.

It is not easy to conceive weather better suited than the above to the well-being of wasps. The impregnated females emerge from winter quarters early in the year, generally making their first appearance during March. The occurrence of frost, snow, heavy rains, and the like during April or even May, cannot fail to destroy enormous numbers of females and their first few grubs lying in the small nests built of delicate "wasp-paper" by the queens themselves. Severe weather setting in after the commencement of the nests must be fatal to tree- and ground-wasps alike. This year after the severe weather of early spring had once gone it never returned. Hence the females, tempted from hibernation by warmer days, met with no disaster either in their persons or their habitations, and thus an unusually large number of nests became successfully established. Further than this, the mean temperature for the summer months has been decidedly above the average, and many animals and flowering plants made their appearance several weeks sooner than is customary, thus affording a plentiful supply of food material to the omnivorous worker wasps. The drought appeared to affect the wasps in two ways, advantageously by causing great and rapid increase of Aphides whose secretions the wasps keenly appreciate, in addition to favouring the multiplication of flies, earwigs, etc., on which wasps to a great extent subsist; detrimentally by rendering it difficult to supply an adequate amount of moisture to the growing grubs. In some nests which were taken by chloroforming, I found numerous grubs shrivelled up and many of the wasps captured in the open were unusually small. This lack of water was very clearly shown by the way in which wasps swarmed round plants which were regularly watered so that the leaves and earth were constantly moist; in such cases wasps could be seen at all times of day greedily sipping up drops of water, or getting it from the wettest earth beneath the leaves.

From an economical point of view, a word in favour of the much-abused wasp is due. As scavengers, wasps undoubtedly confer great benefit upon us; the rapidity with which they remove the flesh from dead animals requires to be seen to be believed. On several occasions I exposed dead mice in order to notice the ravages produced by the wasps, and in each instance every particle of soft material was completely removed from the bones in the course of two days.

It is never wise to prophesy, but there are indications of what may be expected next year; the drones and young females have now been on the wing for some time, having appeared earlier than usual. This renders it probable that many females will be destroyed before they search out convenient spots for hibernation, so that already the balance is probably being readjusted, and unless the spring and early summer of 1894 prove repetitions of this year, there is no cause to anticipate a recurrence of the plague.

Charterhouse, Godalming. Oswald H. Latter.



PHOTOGRAPH OF LEFT WING OF *Archæopteryx*, from the specimen in the Natural History Museum, Berlin. *(Two-thirds nat. size.)*

IV.

Biological Theories.

VII.—THE DIGITS IN A BIRD'S WING: A STUDY OF THE ORIGIN AND MULTIPLICATION OF ERRORS.

THE plate accompanying this essay is a photograph from nature of the dorsal aspect of the left wing of the specimen of *Archæopteryx*, which is now in the Museum of the Berlin University, and which was found, in 1877, in a stratum belonging to the Upper Jurassic series, near Eichstätt in Bavaria. This photograph is now published because the best known figure of this specimen is characterised not so much by carelessness or by inadvertent error as by wilful falsification. That figure has, however, been copied into some of the best palæontological and geological treatises, and has received the authoritative sanction which insertion in Zittel's "Palæontologie" involves. In controverting views supported by such an authority, and views to be found even in the careful writings of Professor Huxley, and of almost every lesser light who has ever expressed an opinion on the subject, it is hardly probable that I should gain credence if I had given merely a new drawing. The photograph, however, will carry conviction.

In the plate, the humerus, the straight radius, and *curved* ulna, a carpal bone, and three long slender fingers may be seen at a glance. The fingers are all clawed, though the claw itself is not easy to make out: still the form of the ungual phalanx can leave no doubt upon the subject.

The fingers are recognised by everybody as being the equivalents (or homologues) of the digits I, II, and III of the normal pentadactyle sauropsidan fore-limb: this view is fully supported by the relative positions of the three fingers and by the numbers of phalanges—two, three, and four respectively—which they possess.

To the post-axial side (*left* in the plate) of the ulna and of the fingers is seen a wing in a marvellously perfect state of preservation. In the original, even the barbules of some of the quills are recognisable, and the barbs are easily seen even in the photograph. Of the quills, there are sixteen or seventeen (possibly even one more). The first seven are *primary* quills, and these are of chief importance to us for the present purpose. To merely state that the fourth is straight, and that the remainder are very nearly so, would not be sufficient. I must beg the reader to lay upon the plate the *straight* edge of a piece of paper, or a rule, and to see for himself that this is true, and that the concavity of the curvature of every primary quill is turned towards the middle one, *i.e.*, the fourth. The hinder ones are curved—but only *very* slightly—in such way that the concavity of the curve is turned forwards. If the line of the rhachis of each individual primary quill be traced onwards, a pretty accurate idea may be obtained as to where these *primary* quills were inserted.

In the Steinmann-Döderlein figure, which is spreading like a plague in modern books, those hinder primary quills which the photograph shows to be curved slightly *forward* (*i.e.*, with the concavity forwards), are represented as curving *strongly backwards*—curving, that is, through an angle of more than forty degrees! The distinction between primary and secondary quills is abolished, and all but the first two or three are represented as attached to the ulna! The second (or ? first) quill is represented as the longest, and the fourth, which is seen in the photograph to be the longest, is drawn much shorter than this, or rather as falling short of it at the tip. The continuation of the quills in a wrong direction as far as the ulna involves, in the case of the third one, the representation of the feather of nearly double its true length.

Nor is it difficult to guess how the originators of the figure came to draw it thus falsely. Inadvertent error on the part of the engraver is out of the question. The drawing is not to be described simply as erroneous, but as deliberately falsified. The uselessness of *strongly*-curved feathers for flight may not have occurred to the authors, but the absurdity of supposing (as many do!) that the three long, *slender*, and especially *weak-jointed* fingers could bear the torsional stress to which they would be exposed during flight if they supported those large quills, appears to have occurred to them and to have led them to avoid the absurdity of this, the everyday view, by the falsifications to which I have referred.

If the dissected wing of a common bird, such as a pigeon—the left wing—be laid on the table and compared with the plate of *Archæopteryx*, the conclusion that those two wings are essentially alike will be inevitable. It will be impossible to avoid the conclusion that the two digits which support the quills of the ordinary bird existed also in *Archæopteryx*, and their position will be seen to be indicated faintly in the photograph by a shadow which runs parallel with and behind the slender digits. The carpal angle of the wing will be seen in front of the carpal ends of the slender fingers, and from this point the outline of the anterior margin of the wing can be traced to the tip. This margin lies under those fingers. Not only did these, as their form and structure show, *not* support the quills, but they did not even contribute to the support of them. These fingers lie not in the wing at all, but *upon its feather-clad surface.*

Those slender fingers, like the free fingers of the *Pterodactyla*, or of the recent "flying" phalangers and squirrels, or of *Galeopithecus*, or like the pollex of a bat, are admirably adapted for climbing in trees. They proclaim *Archæopteryx* to have been a winged quadruped, and this conclusion receives ample support from the weakness of the vertebral column and of the hind limbs, and from the small size of the pelvis and sacrum.

In the dissected wing, or in any fairly good figure showing both feathers and bones in the wing of an ordinary bird (1, 2), the *ala spuria* will be seen to correspond to one or more of these free fingers of *Archæopteryx*. I cannot at present see any way of deciding whether it is a vestige of one, or of two, or of all three of them, and I cannot satisfy myself as to whether certain slender feathers seen to lie upon the primary quills in the plate, and making a considerable angle with them, are really coverts as usually described, or whether they were attached to the free fingers. If they are coverts, as seems probable, their position (lying *across* the primary quills) may be due to the action of a current of water flowing over the recently dead bird and bringing with it the mud which, being deposited—apparently rather quickly—effected the preservation of the specimen in the perfect state in which it was found.

Dames (3), in his detailed description of the specimen, states that the primary quills ("6 to 7") were attached to the longest finger (II). If while we look at the photograph we consider what would be the result of such an attachment, it must be obvious that it would be twofold. Firstly, the attachment of such a series of quills would render the fingers perfectly useless for climbing, and secondly, a single flap of that wing would twist the phalanges off at the joints. In other words, both wing and finger would be rendered useless by such an attachment.

If Natural Selection has been operating long enough and efficiently enough to determine the evolution of so perfect a series of feathers, it is perfectly certain that that same selection will have led also to the evolution of supports for those feathers as fully fitted to support the feathers as the feathers are fitted for flight; and even if there had been no indication of those supports on the slab, we need have still had no doubt as to their existence.

That the views which have gained such wide acceptance should have been possible when Owen (4) 30 years ago recognised and figured the very bones in question, is a striking testimony to the credulity of modern zoologists!

We read in any text-book that happens to be at hand that the three digits of a bird's wing answer to the "pollex and second and third digits of the pentadactyle fore-limb." Such a statement, though found in almost all modern books which treat of the subject at all, is in no case, so far as I am aware, supported by any evidence whatever. I believe the statement originated in a mistranslation of the pre-Darwinian statement that the ala spuria is "analogous to the thumb," while the other two digits are called simply "second" and "third," that is *second and third digits* not of the pentadactyle but *of the tridactyle fore limb*. Such phrases in works written on the then undoubted hypotheses of special creation and of fixity of species, could, obviously, not mean that the three digits called "thumb" and "second" and "third" digits had been evolved from the digits I, II, and III of the pentadactyle fore limb of an ancestor: the authors did not believe birds had ever had such ancestors. The transcription of such phrases into post-Darwinian treatises, without consideration of the new meaning which they would thus gain from the new context, appears to have been the origin of the error.

And yet Professor Huxley (5) has relied upon the unfounded statement that these digits are I, II, and III in constructing a theory of the phylogeny of birds. On the strength of this assumption he has denied the descent of birds from Pterodactyles. They may, of course, not be so descended, but we may not reasonably believe that his argument from the wing-structure makes their descent one jot more improbable than it was before.

It may at least be said that, in the matter of pelvis and of wing, *Archæopteryx* is much more like a Pterodactyle than it is like *Compsognathus*, or any other Dinosaur.

Those "ornithic" characters of the Dinosaurs which distinguish them from Pterodactyles, distinguish them also from the undoubted bird *Archæopteryx*; and if the "ornithic" characters of the Dinosaur pelvis generally, and of the foot of *Compsognathus*, are due to any blood-relationship with birds at all, it seems probable that the phylogenetic tree will have to be turned upside down so as to express the descent of Dinosaurs from Carinate birds through a series of "ratite" birds which had lost the power of flight through isolation and consequent suspension of the action of a selection dependent on powers of flight, much as the existing "Ratitæ" have probably descended from Carinates of very various orders.

It is, however, not to put forward new phylogenies that I now write, nor merely to correct a particular error in biological theory, but to point to one of the most fruitful sources of error.

To attempt to expose one by one all the current biological errors would be very much like going to Mecca, armed with a microscope, in order to eradicate cholera by finding the bacilli, and killing them one by one as they were found.

With reference to cholera epidemics, it has been said, "Cholera is a filth-disease: abolish filth, and cholera will vanish."

Uncritical credulity plays the same part in the spread of error as filth plays in the spread of cholera. The standard text-book of the anatomy of the Vertebrata (6) says, "In all Carinatæ there are three digits in the manus, which answer to the pollex, and the second and third digits of the pentadactyle fore limb"; and the statement is not accompanied by any evidence of any kind in support of it. In a healthy community of students, such statements would do no harm, for only a morbidly-credulous person unfit for scientific work would accept the statement as true, *on the authority* of even the greatest anatomist living. For an honest record of mere fact, we are often compelled to rely upon the accounts given by others—we do not all have the opportunity of dissecting *Nautilus* and other rare animals, and we none of us have time to dissect one-hundredth part of the animals of which specimens are easily obtainable. We are therefore compelled to rely upon each other, and so glaring a falsification as that shown in the Steinmann-Döderlein figure is, fortunately, not common. The discrepancy between the description and the figure (in Zittel, for instance) would, moreover, show any careful reader that either description or figure was false, if not both.

For facts, then, we must rely upon others, but must exercise judgment in doing so.

For opinions it is otherwise. To take another man's opinion and accept it untested—as has been done in the case of the bird's wing—is a sure way to something worse than mere error. Darwin was one of the greatest men who have lived, and his opinion is worth more than that of most other men. To accept even his opinion, however, except after examination of the argument upon which it rests, is evidence that the man so accepting it is unfit for scientific work. He will probably gain applause from "a popular audience" (just as a good encyclopædia may gain approval), but if he holds opinions otherwise than as the result of conviction he is only a walking encyclopædia, and not nearly so good a one as the "Britannica." So long as teachers continue to regard, as many do now regard, the "passing" of their students as the object of their work or of their students' work, so long will the "cramming" system continue to convert men into walking encyclopædias, stocked with second-hand ideas, and incapable of either creating new ideas or of judging of the value of the old ones.

In a paper by Morse (7) the view that the bird's *wing*-digits are II, III, and IV is put forward on the strength of the existence of a supposed vestige of another digit on the radial side. Granting his facts, such a vestige might be a vestige of a pre-pollex, so his contention does not prove even that the usual view is erroneous, though it lessens its apparent probability. It of course does not in the least affect the view I have put forward, for he makes no attempt to prove that the digits *are* II, III, and IV, but only that they are *not* I, II, and III. The possibility of their being III, IV, and V seems not to have occurred to him or to those who have adopted his view. What W. K. Parker (8) regarded as a vestige of the digit IV appears to me to be *os pisiforme.*

Zittel (9) gives a figure described as "nach Owen." Owen's figure shows *four* digits. Zittel has eliminated the innermost. It would not otherwise fit in with the orthodox view. Owen was a strong opponent of the Dinosaurian-ancestry theory, and his figure was valuable evidence in favour of the Pterodactyle-ancestry view which he maintained. Is it right that Zittel should so falsify that figure as to make it tell the other way, and then describe the mutilated figure as "nach Owen," thus ascribing to Owen a view which he strongly opposed?

The falsification of the figure is a typical example of the inevitable results of "pinning one's faith to a creed," the creed being, in this case, the orthodox view that the three digits in an ordinary bird's wing are I, II, and III.

SUMMARY.

Archæopteryx was a winged quadruped, probably arboreal in habit.

The photograph accompanying this essay shows the primary quills to have been supported by none of the first three digits, and justifies, if it does not even prove, the view that those quills were supported by the digits IV and V (or one of them). Portions of these large digits were figured by Owen thirty years ago, and they are seen in the London specimen and are quite unlike anything seen on the surface of the Berlin specimen, in which these digits probably still lie hidden.

This justifies the belief that the two large digits in an ordinary bird's wing are IV and V, and that the *ala spuria* is a vestige of one or more of the other three digits of the pentadactyle fore limb.

The argument against the view that birds are descended from Pterodactyles is, therefore, worthless so far as it rests upon the assumption that the large digits of the bird's wing are II and III.

The "ornithic" characters of Dinosaurs do not justify the view that birds are descended from these reptiles: for the oldest known bird is devoid of those "ornithic" characters.

Compsognathus may form a connecting link (but not, of course, in the direct line) between some unknown "ratite" birds on one hand, and the great Dinosaurs on the other, but only if these be regarded as "degenerate" and overgrown descendants of birds which had lost the power of flight.

Careless transcription of a pre-Darwinian statement (which was perfectly true) into post-Darwinian treatises has given the statement a new and false meaning.

Uncritical plagiarism has rendered the false statement an integral part of almost every modern treatise on the subject.

The false idea expressed in that statement after transcription has dominated the minds of even those (10, 11) who wrote with *Archæopteryx* before their eyes, and the absurdity of a corollary of that idea has induced some (12) to issue a figure so falsified as to hide the absurdity: and the falsity of that figure has gained for it admission into modern text-books written also under domination of the same false idea, while Owen's figure has been mutilated so as to make it reconcilable with a view which it was intended by Owen to disprove (9).

The spread of error is largely due to the domination of ideas (not invariably false!), and is possible only in a world of uncritical compilers and credulous readers and hearers.

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C. HERBERT HURST.

V.

The Problem of Variation.

IT is certain that, in order to get a deeper insight into the causes and modes of organic evolution, we must investigate the problem of variation. The first requisite for such an investigation is a definite grasp of the problem, an analysis of its complications. Much that is written at the present time on ultimate biological theories is more or less barren from the absence of a definite and comprehensive conception of the phenomena which have to be explained. It is easy to expand the proposition that, given all kinds of hereditary variations in individual organisms, selection will accumulate those which lie in a particular direction and result in special adaptations of structure to function. But what causes the variations? It is more important and more difficult to construct a theory of heredity as due to the continuity and conservatism of the germ-plasm, but we have to explain, not merely conservative, but progressive and retrogressive heredity.

It is necessary always to bear in mind that the phenomena to be dealt with are presented by individual organisms. There is no such thing as the origin of species apart from the origin of individuals. It has been maintained by philosophers that a horse can be locked in a stable, but the species horse cannot, being a mental abstraction formed from the perceptions of individuals; but it seems to me more correct to say that we recognise a number of individuals with such a degree of similarity to one another that we class them together and give them all a specific name. It would not be impossible to put all existing horses together into one enclosure.

Variations then, of course, must be variations of individuals; but a most important distinction, which is constantly ignored, exists between contemporary individual differences and the differences between consecutive generations, which form, so to speak, the units of evolution. The nature and degree of individual differences in a single species, the relations of varieties and doubtful species, are important enough as observed facts; but Darwin, Wallace, and others present them as the material on which selection acts, without adequately discussing the question of the relation they bear to the difference between one generation and its successor. Galton and Weldon have recently applied the higher mathematics to the study of individual differences, but it does not seem to me that in this way we shall make much advance towards an explanation of the variations which make evolution. The differences between a large number of individuals, without reference even to the ontogeny of these differences, are recorded with greater accuracy and completeness than before, but that is all. The same differences might have been present in every generation of the species if it had been created in the form in which we see it.

In order that evolution may take place, individuals must be generated which are different to any that ever existed before. If we take any particular organ or character, one or more individuals in a generation must have possessed this organ or character in a more advanced (or less advanced in the case of retrogression) condition than any individual which had previously existed. This is the kind of variation which we must account for, and find the cause of, in order to have a complete theory of evolution or of heredity. This must occur over and over again to produce the evolution which, according to the evidence, has taken place.

Now the actual appearance of new characters has been observed, for instance, in the case of the Ancon sheep, and in wild plants when cultivated; and, on the other hand, Weismann has proposed the theory that variations of the kind defined above are due to the union of two individuals in sexual reproduction; but I have not space to discuss, on this occasion, either of these two subjects, because, with regard to the second, Weismann has abandoned the theory, and, with regard to the first, such variations occur at the beginning of the development of the individual. Nothing is more certain than that no theory of variation is worthy of attention unless it takes into account the phenomenon of recapitulation. In a very large number of cases an organ, or a character, or an individual, passes through in development stages which reproduce, more or less exactly, an ancestral condition. In other words, new characters are added usually at the end of ontogeny. We have convincing evidence, then, that the modified individual first resembled its parent, and afterwards became different. The ancestor of the flat-fish was symmetrical; but, at some time or other, individuals of this pedigree, after developing into symmetrical fish, became more or less asymmetrical, and every flat-fish *at the present day* develops first into a symmetrical fish, and then turns into a Pleuronectid.

It is certain that no combination of invariable parental germ-plasms could result in this change. How, then, does Weismann's new theory account for it? The determinants, the units of the germ-plasm corresponding to particular cells, or group of cells, in the adult, are modifiable ultimately by external influences. On this new theory Weismann attempts the explanation of recapitulation in the following words:—

"The determinants of the id of germ-plasm become endowed with a greater power of multiplication, so that each one of them causes the addition of one or more cell-generations to the end of the ontogeny. At the same time, the determinants in the germ-plasm increase in number, and each of them becomes differentiated in a fresh manner. As, however, every two new determinants always follow the same course, from the id of germ-plasm to the final stage in ontogeny as was taken by the single original determinant, they will pass through the same determinant figures as before, and only lead to the formation of new structures in the final stages when they become separated from one another. The ontogenetic stages will be repeated less accurately the nearer development approaches its termination."

Now, for my part, I cannot see that this explanation in any way covers the phenomena. According to the theory, the specialisation of every cell and every organ at every stage depends on determinants. The problem to be explained is, therefore, why the determinants for the organs of the early stages of ontogeny are not modified. Weismann supposes that in the differentiated cells of any stage determinants are used up, while in the undifferentiated multiplying cells unconsumed determinants are held in reserve to supply later generations of differentiated cells. The determinant figure is merely an attempt to show why the consumption of the determinants follows a definite course. It does not afford any reason why the determinants which are reserved to the last should be modified, while those which pass into differentiated cells belonging to early stages remain unaltered.

As to the cause of changes in the determinant, Weismann now attributes them to the direct effect of external influences, which influence the nutrition of the biophors and determinants during their continual reproduction through countless generations; but in his chapter on variation he mentions no reason why such changes should occur rather in the determinants belonging to the final stages of ontogeny, than in those belonging to the earlier stages.

It may be truly said, therefore, that we have no theory of variation which necessarily involves the recapitulation of phylogeny, except the theory of the inheritance of acquired characters. It is important to notice that recapitulations occur chiefly in connection with adaptive modifications. The young flat-fish swims with its principal plane in a vertical position, and its body is symmetrical about that plane; the adult flat-fish, on the contrary, is adapted by its asymmetry to living in a position in which the principal plane of the body is horizontal. The tadpole is essentially a fish adapted to live in water, while the frog is adapted to breathe air and lead a terrestrial existence. These obvious and striking facts point, at all events apparently, to the conclusion that the change of structure was produced by the change of conditions.

Another consideration which points in the same direction is that the actual ontogeny of these changes of structure in the individual has been shown in some instances to be largely dependant on conditions. For instance, the metamorphosis of tadpoles can be prevented for a long time by certain appropriate conditions, which would not be the case if the changes were entirely determined by modification of the germ-plasm.

A case which I have myself recently investigated experimentally seems to me to support very strongly the theory of the inheritance of acquired characters. I have shown that in normal flat-fishes, if the lower side be artificially exposed to light for a long time, pigmentation is developed on that side; but when the exposure is commenced while the specimens are still in process of metamorphosis, when pigment-cells are still present on the lower side, the action of light does not prevent the disappearance of these pigment-cells. They disappear as in individuals living under normal conditions, but after prolonged exposure pigment-cells reappear. The first fact proves that the disappearance of the pigment-cells from the lower side in the metamorphosis is a hereditary character, and not a change produced in each individual by the withdrawal of the lower side from the action of light. On the other hand, the experiments show that the absence of pigment-cells from the lower side throughout life is due to the fact that light does not act upon that side, for when it is allowed to act, pigment-cells appear. It seems to me the only reasonable conclusion from these facts is that the disappearance of pigment-cells was originally due to the absence of light and that this change has now become hereditary. The pigment-cells produced by the action of light on the lower side are in all respects similar to those normally present on the upper side of the fish. If the disappearance of the pigment-cells were due entirely to a variation of the germ-plasm, no external influence could cause them to reappear, and, on the other hand, if there were no hereditary tendency, the colouration of the lower side of the flat-fish, when exposed, would be rapid and complete.

Weismann admits that hereditary colour-changes, such as a darker colour in butterflies, are produced by climatic influences, but explains this on the supposition that the climatic influences have simultaneously modified in an individual the determinants of the pigmented cells in the integument, and the determinants in the germ-cells of that individual which belong to the corresponding pigmented cells of its offspring. Now it is impossible to suppose that the light acting on one side of a flat-fish would modify the determinants belonging to one side in the ova of that individual, and even if it did it would not cause the pigment-cells to develop symmetrically on both sides of the larva, and then to disappear from one side of the perfect fish.

It is impossible or inconceivable on Weismann's theory that changes of somatic cells should affect the determinants of the germ-cells contained in the soma; there is no connection between the two; but, on the other hand, it is equally inconceivable on his theory that the presence or absence of the germ-cells in an individual should affect the ontogeny of the soma. He assumes that the determinants for the soma are separated off and determine a certain ontogeny, which is not affected by the fate of the determinants of succeeding generations contained in the germ-cells and germ-tracks; and yet we know that male secondary sexual characters are not developed if the young male is castrated. The development of these characters depends on the normal presence of the testes and their germ-cells in the individual. Weismann mentions this fact, but how does he connect it with his theory? He simply concludes that in such cases the secondary sexual characters of both sexes are present in each individual, and one or the other set develop according to circumstances. He makes no attempt to show why the particular circumstance which determines the development is the presence or removal of the generative organs. The fact of the course of ontogeny in this case being affected in so radical a manner by the mere presence of the generative organs, is alone sufficient to overthrow his fundamental assumption of the essential independence of the soma and the germ-cells contained within it.

Degeneration of organs is a phenomenon in which recapitulation is particularly well exhibited. Scarcely any case is known of an organ which has disappeared or become rudimentary in the adult condition, and which is not more perfectly developed or developed to some extent in earlier stages of the life-history. Even the limbs of snakes are indicated to some extent in the embryo, and the teeth of the right whale are present in the fœtus; but the loss of the eyes in blind subterranean animals is the instance I prefer to bring forward here. On the Natural Selection theory it is no advantage to such animals that the larvæ or the young should have eyes better developed than the adults. Yet this is known to be the case in the blind amphibian Proteus of the caves of Dalmatia, the blind crayfish of the caves of Kentucky, and in other cases. This case has a superiority over others in my argument, because there is no change of conditions as in the frog and the flat-fish: the whole development takes place in the dark. Weismann considers this case in relation to his theory, but does not succeed in showing any logical connection between his assumptions and the phenomenon. He supposes that the determinants for the degenerating organ first begin to lose their power of multiplication, and then a continually increasing number disappear. This means, presumably, that a succession of determinants is necessary to supply the successive generations of cells which keep an organ in proper condition, and that the reserve determinants in the soma are continually multiplying when degeneration does not take place; but we know that in the life of the individual this "multiplication of determinants" is checked or stopped by disuse, and we do not know how the "power of multiplication" is diminished in the germ-plasm. In spite of all that has been discovered or conceived up to the present, the hypothesis that the properties of the germ-plasm are modified by changes effected in the adult organs by external influences still holds the field.

It must be conceded, even by his opponents, that Weismann has done a very great service to biological science by insisting upon and forcing upon the general attention the actual relations of the germ-cells to the soma. We cannot now overlook the fact that germ-cells are not descended from the modified and specialised cells of the soma, nor can we any longer use a hypothesis of the construction of the germ-cells from gemmules emitted by the somatic cells. The germ-cells, like the cells of a muscle or a gland are derived directly from the ovum by a succession of multiplying cells forming the germ-track; but we have still to discover how the ontogenetic properties of these germ-cells are modified. In the present state of knowledge it is a more irresistible conclusion that changes produced in the soma ultimately affect the germ-cells, than that variations in the latter are due to variations of nutrition in some manner which no one has attempted to describe.

J. T. Cunningham.

VI.

The Nearctic Region and its Mammals.[1]

IT would appear that the Munroe doctrine of "America for the Americans," is little heeded by the biologists of that Continent. Although the best European authorities on the geographical distribution of animals have long ago conceded to the northern half of the New World the rank of one of the six primary divisions of the earth's surface, under the name of the "Nearctic Region," our American friends will have none of it. Two recent writers of deservedly great authority on the Mammal-life of North America, though, as we shall show presently, they differ much in minor details, agree in repudiating the Nearctic Region altogether. They refer the northern parts of North America to the "Arctic Realm" or "Boreal Region," of the Old World, and the southern portion to the "American Tropical Realm" or "Tropical Region," leaving only the intermediate area separate and apart. This intermediate area is included by Mr. Allen in his "North Temperate Realm," which embraces the whole of the northern hemisphere on both sides of the Atlantic, between the annual isotherms of 32° and 70°, but is allowed to rank independently as the "North American Region," while Dr. Merriam, following Cope, calls it the "Sonoran Region." Thus both these authorities agree in splitting up the Nearctic Region into three constituent parts, and in repudiating the views of Sclater and Wallace that it should form one of the main zoo-geographical divisions of the earth's surface. While, however, it must be allowed that both Mr. Allen and Dr. Merriam have studied the distribution of Mammal-life in North America to some effect, and are well acquainted with its details, it will be easy to show that their general views on geographical distribution are not entitled to acceptance. At the same time, we admit that there is something to be said on their side of the question.

Taking a general view of the distribution of Mammal-life over the terrestrial portion of the earth's surface, we see at once that Australia and the adjacent islands stand strongly apart from the rest

[1] i. "The Geographic Distribution of Life in North America, with Special Reference to the Mammalia." By C. Hart Merriam, M.D. *Proc. Biol. Soc. Washington*, vol. vii., p. 1 (April, 1892).

ii. "The Geographical Distribution of North American Mammals." By Joel Asaph Allen. *Bull. Amer. Mus. Nat. Hist.*, vol. iv., no. 1 (Dec., 1892).

of the world as being principally tenanted by Marsupials, and as being the sole home of the existing Monotremes. Again, South and Central America together form a division easily separable from the remaining portion of the world, after Australia has been subtracted. The "Neotropical Region," as it is usually called, has a family of Marsupials peculiar to it; it has almost no Insectivores, and it is the home of all the three typical families of the great Order Edentata. Thus we have a very obvious threefold division of the earth's surface, taking Mammals as our text, into what may be called *Notogæa*, *Neogæa*, and *Arctogæa*. The vice of this division is that it leaves the great bulk of the earth's surface (*Arctogæa*), which remains after taking off the Australian and Neotropical Regions, of rather unmanageable size, consisting as it does of North America and the whole of the Old World except Australia and its islands. This large area, however, readily falls into four sections—North America, Europe and Northern Asia, Africa, and Southern Asia, which are denominated by Sclater and Wallace the "Nearctic," "Palæarctic," "Æthiopian," and "Oriental" regions respectively. No one pretends to say that these four regions are exact equivalents in zoological value to the two regions first spoken of—namely, the "Australian" and "Neotropical" regions. As Mr. Wallace ("Geogr. Distr.," vol. i., p. 66) has well put it:—

"It is admitted then that these six regions are by no means of precisely equal rank, and that some of them are far more isolated and better characterised than others; but it is maintained that, looked at from every point of view, they are more equal in rank than any others that can be found; while as regards geographical equality, compactness of area, and facility of definition, they are beyond all comparison better than any others that have yet been proposed for the purpose of facilitating the study of geographical distribution."

Mr. Allen and Dr. Merriam seem to be of opinion that when we come to scientific questions we are bound to throw away considerations of convenience altogether and to stick to matters of fact. This is a beautiful doctrine, but if the matters of fact are in dispute, as is the case in many of the details of geographical distribution, owing to our as yet imperfect knowledge of the subject, it is far better to adopt some easily comprehensible system, of which the leading features are obviously correct, than to use a more elaborate plan, based upon details that are more or less open to question. Contrast, for example, the six divisions of the world already given, their simple names and their easily-defined boundaries, with Mr. Allen's seven "Primary Life-Regions," their complicated titles and their uncertain limits. Where are we to draw the line between the "American Tropical" and the "South American Temperate Realm," or between the "Arctic" and the "North Temperate," both of which embrace portions of the Old and the New Worlds? Than Africa, as regards its Mammals at least, no part of the world, except Australia, has a better set of characteristic types, such as *Hippopotamus*, *Camelopardalis*, *Orycteropus*, *Hyrax*, and many others. Yet Mr. Allen proposes to unite Africa with India as the "Indo-African Realm," while he would actually split up South America between two different regions. In these proposals, at least, very little consideration is shown to the much-lauded principle of the equivalence in value of the realms or regions.

While, however, we insist upon the propriety of retaining the Nearctic Region as a whole, and as one of the primary zoo-geographical divisions of the earth's surface, we are quite disposed to attend to the views of Messrs. Merriam and Allen as to the best mode of subdividing the large area, and to admire the maps and tables in which they have set it forth. Take, for example, Mr. Allen's first map of his so-called "Realms" of North America (*Bull. A. M. N. H.*, vol., iv., pl. v.). Here we find a threefold division of the contiment proposed into "Arctic," "Temperate," and "Tropical" Realms. The "Arctic Realm," which consists merely of the land bordering the Polar Ocean and Hudson's Bay and the great peninsula of Greenland, and is "beyond the limit of arboreal vegetation," Mr. Allen unites to the similar arctic portion of the Old World, stating, no doubt quite correctly, that it is really a part of a "homogeneous hyperborean fauna of circumpolar distribution." But looking to the extreme proverty of life in these inclement latitudes, as Mr. Allen well puts it, we think it quite unnecessary to elevate this wretched fraction of the earth's surface to one of its principal constituent life-regions, and must prefer the plan adopted by Sclater and Wallace, of regarding it as a borderland between the Nearctic and Palæarctic Regions.

We now come to Mr. Allen's "North American Region," which is regarded by the author as a subdivision of the "North Temperate Realm," corresponding in value to what is usually called the "Palæarctic Region" of the Old World, but what Mr. Allen prefers to denominate by the horrible compound term "Eurasia." Mr. Allen's "North American Region" embraces, as will be seen by a glance at the map already referred to, by far the largest portion of that continent. In fact, it embraces the whole, except the extreme arctic portion already referred to, and what Mr. Allen terms "Tropical North America," which consists of the southern end of the peninsula of Florida and a narrow strip of the coast of Mexico on both sides, extending on the Atlantic side up to the Rio Grande and on the Pacific side up to Mazatlan. This "Tropical North America" of Mr. Allen, however, is, in fact, merely the borderland between the Nearctic and Neotropical Regions of Sclater and Wallace, and may be left out of account when a general view of the great life-regions of the world is being taken. We thus see that Mr. Allen's "North American Region" is, for all practical purposes, identical with the Nearctic Region of Sclater and Wallace. Let us now see how Mr. Allen proposes to divide it—according to its Mammal-life.

"The North American Region," Mr. Allen tells us (*op. cit.*, p. 221), "falls into two sub-regions, namely (1) a 'Cold-Temperate Sub-region' extending southward to about the mean latitude of the Great Lakes, with outlying portions extending further southwards along the principal mountain-system of the continent, and (2) a 'Warm-Temperate Sub-region' occupying the remaining area." This mode of division (which is shown on Mr. Allen's plate vi.) corresponds pretty nearly with that proposed by Dr. Merriam (see his Biogeographic Map), who calls those two sub-regions "Boreal" and "Sonoran"; but upon comparing the maps of the two authors together, it will be seen at a glance that the minor subdivisions do not exactly correspond, and indeed such particulars must be always more or less matters of opinion.

In his carefully drawn up tables of North American Mammals, Mr. Allen shows that fourteen genera occur in his Cold-Temperate Sub-region, which do not range to any extent into the Warm-Temperate Sub-region. On the other hand, 33 genera found in the Warm-Temperate do not occur in the Cold-Temperate, while 27 genera are, to a greater or less extent, common to both their sub-regions. He points out that the 42 genera of the Cold-Temperate Sub-region are "either obviously of boreal origin, or find their nearest relationship with boreal types," while of the 62 genera which occur in the Warm-Temperate Sub-region, "about fourteen are wide-ranging southern or subcosmopolitan types, 24 may be regarded as indigenous, and about thirteen are of southern origin."

Neglecting, therefore, as we have already proposed to do, Mr. Allen's strip of the "Arctic Realm" in the North American Fauna as merely borderland, and, in a similar way, treating the area south of the Mexican Tableland and the extreme southern parts of the peninsulas of Lower California and Florida as merely debatable land between the Nearctic and Neotropical Regions, we find Mr. Allen's "North American Region of the North Temperate Realm" practically identical with the Nearctic Region of Sclater and Wallace. Following the guidance of Mr. Allen and Dr. Merriam, we recognise two sub-regions only in the region, namely, a Northern and a Southern one.

The Northern Sub-region is composed of Mr. Allen's "Cold-Temperate Sub-region," and the adjacent district on the north which he assigns to the "Arctic Realm." Here the general facies of the Mammal-fauna is much more decidedly similar to that of the Palæarctic Region than in the Southern Sub-region. Such genera as *Cervus*, *Alces*, *Rangifer*, *Ovis*, *Castor*, *Lagomys*, *Gulo*, and *Putorius*, betray at once a very strong Palæarctic element. At the same time, a decidedly endemic element is shown by such types as *Haploceros*, *Fiber*, *Condylura*, *Procyon*, *Mephitis*, and *Taxidea*, which are absolutely unknown in the Palæarctic Mammal-fauna. For this Northern Sub-region of the Nearctic Region perhaps "Canadian" would be the best term, as it embraces the whole of the Dominion of Canada, and the area of the sub-region is therefore at once recognisable by the name. Mr. Allen's term "Boreal" is much too vague.

The Southern Sub-region is composed of Mr. Allen's "Warm Temperate Sub-region," and the fragmentary bits of borderland to the south, which he assigns to the "American Tropical Realm." For this Sub-region "Sonoran," as used by Dr. Merriam, seems to be a good and appropriate term, much preferable to Mr. Allen's "Warm-Temperate," which is quite indefinite. The Sonoran Sub-region of the Nearctic Region, as we propose to call it, contains a much smaller Palæarctic element than the Canadian Sub-region. Nevertheless, *Bison*, *Tamias*, *Spermophilus*, *Arvicola*, and others, are undoubtedly Palæarctic types. On the other hand, *Antilocapra*, *Cynomys*, *Sigmodon*, *Oryzomys*, *Neotoma*, *Thomomys*, *Dipodomys*, *Scalops*, *Urotrichus*, and *Bassaris*, with their allies, constitute a strong endemic force. But what principally differentiates the Sonoran from the Canadian Sub-region is the presence in it of a lot of intruders from the Neotropical Region—such as *Didelphys*, *Dicotyles*, *Cariacus*, *Tatusia*, *Nasua*, and *Molossus*, which have nothing to do with the autochthonous fauna of North America. On the whole, the Nearctic Mammal-fauna may be defined as having a strong Palæarctic basis mixed up with endemic elements, and invaded largely on its southern frontiers by Neotropical immigrants. The presence of these Neotropical immigrants serves chiefly to distinguish the Sonoran from the Canadian Sub-region, though, as Mr. Allen has shown, there is also a material difference in the endemic forms peculiar to the respective Sub-regions.

VII.

The British Association Addresses, 1893.

SUCH authoritative expressions of opinion on current questions of scientific interest as always characterise the Presidential Addresses delivered to the British Association and its several Sections, are of importance in indicating the tendency of research; but it is not often that these general discourses contain much new matter. We are, therefore, not expressing any feeling of disappointment, when we remark upon the conspicuous lack of new facts and unfamiliar ideas in the addresses relating to Natural Science delivered last month at Nottingham. Everyone interested in Physiology, Petrology, Field Natural History, Arctic Exploration, and Anthropology, will have read the carefully-prepared digests of current thought and research with profit; and if there is little in them that is absolutely new, there are at least some personal expressions of opinion of the deepest significance.

Dr. Burdon Sanderson, the President, dealt with the present aspect of the problems of Physiology; and the whole address, full of the history of progress in the science, seems to have been designed to lead up to the climax—a plea for the establishment of a "British Institute of Preventive Medicine."

"It is possible that many members of the Association are not aware of the unfavourable—I will not say discreditable—position that this country at present occupies in relation to the scientific study of this great subject—the causes and mode of prevention of infectious diseases. As regards administrative efficiency in matters relating to public health, England was at one time far ahead of all other countries, and still retains its superiority; but as regards scientific knowledge we are, in this subject as in others, content to borrow from our neighbours. Those who desire either to learn the methods of research or to carry out scientific inquiries, have to go to Berlin, to Munich, to Breslau, or to the Pasteur Institute in Paris, to obtain what England ought long ago to have provided. For to us, from the spread of our race all over the world, the prevention of acute infectious diseases is more important than to any other nation. May I express the hope that the effort which is now being made to establish in England an Institution for this purpose not inferior in efficiency to those of other countries, may have the sympathy of all present ? "

Physiology not Physics.

The final expression of Dr. Sanderson's opinion of the nature of physiological processes is of great interest. There is a certain "specific energy of cells" that neither physics nor chemistry can explain ; and physiology can never become a mere branch of applied physics or chemistry. Nevertheless, there are parts of physiology wherein the principles of these sciences may be applied directly. "Thus, in the beginning of the century, Young applied his investigations as to the movements of liquids in a system of elastic tubes, directly to the phenomena of the circulation ; and a century before Borelli successfully examined the mechanism of locomotion and the action of muscles, without reference to any excepting mechanical principles. Similarly, the foundation of our present knowledge of the process of nutrition was laid in the researches of Bidder and Schmidt, in 1851, by determinations of the weight and composition of the body, the daily gain of weight by food or oxygen, the daily loss by the respiratory and other discharges, all of which could be accomplished by chemical means. But in by far the greater number of physiological investigations, both methods (the physical or chemical and the physiological) must be brought to bear on the same question —to co-operate for the elucidation of the same problem. In the researches, for example, which during several years have occupied Professor Bohr, of Copenhagen, relating to the exchange of gases in respiration, he has shown that factors purely physical—namely, the partial pressures of oxygen and carbon dioxide in the blood which flows through the pulmonary capillaries—are, so to speak, interfered with in their action by the 'specific energy' of the pulmonary tissue, in such a way as to render this fundamental process, which, since Lavoisier, has justly been regarded as one of the most important in physiology, much more complicated than we for a long time supposed it to be. In like manner Heidenhain has proved that the process of lymphatic absorption, which before we regarded as dependent on purely mechanical causes—*i.e.*, differences of pressure—is in great measure due to the specific energy of cells, and that in various processes of secretion the principal part is not, as we were inclined not many years ago to believe, attributable to liquid diffusion, but to the same agency. I wish that there had been time to have told you something of the discoveries which have been made in this particular field by Mr. Langley, who has made the subject of 'specific energy' of secreting-cells his own. It is in investigations of this kind, of which any number of examples could be given, in which vital reactions mix themselves up with physical and chemical ones so intimately that it is difficult to draw the line between them, that the physiologist derives most aid from whatever chemical and physical training he may be fortunate enough to possess."

Uniformitarianism in Geology.

Mr. Teall's address to the Geological Section is the most remarkable for startling expressions of opinion, and may be regarded as indicating another reaction in the ever vacillating body of enquirers into the history of the earth. Speaking as a petrologist, Mr. Teall remarks that although enormous progress has been made in this science during the last hundred years, there has been comparatively little advance so far as broad, general theories relating to the origin of rocks are concerned. He even declares his belief, that those who have deserted the old school of Uniformitarians for a certain modern creed of Evolution, will ere long discover their mistake. "The uniformitarian hypothesis, as applied to the rocks we can examine, has assimilated and co-ordinated so many facts in the past, and is assimilating and co-ordinating so many new discoveries, that we should continue to follow it, rather than plunge into the trackless waste of cosmogonical speculation in pursuit of what may after all prove to be a will-o'-the-wisp." " . . . The good old British ship, 'Uniformity,' built by Hutton, and refitted by Lyell, has won so many glorious victories in the past, and appears still to be in such excellent fighting trim, that I see no reason why she should haul down her colours either to Catastrophe or Evolution. Instead, therefore, of acceding to the request to 'hurry up,' we make a demand for more time. The early stages of the planet's history may form a legitimate subject for the speculations of mathematical physicists, but there seems good reason to believe that they lie beyond the ken of those geologists who concern themselves only with the record of the rocks."

Crystalline Schists.

The earliest sedimentary rocks in Britain, Mr. Teall admits, present some differences from the later rocks in this country, but he doubts whether any conclusions of universal application can be drawn from the fact. The common idea, however, that crystalline schists belong exclusively to the earliest periods of the earth's history, and therefore are witnesses of cosmic evolution, is combated in a vigorous manner. "The crystalline schists certainly do not form a natural group. Some are undoubtedly plutonic igneous rocks showing original fluxion; others are igneous rocks which have been deformed by earth-stresses subsequent to consolidation; others, again, are sedimentary rocks metamorphosed by dynamic and thermal agencies, and more or less injected with 'molten mineral matter'; and lastly, some cannot be classified with certainty under any of these heads. So much being granted, it is obvious that we must deal with this petrographical complex by separating from it those rocks about the origin of which there can be no reasonable doubt. Until this separation has been effected, it is quite impossible to discuss with profit the question as to whether any portions of the primitive crust remain. In order to carry out this work it is necessary to establish some criterion by which the rocks of igneous may be separated from those of sedimentary origin. Such a criterion may, I think, be found, at any rate in many cases, by combining chemical with field evidence. If associated rocks possess the composition of grits, sandstones, shales, and limestones, and contain also traces of stratification, it seems perfectly justifiable to conclude that they must have been originally formed by processes of denudation and deposition. That we have such rocks in the Alps and in the Central Highlands of Scotland, to mention only two localities, will be admitted by all who are familiar with those regions. Again, if the associated rocks possess the composition of igneous products, it seems equally reasonable to conclude that they are of igneous origin. Such a series we find in the North-West of Scotland, in the Malvern Hills, and at the Lizard. In applying the test of chemical composition, it is very necessary to remember that it must be based, not on a comparison of individual specimens, but of groups of specimens. A granite and an arkose, a granitic gneiss and a gneiss formed by the metamorphosis of a grit, may agree in chemical and even in mineralogical composition. The chemical test would therefore utterly fail if employed for the purpose of discriminating between these rocks. But when we introduce the principle of paragenesis it enables us in many cases to distinguish between them. The granitic gneiss will be associated with rocks having the composition of diorites, gabbros, and peridotites; the sedimentary gneiss with rocks answering to sandstones, shales, and limestones. Apply this test to the gneisses of Scotland, and I believe it will be found in many cases to furnish a solution of the problem."

" . . . The origin of gneisses and schists, in my opinion, is to be sought for in a combination of the thermal and dynamic agencies which may be reasonably supposed to operate in the deeper zones of the earth's crust. If this view be correct, it is not improbable that we may have crystalline schists and gneisses of post-Silurian age in the North-West of Europe formed during the Caledonian folding, others in Central Europe of post-Devonian age due to the Hercynian folding, and yet others in Southern Europe of post-Cretaceous age produced in connection with the Alpine folding. But if the existence of such schists should ultimately be established, it will still probably remain true that rocks of this character are in most cases of pre-Cambrian age. May not this be due to the fact, suggested by a consideration of the biological evidence, that the time covered by our fossiliferous records is but a small fraction of that during which the present physical conditions have remained practically constant?"

The Polar Basin.

The address of Mr. Seebohm to the Geographical Section is perhaps the most readable, but it contains nothing new beyond six beautiful maps, illustrating the River Basins, Temperature, Rain and Snow, Heights and Depths, and Vegetation, prepared under the direction of Mr. E. G. Ravenstein. The general reader, however, will perhaps obtain a clearer broad view of the Arctic Regions from this carefully-prepared address, than from any previously-published description.

Zoo-geographical Regions.

Incidentally Mr. Seebohm expresses his views on the possibility of dividing the earth's surface into regions, and then makes some interesting remarks on the special case of the Polar Basin. "The fact is that life areas, or zoo-geographical regions, are more or less fanciful generalisations. The geographical distribution of animals, and probably also that of plants, is almost entirely dependent upon two factors, climate and isolation, the one playing quite as important a part as the other. The climate varies in respect of rainfall and temperature, and species are isolated from each other by seas and mountain ranges. The geographical facts which govern the zoological provinces consequently range themselves under these four heads. It is at once obvious that the influences which determine the geographical distribution of fishes must be quite different from those which determine the distribution of mammals, since the geographical features which isolate the species in the one case, are totally different from those which form impassable barriers in the other. It is equally obvious that the climatic conditions which influence the geographical range of mammals, must include the winter cold as well as the summer heat, while those which determine the geographical distribution of birds, most of which are migratory in the Arctic Regions, is entirely independent of any amount of cold which may descend upon their breeding grounds during the months which they spend in their tropic or sub-tropic winter quarters. Hence all attempts to divide the Polar Basin into zoological regions or provinces are futile. Nearly every group of animals has zoological regions of its own, determined by geographical features peculiar to itself, and any generalisations from these different regions can be little more than a curiosity of science. The mean temperature or distribution of heat can be easily ascertained. It is easy to generalise so as to arrive at an average between the summer heat and the winter cold, because they can be both expressed in the same terms. When, however, we seek to generalise upon the distribution of animal or vegetable life, how is it possible to arrive at a mean geographical distribution of animals? How many genera of molluscs are equal to a genus of mammals, or how many butterflies are equal to a bird? If there be any region of the world with any claim to be a life area, it is that part of the Polar

Basin which lies between the July isotherm of 50 deg. or 53 deg. F. and the northern limit of organic life. The former corresponds very nearly with the northern limit of forest growth, and they comprise between them the barren grounds of America and the tundras of Arctic Europe and Siberia. The fauna and flora of this circumpolar belt is practically homogeneous; many species of both plants and animals range throughout its whole extent. It constitutes a circumpolar Arctic region, and cannot consistently be separated at Behring Strait into two parts of sufficient importance to rank even as sub-regions. The mean temperature of a province is a matter of indifference to some plants and to most animals. The facts which govern their distribution are various, and vary according to the needs of the plant or animal concerned. To a migratory bird the mean annual temperature is a matter of supreme indifference. To a resident bird the question is equally beside the mark. The facts which govern the geographical distribution of birds are the extremes of temperature, not the means. Arctic birds are nearly all migratory. Their distribution during the breeding season depends primarily on the temperature of July, which must range between 53 deg. and 35 deg. F. It is very important, however, to remember that it is actual temperature that governs them, not isotherms corrected to sea-level. If an Arctic bird can find a correct isotherm below the Arctic circle by ascending to an elevation of 5,000 or 6,000 ft. above the level of the sea, it avails itself of the opportunity. Then the region of the Dovrefield above the limit of forest growth is the breeding place of many absolutely Arctic birds; but this is not nearly so much the case on the Alps, because the cold nights vary too much from the hot days to come within the range of the birds' breeding grounds. Here, again, the mean daily temperature is of no importance. It is the extreme of cold which is the most potent factor in this case, and no extreme of heat can counterbalance its effect."

The Field Naturalist.

Canon Tristram's address to the Biological Section, as might be expected, is devoted to the interests of the Field Naturalist. It is a matter of gratification, for once, to turn from the modern professor and his laboratory to a distinguished biologist whose work has been chiefly accomplished in the field. Observations of organisms in their natural surroundings are not merely essential for the understanding of such broad questions as geographical distribution, variation, mimicry, migration, and so forth; but they are also, as the Canon points out, often indispensable even in researches for which museum cabinets are commonly deemed to furnish adequate material.

"The closet systematist is very apt to overlook or to take no account of habits, voice, modification, and other features of life which have an important bearing on the modification of species. To take one instance, the short-toed lark (*Calandrella brachydactyla*) is spread over the countries bordering on the Mediterranean; but, along with it, in Andalusia alone is found another species, *Cal. bætica*, of a rather darker colour, and with the secondaries generally somewhat shorter. Without further knowledge than that obtained from a comparison ot skins, it might be put down as an accidental variety. But the field naturalist soon recognises it as a most distinct species. It has a different voice, a differently shaped nest; and, while the common species breeds in the plains, this one always resorts to the hills. The Spanish shepherds on the spot recognise their distinctness, and have a name for each species. Take, again, the eastern form of the common song-thrush. The bird of North China, *Turdus auritus*, closely resembles our familiar species, but is slightly larger, and there is a minute difference in the wing formula. But the field naturalist has ascertained that it lays eggs like those of the missel-thrush, and it is the only species closely allied to our bird which does not lay eggs of a blue ground colour. The hedge accentor of Japan (*Accentor rubidus*) is distinguished from our most familiar friend, *Accentor modularis*, by delicate differences of hue. But, though in gait and manner it closely resembles it, I was surprised to find the Japanese bird strikingly distinct in habits and life, being found only in forest and brushwood several thousand feet above the sea. I met with it first at Chinsenze—6,000 ft.—before the snow had left the ground, and in summer it goes higher still, but never descends to the cultivated land. If both species are derived, as seems probable, from *Accentor immaculatus* of the Himalayas, then the contrast in habits is easily explained. The lofty mountain ranges of Japan have enabled the settlers there to retain their original habits, for which our humbler elevations have afforded no scope."

Migration of Animals.

In the long-studied subject of the migration of birds and other animals, the field naturalist can also still find continuous and profitable occupation; and Canon Tristram calls attention especially to an exceptional phenomenon, "Not the mere wanderings of waifs and strays, nor yet the uncertain travels of some species, as the crossbill in search of food, but the colonising parties of many gregarious species, which generally, so far as we know in our own hemisphere, travel from east to west, or from south-east to north-west. Such are the waxwing (*Ampelis garrula*), the pastor starling (*Pastor roseus*), and Pallas's sandgrouse, after intervals sometimes of many years, or sometimes for two or three years in succession. The waxwing will overspread Western Europe in winter for a short time. It appears to be equally inconstant in its choice of summer quarters, as was shown by J. Wolley in Lapland. The rose pastor regularly winters in India, but never remains to breed. For this purpose the whole race seems to collect and travel north-west, but rarely, or after intervals of many years, returns to the same quarters. Verona, Broussa, Smyrna, Odessa, the Dobrudscha, have all during the last half-century been visited for one summer by tens of thousands, who are attracted by the visitations of locusts, on which they feed, rear their young, and go. These irruptions, however, cannot be classed under the laws of ordinary migration. Not less inexplicable are such migrations as those of the African darter, which, though never yet observed to the north of the African lakes, contrives to pass, every spring, unobserved to the lake of Antioch in North Syria, where I found a large colony rearing their young, which, so soon as their progeny was able to fly, disappeared to the south-east as suddenly as they had arrived."

The Perfection of the Human Frame.

The last of the addresses requiring notice, that of Dr. Munro to the Anthropological Section, will scarcely bear abstracting. It discusses, in an interesting manner, the direct and collateral advantages conferred on man by the erect position of his frame; and there are some noteworthy remarks on the theory of Natural Selection as applied to man. Dr. Munro asks Dr. A. R. Wallace to explain why, in his philosophy, he dispenses with the operation of a "higher intelligence" in the early stages of man's evolution, and finds its assistance only requisite to give the final touches to humanity. We shall be interested to learn the reply.

SOME NEW BOOKS.

Naturalist's Map of Scotland. By J. A. Harvie-Brown and J. G. Bartholomew. Edinburgh: John Bartholomew & Co., 1893. Price 2s. 6d.

This is a beautifully-executed map of Scotland, on the scale of 10 miles to an inch, full of information of value not only to the naturalist, but also to tourists and sportsmen. The various "Faunal Areas" are regarded as coincident with the river basins, and the lines of demarcation are accordingly marked along the watersheds. The areas of cultivated land, patches of woodland, bare moorland and other uncultivated land, with the deer forests, are all indicated by different colours. The areas above 1,000 and 2,000 ft. in elevation respectively are marked by special shading; and the depth of the sea is shown by various tints of blue. As the authors remark, much of their work is tentative and liable to modification by future research; but no stronger incentive to this important research could have been devised than the result of their painstaking labours now before us.

A Contribution to the Geology and Natural History of Nottinghamshire. Edited by J. W. Carr, M.A., F.G.S. Small 8vo. Pp. 90. Nottingham: James Bell, 1893. Price 2s. net.

This little work was specially prepared for the use of members of the British Association during the recent meeting, and is a valuable compendium of the Geology, Zoology, and Botany of the district of which it treats. Contributions to the geological section are made by Messrs. J. Shipman and R. M. Deeley, and the Rev. J. M. Mello; the notes on birds were written by Mr. F. B. Whitlock, and the list of mollusca was compiled by Mr. B. Sturges Dodd. The list and notes on the vascular plants are contributed by Mr. H. Fisher. Nottinghamshire is a rather monotonous county of cultivated land, almost entirely within the drainage area of the Trent; and the greater part of its surface is occupied by the New Red Sandstone. The fauna and flora, however, is far from meagre, and much still remains to be done in recording the distribution of many groups both of animals and plants.

A Biographical Index of British and Irish Botanists. By J. Britten and G. S. Boulger. 8vo. Pp. xvi., 188. London: West, Newman & Co., 1893. Price 5s.

We are glad to welcome the re-issue, in one volume, of Messrs. Britten and Boulger's Index of Botanical Biography. It has been promised for some time, but the authors assure us that the amount of corrections and amplifications necessary have justified the delay. The record has been carried down to the end of 1892, and the extent of the revision "may be gauged from the fact that, whereas the issue in the *Journal of Botany* comprised 1,619 names, occupying 148 pages, an average of over ten names to the page, in its present form our little book contains 1,825 names, and occupies 188 pages, an average of less than ten names to the page." Even had there been no additions we should have commended a re-issue, for a bibliography scattered through the pages of three volumes of the *Journal of Botany* compares unfavourably with a compact little volume like that now before us. As the title implies, the work is an index, "intended mainly as a guide to further information," and as such is admirable; references to the chief sources of further information being freely given.

The term botanist is held to include all who have in any way contributed to the literature of botany, who have made scientific collections of plants, or have otherwise assisted directly in the progress of botany, exclusive of pure horticulture. Mere patrons have, as a rule, not been included, or those known only as contributing small details to a local flora. The authors have done their work well and made a valuable contribution to botanical literature. Bibliography is an endless task and, doubtless, one might by toil find additions even to the long roll of 1,825 names, among which, by the way, we do not find that of Gilbert White, of Selborne, who surely might claim to be inserted equally with some of those whose names are included.

The book is nicely printed, and remarkably free from typographical errors.

A Descriptive Account of the Mammals of Borneo. By Charles Hose, F.R.G.S., F.Z.S. Pp. 78, with map and three plates. Diss, Norfolk: Edward Abbot, 1893.

The excellent exploring work which Mr. Charles Hose, one of the Residents in the service of the Rajah of Sarawak, has been doing in Borneo has been for some time known to most English scientific men, partly through the receipt by many different museums of zoological specimens of his collecting, and partly by the constantly-recurring descriptions in scientific periodicals of new vertebrates and invertebrates discovered by him in that wonderful island, the riches of which still seem so far from being exhausted.

During a recent visit to Europe, Mr. Hose has found time to compile, originally merely for his own and his friends' use in the jungle, the descriptions of all the mammals known to occur in the island; and at the request of some of his scientific friends he has added to the rough notes thus made some short original remarks drawn from his own knowledge of the species. This being the origin of the unpretentious little book above quoted, any detailed criticism of it would be out of place, and it should be accepted simply as the first rough basis on which, as we may hope, the author will found such a complete work as may be worthy of his intimate knowledge of the Bornean wild beasts and their ways.

The descriptions in this book are adapted almost or quite *verbatim* from the technical writings of recent workers on the subject, and mainly from Blanford's "Mammals of India," and Anderson's "Zoology of the Yunnan Expedition"; those of the species more recently discovered being, of course, taken from the original notices. Following the descriptions, Mr. Hose has given his own notes, which we hope will, in the future, be largely amplified, on the habits, comparative rarity, ranges, native names, and other such particulars of the animals as can only be learnt on the spot, and then only by an enthusiastic lover of the subject.

In all 144 species are recognised as inhabitants of Borneo, and of these about twenty of all sorts, from monkeys to shrews and mice, have been added to the fauna by Mr. Hose and his friend and co-worker, Thos. A. H. Everett, to whose advice and encouragement zoological science is largely indebted for the enrolment of such an ardent recruit as our author has proved himself to be.

Such work as Mr. Hose's is a type of what ought to be going on all over the world, wherever Englishmen with their national love of sport and natural history are living in the midst of a fauna as yet undisturbed by the onward march of civilisation. As European settlements and colonies gradually cover the earth the native fauna is inevitably killed out, either directly for sport or food, or indirectly by the introduction of domestic and parasitic animals. Now, and for the next twenty years, is, therefore, the time that our museums should be filled with carefully-preserved specimens of all sorts, so that our successors may have some chance of actually seeing examples of the animals which will soon no longer exist in nature. There can be no greater absurdity than the recent clamour made by certain "nature-lovers" in decrying the killing of specimens for collections, since such collecting can have not the slightest appreciable effect in exterminating species in comparison with many other processes now going on, notably the barbarous slaughter of birds for their feathers; a method by means of which millions of individuals are killed and their spoils thrown away after a year or two's administering to someone's vanity and thoughtlessness. How great is the destruction thus worked is to be gathered from the fact that the British Museum collection of birds, the largest in the world, does not consist of so many individual skins as have been sold for the purposes of fashion at a single day's sale at a City warehouse. But, protest as we may, the sad fact has to be faced that many of the most interesting and beautiful species now existing are doomed to destruction, and the liberal storage of specimens in museums for careful and long preservation appears to be the only means to give our successors the advantage we ourselves enjoy; and to decry such accumulations is a form of sentimental selfishness of which we hope no reader of NATURAL SCIENCE will be guilty. Such work as Mr. Hose's, therefore, combining, as it does, both observation and collection, is worthy of the highest commendation, and we may well wish him success in his future studies of the rich Bornean fauna. O. T.

THE AMPHIOXUS AND ITS DEVELOPMENT. By Dr. B. Hatschek. Translated and Edited by James Tuckey, M.A., Lecturer in the University of Durham. Pp. 181, with nine full page illustrations. London: Swan Sonnenschein & Co., 1893. Price 6s.

IT is difficult to know exactly what to say about this book. It is a bald translation of Dr. Hatschek's researches on Amphioxus with marvellously incapable reproductions of the beautiful figures. The excuse of the "translator and editor" for the book is that hitherto "these investigations have not been accessible to that portion of the scientific world which does not read German." It may be laid down dogmatically that that portion of the scientific world which does not read German has no business with original memoirs. The results of memoirs are collated and simplified for them in a multitude of excellent text-books, and the perusal of translations of isolated researches can but bloat them up with unassimilated materials, and give them a lopsided view of the animals they wish to study. Take this particular case. Dr. Hatschek has written more and discovered more about Amphioxus than any other investigator; but many others have written on it, and our total knowledge of Amphioxus is based on a very large number of original papers. About these others the "editor" says no word, and anyone judging by this book would conclude that he had exhausted the subject, whereas by a little laboratory work and a perusal of "Marshall and Hurst" or Parker's "Elementary Biology" he would have learned much more. The most astonishing thing about this publication is that it forms one of a series of "Introductory Science Text-Books," for original papers come at the end, not at the beginning of biological work.

Report on the European Methods of Oyster-culture. By Bashford Dean. Bull. U.S. Fish Commission for 1891. Pp. 357-406, pls. lxxv.-lxxxviii. 1893.

This report is drawn up on the author's personal observations made during a tour through Europe for the special purpose in 1891. Previous reports by Dr. Dean have been noticed in this Journal for Feb., 1893. Detailed descriptions of oyster-culture as carried on in Italy, Spain, Portugal, Germany, Holland, Belgium, and England are given, that of France forming the subject of one of the previous reports. Dr. Dean gives an account of the actual processes of oyster-culture, shows in a general way the influence exerted upon the industry by Government concessions or restrictions, and deals with a variety of questions relating to the living conditions of the oyster. The report is full of useful and practical remarks, is illustrated by photographs taken by the author, and we can only repeat what we said on the previous occasion, that it should be furnished to every grower around our coasts.

Ethnographie Nordost-Afrikas: Die materielle Cultur der Danâkil, Galla und Somâl. By Philipp Paulitschke. 8vo. Pp. 338, with 25 plates containing 100 figures and map. Berlin: 1893. Price 20 marks.

This volume commences with a geographical sketch of the district, notes on the peoples, the pure and mixed races, and then deals with their culture. Clothing, ornament, weapons, building construction, utensils, foods, physiology and hygiene, family and social life, are all taken in turn and fully dealt with; these are followed by chapters on economics, such as production and sale of valuables, imports, value of the natives and their work. The plates represent the natives themselves and their industries; and the map, on the scale of 1 : 4,000,000, shows the distribution of the various races.

Guide to the Siebengebirge. [Führer durch das Siebengebirge.] By B. Stürtz. Bonn: A. Henry, 1893.

Naturalists visiting the Rhine will welcome this valuable little guide-book as affording not merely the usual information for tourists, but also a concise and interesting notice of the Geology, Zoology, and Botany of the region immediately to the south of Bonn. It is written in popular style by Mr. Stürtz, the well-known dealer in geological specimens at Bonn, and is accompanied by a frontispiece giving a general view of the Siebengebirge. There are also lists of maps and works of reference.

A Classed and Annotated Bibliography of the Palæozoic Crustacea, 1698–1892, to which is added a Catalogue of North American Species. By Anthony W. Vogdes. Occasional Papers IV., California Academy of Science. 8vo. Pp. 416. San Francisco: June, 1893. Price 10s.

This valuable book, as its title indicates, is divided into parts. The bibliography (part 1) occupies 252 pages, and is followed by a catalogue of Trilobites (part 2), arranged firstly under the families, and secondly in alphabetical order of genera. The third part (pp. 361–414) treats of the non-trilobitic genera and species, which follow precisely the same convenient arrangement as do the Trilobites. The volume appears to be compiled in the author's usual careful manner.

The Clarendon Press announces the issue of part i. of Mr. B. D. Jackson's "Index Kewensis," a monumental index to the names (with authorities) of all known flowering plants, with an indication of their geographical distribution. The part comprises 728 pp., 4to, and is issued to subscribers at two guineas net. Sir Joseph Hooker, who has supervised the work, thus explains its origin :—"Shortly before his death, Mr. Darwin informed me of his intention to devote a considerable sum in aid or furtherance of some work of utility to biological science; and to provide for its completion, should this not be accomplished during his lifetime. He further informed me that the difficulties he had experienced in accurately designating the many plants which he had studied, and ascertaining their native countries, had suggested to him the compilation of an Index to the Names and Authorities of all known Flowering Plants and their Countries, as a work of supreme importance to Students of Systematic and Geographical Botany, and to Horticulturists, and as a fitting object of the fufilment of his intentions. I have only to add that, at his request, I undertook to direct and supervise such a work; and that it is being carried out at the Herbarium of the Royal Gardens, Kew, with the aid of the staff of that establishment."

The third part of the new edition of the Molluscan portion of "Dr. H. G. Bronn's Klassen und Ordnungen des Thier-Reichs" is just out, and permits one to judge of its merits. So far the work is in every way a worthy successor to the previous edition, and the fact that it is being brought out under the able editorship of Dr. Simroth, of Leipsic, is a guarantee that this standard of excellence will be maintained to the end.

The first two lieferungen, issued last year, were devoted to an historical summary; the new part, which ranks as lief. 3 to 6, contains a general bibliography and the beginning of the Aplacophora, with the first four plates.

By some curious oversight, while many of the minor papers on molluscan adults are duly noted in the bibliography, the standard work by Troschel, "Das Gebiss der Schnecken," has been omitted.

The treatment of the subject matter of the work, as evinced by the portion before us, leaves little, if anything, to be desired, and the plates are most excellent. It is to be hoped that the work will not take as long to complete as some other Teutonic productions that could be named.

Mr. H. A. Pilsbry has completed and issued the last part of vol. xiv. of Tryon's "Manual of Conchology." The volume is illustrated with 68 plates, and is prefaced by a general account of the structure and classification of the Chitons. An index to these molluscs will appear with the next part, which completes their description.

The last part (fasc. v.) of Professor A. de Lapparent's "Traité de Géologie" (pp. 1281 to end) has been issued by F. Savy, Paris. Messrs. Macmillan & Co. announce the preparation of a third edition of Sir Archibald Geikie's well-known Text-book.

Messrs. W. H. Allen & Co. will shortly publish a new edition of "Our Reptiles and Batrachians," by Mr. M. C. Cooke, which has long been out of print. The work has been revised and corrected to date by the author with some additional matter. The coloured plates are reproduced in chromolithography.

The same publishers have also nearly ready for publication a "Handbook of British Hepaticæ," by Mr. Cooke, with introduction and full description of all the genera and species hitherto found in the British Isles, illustrated by 200 woodcuts and seven plates. The work will be the only complete guide to the subjects which has been published in this country for upwards of a quarter of a century.

OBITUARY.

EDUARD SCHNITZER.

BORN MARCH 28, 1840. DIED DECEMBER, 1892.

NEWS has been received during the past month of the sad murder of Dr. Eduard Schnitzer, better known as Emin Pasha. The event took place at the close of last year.

Eduard Schnitzer was born on the 28th of March, 1840, in Oppeln, in the Prussian province of Silesia. He was the son of Ludwig Schnitzer and Pauline his wife. The family removed to Neisse in 1842. After being educated in the Gymnasium of Neisse, Eduard commenced the study of medicine at Breslau in 1858. He completed his studies in Berlin, where he attended the University during 1863–1864 and graduated.

A strong desire for travel led the young doctor to look for a sphere of work in a foreign land, and leaving Berlin at the end of 1864, he went to seek a practice in Turkey. Chance led him to Antivari and Scutari, where he obtained the confidence of the Vali Mushir Divitji Ismail Hakki Pasha, from whom he received a post on his staff, and whom he accompanied on his official journeys throughout the various provinces of the extensive district under his jurisdiction. In this way Schnitzer became acquainted with Armenia, Syria, and Arabia, and at length Constantinople, where Hakki Pasha died in 1873.

In 1875, Schnitzer returned to Neisse, devoting his leisure to Natural History. Suddenly he started off for Egypt, and in 1876 we find him entering the Egyptian service as Dr. Emin Effendi. He was ordered to join the Governor-General of the Soudan at Khartoum, and from there was sent to act as chief medical officer in the Equatorial Provinces, of which Gordon was the Governor.

In 1878, on Gordon accepting the Governor-Generalship of the Soudan, he appointed Emin Governor of the Equatorial Provinces. Immediately setting to work to restore matters, his success was unbounded, and by 1879, Felkin says, a most wonderful change had taken place. "Stations had been rebuilt, discontent was changed to loyal obedience, corruption had been put down, taxation was equalised, and he had already begun the task of clearing his province from the slave-traders who infested it." All this, with the exception of a few months' help from Lupton

Bey, he did entirely alone and unassisted. He received the title of Bey in 1879, and by the end of 1882 he was able to report that not only was his province at peace and contented, but that he had entirely banished the slave-traders from his borders. He was also able to show a profit of £8,000 in that year, whereas when he took up the work there was a deficit of £32,000 per annum.

The reader is referred to Felkin's life of Emin, which forms the Introduction to " Emin Pasha in Central Africa," London, 1888, from which much of our information is taken.

EDWARD CHARLESWORTH.

A REMARKABLE man in many respects was Mr. Charlesworth, whose death, at an advanced age, took place recently at Saffron Walden. Educated at Guy's Hospital, he gained a good general knowledge of Comparative Anatomy, and while still a student, he came prominently into notice by the publication in 1835 of a masterly paper on the "Crags of East Anglia." He then pointed out that the Crag of Suffolk was divisible into two portions, termed respectively the Coralline and the Red Crags. Those divisions were accepted by Lyell, and their names have become permanently established. Later on he pointed out that in Norfolk a newer division occurred, and this he named the Mammaliferous Crag, now generally known as the Norwich Crag.

In 1836, Charlesworth was temporarily employed in the British Museum, but, having busied himself with the invention of an "elevator gun," he soon retired to join an expedition to Mexico.

In 1837, he succeeded Loudon as Editor of the *Magazine of Natural History*. He commenced a new series and edited four volumes, terminating his connection in 1840, when the *Magazine* was united with the *Annals of Natural History*, that had been started two years previously.

About the year 1843, Charlesworth planned the publication of the *London Geological Journal*, but the first number did not appear until September, 1846, owing chiefly to the fact that in 1844 he was appointed Curator of the York Museum. The *Journal* was profusely illustrated with plates, and it contained valuable contributions from the leading palæontologists of the day. Charlesworth himself gave an account of the occurrence of flint in the pulp-cavity of a tooth of *Mosasaurus*. Three numbers only of this *Journal* were published, the last being issued in May, 1847.

In 1858, Charlesworth gave up his Curatorship at York, where he was succeeded by the late W. S. Dallas, and eventually settled for a time in London. From this date, he appears to have made a somewhat precarious livelihood, chiefly by the sale of specimens. He formed the "British Natural History Society," which consisted only of himself, and undertook the disposal more especially of the Tertiary and Recent Mollusca. He prided himself also on being the first to introduce glass-topped boxes for the preservation of delicate specimens. He would appear occasionally at a scientific meeting, when there was a discussion on flints, in which he took an especial interest; and for several years he successively attended the anniversary meetings of the Geological Society, then held at Somerset House, and argued at length, with great fluency, and in loud tones, about the management of the Society's affairs. While his speeches contained, at times, matter well deserving of discussion, his manner too often was aggressive and needlessly offensive; and this want of tact was displayed in some of the critical remarks he introduced into the journals he edited. From these and other causes Charlesworth came gradually to lose position and friends. Starting, as he did, so full of promise, with ability of a high order and much enthusiasm, it is sad to think how in later years he neglected his talents and misused his opportunities, for the work of his early years, though not great, is sufficiently important to form an enduring contribution to the literature of geology.

LEONARD BLOMEFIELD (JENYNS).

Born 1800. Died September 1, 1893.

THE last of Darwin's associates in the preparation of "The Zoology of the Voyage of H.M.S. 'Beagle,'" has passed away in the person of the Rev. Leonard Blomefield (formerly Jenyns), who contributed the section on the fishes. He was the youngest son of the late Rev. George Leonard Jenyns, of Bottisham Hall, Cambridgeshire, and for many years had resided at Bath. He graduated at Cambridge, and his first paper on the Ornithology of Cambridgeshire, was read before the Cambridge Philosophical Society in 1825. Between that year and 1833 he also contributed to the Transactions of the same Society papers on the reptiles of Cambridgeshire, the habits of the natterjack toad, a mite parasitic on slugs, a swarm of flies, and on the British species of *Cyclas* and *Pisidium*. In 1834 he presented a report on "The Recent Progress and Present State of Zoology" to the British Association, and in the following year he published at Cambridge his well-known "Manual of British Vertebrate Animals." Eleven years later he also published another small work, entitled "Observations in Natural History: with an Introduction on habits of observing, as connected with the study of that Science. Also a Calendar of Periodic Phenomena in Natural History." He was especially devoted to field observations, and during the last fifty years made many contributions to scientific literature, his last being a Presidential Address to the Bath Natural History and Antiquarian Field Club in 1892. Both in this Club and in the Bath Institution, Mr. Blomefield took the deepest interest, and the Institution is indebted to him for a fine scientific library of nearly 2,000 volumes, besides a good herbarium.

ALEXANDER STRAUCH.

THE death is announced of the eminent herpetologist, who has for many years been Director of the Museum of the Imperial Academy of Sciences at St. Petersburg. Though only in his sixty-second year, Dr. Strauch had been in feeble health for some time before his death, and had been unable to make much progress in his favourite studies. His more important memoirs were published by the Academy, and comprise, among others, an essay on the Herpetology of Algeria (1862), studies of Chelonia (1862, 1865, and 1890), a synopsis of the Crocodilia (1866), a synopsis of the Viperidæ (1869), a revision of the Salamanders (1870), on the Snakes of Russia (1873), and on the Geckos (1887). Dr. Strauch's memoir on the Russian snakes is especially valuable, and it is a misfortune that he was unable to complete similar memoirs on the other reptiles and batrachians of the empire. His contemplated description of the reptiles collected by Przewalski remains unfinished.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Professor C. H. Tyler Townsend, of Las Cruces, New Mexico, has been appointed Curator of the Museum and Institute, Kingston, Jamaica.

Mr. L. J. Spencer, B.A., of Sidney Sussex College, Cambridge, is the successful candidate in the recent examination for the vacant Assistantship in the Mineralogical Department of the British Museum. Mr. Spencer was awarded the Harkness Scholarship last June, and before entering upon his appointment will spend the remainder of this year in visiting the Museums and Universities of the Continent.

The Scientific Library of the late Professor J. S. Newberry, has been present ed as a memorial to the Geological Department of Columbia College, New York.

A valuable concise report on the Marine Biological Laboratories of Europe, by Dr. Bashford Dean, appears in the July and August numbers of the *American Naturalist*.

According to *Indian Engineering*, a wealthy citizen of Calcutta has offered a sum of money for the purpose of building a library in the Zoological Gardens of that city. The new laboratory at the Gardens is completed, and will shortly be ready for use.

The first of a series of Stratigraphical Memoirs, published by the Geological Survey, was on the Pliocene Deposits of Britain, by Mr. Clement Reid, and was issued in 1890. Now a further instalment has been published, in two volumes, on the Jurassic Rocks of Yorkshire, by Mr. C. Fox-Strangways. These works summarise our knowledge on the various formations and their subdivisions, and they contain full lists of the fossils.

The Annual Report of the Madras Museum, 1892–93, records, among other additions, the mounting of a stuffed specimen of the rare shark, *Rhinodon typicus*, cast ashore at Madras in 1889. A figure is given. During the year the Superintendent has visited the corundun deposits, and collected a series of Cretaceous fossils near Pondicherry. A collection of ammonites has been sent to Vienna, for study by Dr. W. Waagen.

The Annual Report of the British Museum for 1892 has been issued. The Director of the Natural History Departments states that the building of a temporary room for the accommodation of the Cetacea is under contemplation ; and special allusion is made to the inconvenience caused by the want of a lecture room. The Swiney Lectures on Geology, by Professor Nicholson, will be delivered this year in the Lecture Theatre of the neighbouring South Kensington Museum. The subject is the Bearing of Geology on the Geographical Distribution of Plants and Animals, and the course commences on October 2.

A beautiful new example of protective resemblance in animals has been added to the collection in the hall of the Natural History Branch of the British Museum. It is a beetle (*Lithinus nigrocristatus*) from Madagascar, living on lichen, and adorned in such a manner as to precisely resemble the latter. A very fine collection of madreporarian corals from Western Australia has just been sent to the Zoological Department by Mr. W. Saville Kent, and a nearly complete skeleton of the extinct New Zealand rail, *Aptornis*, has been added to the exhibited collection in the Geological Department.

The political disturbances in Bohemia are seriously retarding the arrangement of the Natural History collections in the new Royal Bohemian Museum at Prague. The annual grant of money this year has been greatly reduced. The cases for the Barrande collection of fossils are still unfinished, and probably will not be ready until next spring; while there is little prospect of the completion of the new cases for the other fossils within the next two years. Considering the lavish expenditure on the architectural features of the Museum, this niggardliness in providing fittings is much to be deplored. For the student of Palæozoic fossils, the Bohemian collection is perhaps the most important in Europe, and any delay in rendering it of service is a great misfortune.

The Proceedings of the meeting of the German Anatomical Society, held last May, have just been issued as a supplement to the *Anatomischer Anzeiger*, forming a small volume of 224 pages.

The Norfolk and Norwich Naturalists' Society has established a "Yarmouth Section" at Great Yarmouth, with the Rev. C. J. Lucas as Chairman, and Mr. Arthur Patterson as Hon. Sec.

The Haslemere Natural History Society is interesting itself in the lectures on Technical Education provided by the Surrey County Council. It has organised a "Teaching Committee" to supplement, by class work, the ordinary lectures.

The ninth part of vol. i. of the *Journal of the Trinidad Field Naturalists' Club* has been issued. Besides the reports of meetings, it contains papers on Composite Plants, Mosquitoes, Ticks on an Iguana, etc. Mr. F. W. Urich's account of the mosquito pest is of special interest, and the Club is doing wisely in restricting its operations to local research. Several members are occupied in collecting the mammals of Trinidad, in preparation for a forthcoming work on the subject by Mr. O. Thomas, of the British Museum.

Timehri, the journal of the Royal Agricultural Society of British Guiana, is nearly always the most interesting of Colonial publications. The latest part (vol. vii., pt. i.) is especially readable, and contains some valuable contributions. Mr. James Rodway, the editor, discusses the seasons in Guiana; an account of the Indians of Guiana is translated from an old Dutch work, published in 1770; Mr. H. I. Perkins contributes notes on a journey to the Cuyuni Gold-Mining District; and Mr. H. C. Swan records his experiences of insect-collecting in British Guiana.

The newly-issued part of the *Proceedings of the Liverpool Geological Society* (vol. vii., pt. ii.) contains Mr. W. Hewitt's Presidential Address on the New Red Sandstone with reference to its mode of formation, and several other papers of much interest. Mr. Mellard Reade gives an elaborate and well-illustrated account of the Glacial Deposits of North Wales, discussing their possible origin; and Dr. C. Ricketts treats of the conditions under which the older Carboniferous rocks of N.W. England were formed. Messrs. P. Holland and E. Dickson discuss the formation of clay, and there are some descriptive papers on local geology.

The recent meeting of the British Association at Nottingham (Sept. 13 to 20), though receiving scarcely any communications of absolute novelty, was full of interest; and the arrangements of the local committee were all that could be desired. The attendance, also, though not remarkably large, was at least equal to that of most recent meetings. In addition to the Presidential Addresses, which we notice elsewhere, there were several interesting discussions to attract widespread attention, notably the debates on the nature of life (opened by Dr. J. S. Haldane), on coral reefs (opened by Professor Sollas), and on the place of geology in education (led by Professors Cole and Lebour). A more futile discussion than the attempt to settle the limits of Geology and Geography, we do not remember to have heard.

The section for Geology seems to have been favoured with the greatest number of papers, and the Petrologists, as might have been expected, mustered in considerable force. There were several contributions to local geology, Mr. A. T. Metcalfe's description of the gypsum deposits being the most elaborate. Professor Brögger (of Christiania) discussed some eruptive rocks in Norway, and Professor Iddings (of Chicago) described a dissected old volcano in Wyoming. Professor Johnston Lavis presented his usual report on Vesuvius, and Professor Sollas explained the origin of intermediate varieties of igneous rocks by intrusion and admixture. Mr. Walcot Gibson gave a general sketch of the geology of Central East Africa, and Mr. R. D. Oldham exhibited two new geological maps of India. Dr. H. Hicks reiterated his views on the base of the Cambrian Formation; and Mr. Goodchild showed how the "eyes" in gneiss give a clue to its origin. In Glacial Geology, Prince Kropotkin attempted the widest theme in contributing a paper on the glaciation of Asia; Professor Sollas exhibited a map of the eskers of Ireland; Mr. De Rance traced the pre-Glacial form of the ground in Lancashire and Cheshire; and Messrs. Abbott and Kendall had some more remarks on the "heretics" who believe that North Wales was submerged to a great extent in the Glacial Period. Professor Bonney and Mr. Lindvall also spoke on theoretical matters in Glacial Geology. In Palæontology, Mr. E. T. Newton gave the latest information about his Triassic reptiles from Elgin; and Dr. Traquair recorded a cephalaspidian fish from Caithness. In addition to the papers, the usual reports on Underground Waters, Erratic Blocks, Photographs, Palæontology, etc., were presented.

The Biological Section had to lament the absence of the President on account of ill-health. Among the more fundamental questions, Professor J. B. Farmer treated of some new features in the division of the nucleus in plant-cells, and Messrs. Hartog and Dixon discussed the digestive ferments of a large protozoon. Messrs. Cattle and Millar read a paper on Gregarines and the possible connection of allied forms with tissue-changes in man, notably with the production of cancer. Mr. W. E. Hoyle described the luminous organs of Cephalopoda, and Mr. F. T. Mott discussed the origin of organic colour. Mr. F. Enock spoke on insect parasites, and Mr. G. B. Rothera on vegetal galls. Mr. W. S. Bruce gave an account of the seals and whales seen during his recent voyage to the Antarctic regions, and Dr. C. H. Hurst made the observations on birds' wings which we print elsewhere. The most generally interesting botanical paper was Miss N. Layard's description of the root of the duckweed (*Lemna*). Mr. J. Clark treated of lime in its relation to some physiological processes in the plant, and Mr. H. Brown had a paper on starch. There were numerous interesting reports of committees, that dealing with the legislative protection of the nests of wild birds leading to the warmest discussion.

The Geographers were treated to some entertaining exhibitions of lantern slides, Mr. W. M. Conway bringing his views of the Karakoram Mts., and Professor John Milne showing some pictures of Japan with especial reference to earthquakes. Mr. Murdock also exhibited his paintings lately made in the Antarctic regions. Besides numerous addresses by travellers, there were papers by Mr. J. Y. Buchanan on the influence of land on the temperature of the air; by Mr. H. N. Dickson on the sea between Scotland and the Faroe Isles; and by Dr. H. R. Mill on the Clyde Sea Area and on the English Lakes.

The Anthropologists began with Mrs. Grove's paper on the Ethnographic Aspect of Dancing, and had numerous interesting discussions both on Ethnographical and Antiquarian subjects. The event of the meeting seems to have been Mr. A. Bulleid's description of the ancient British village recently explored by him near Glastonbury. Mr. E. W. Brabrook communicated the first report of the committee for undertaking an ethnographic survey of the British Isles; and Professor Hans Hildebrand, of Stockholm, discussed Scandinavian antiquities. Dr. J. H. Gladstone also started a somewhat indecisive discussion on the possibility of recognising a copper age between the ages of stone and bronze.

The General Committee of the British Association awarded grants of money for investigations in Natural Science as follows:—Erratic blocks, £15; Fossil Phyllopoda, £5; Geological photographs, £10; Shell-bearing deposits, £5; Eurypterids, £5; New sections of Stonesfield Slate, £25; Earth tremors, £50; Exploration of Calf Hole Cave, £5; Table at Naples Zoological Station, £100, and at Plymouth Station, £15; Zoology of Sandwich Islands, £100; Zoology of Irish Sea, £40; Mammalian heart, £10; Climatology and hydrography of Tropical Africa, £10; Observations in South Georgia, £50; Exploration in Arabia, £30; Anthropometric Laboratory statistics, £5; Ethnographical survey of United Kingdom, £10; The Lake Village at Glastonbury, £40; Anthropometrical measurements in schools, £5; Mental and physical condition of children, £20; Corresponding societies, £25. The total amount expended in grants was £705.

The British Association meets next year at Oxford early in August, under the presidency of the Marquis of Salisbury, Chancellor of the University. A new section for Physiology will then be inaugurated. The invitation of the town of Ipswich has been accepted for 1895.

The nomination of the Marquis of Salisbury for the Presidency, which has met with general approval, was proposed by Sir Frederick Bramwell and seconded by Sir William Flower. It is rarely that one sees gratitude for past favours so tactfully expressed as in the remarks of Sir William Flower, reported in the *Nottingham Daily Guardian* of September 19. Sir William said: "Lord Salisbury had shown general sympathy with all branches of science for a long time, and he was the first Prime Minister to recognise that men of science might sometimes aspire to honours and distinctions which had hitherto been reserved for successful soldiers or barristers and others. He had made a scientific man on his own merits a member of the peerage, and had appointed another purely scientific man a member of the Privy Council."

The Toynbee Hall Natural History Society, Whitechapel, is still actively pursuing its good work in the East End of London. During the winter, meetings are regularly held at the Hall, and during the summer cheap excursions, of long and short duration, are organised to various districts of interest. The longest excursion of the past season was a visit of fifteen days to the island of Jersey. The party, numbering seventeen, started on Friday, July 28, and returned Saturday, August 12. The headquarters during the whole of the time were at Gorey, on the S.E. coast of the island. Those taking part in the expedition represented three sections—Botany, Geology, and Zoology. For the purposes of systematic work, a portion of the island was allotted to each day, so that the whole of the coast and much of the interior was covered during the visit.

The effects of the drought in Jersey were not so evident as they are in England, and most of the plants for which the Channel Islands are noted were secured for the Society's herbarium. Many of them were in great abundance, and included—*Mathiola sinuata*, R. Br.; *Polycarpon tetraphyllum*, L.; *Lythrum Hyssopifolia*, L.; *Hypericum linarifolium*, Vahl.; *Gnaphalium luteoalbum*, L.; *Centaurea aspera*, L.; *Armeria plantaginea*, Willd.; *Microcala filiformis*, Link.; *Echium plantagineum*, L.; *Scrophularia Scorodonia*, L.; *Sibthorpia europæa*, L.; *Scilla autumnalis*, L.; *Cyperus longus*, L.; *Scirpus pungens*, Vahl.; *Fibichia umbellata*, Koch.; *Bromus maximus*, Wesf., and *Briza minor*, L.

It is worthy of note that in the north-east part of the island, at least, *Hypericum humifusum*, L., could not be found, but the form *H. decumbens*, Petermann, was abundant on the roadsides. It is, therefore, not very evident that *H. decumbens* is a hybrid between *H. humifusum* and *H. linarifolium*.

The ferns were not good or abundant except in the north-west. Those members inclined for shore work received valuable aid respecting the best hunting grounds, etc., from Mr. Sinel.

The Geologists were able to collect specimens of nearly all the rocks described from the island, including very fine examples of the Pyromerides of Bouley Bay. Owing to the complexity of structure of the island, the relations of the rocks to one another were in many cases not well made out, but the intrusion of the Granite series into the Diorite was well seen on the north. The great series of dykes were examined all along the coast, and the results of weathering, in the formation of hollows and caves, where they cut the harder granites, attracted general attention. A good series of rocks was secured for the Society's museum, and the micro slides, which are in course of preparation, will be exhibited at one of the winter meetings.

For the last two years, two long excursions, in addition to the usual Saturday and Sunday excursions, have been successfully carried out by Toynbee Hall. The plan of camping has been found to answer well, since the expenses are small; and this is a point of considerable importance, for one great object of the Society is to bring these Natural History outings within the reach of a large number. In the early summer of this year a camp was pitched for eight days at Thursley, Godalming, within a short distance of Hind Head. The cost for eight days' camp, including railway fare, was 24s., and the cost per member for a fortnight in Jersey, including return ticket, was 68s. The Society dispensed with tents in Jersey, as a farm-house was found sufficiently large to accommodate the whole party.

CORRESPONDENCE.

Phylogeny and Ontogeny.

I regret that Mr. Bather should have troubled himself to reply to me while he had not access to my original paper, and while he was subject to the inevitable distractions and inconveniences of travelling. Under such conditions it was probably impossible for him to avoid inadvertent misrepresentation.

I did not "deny that the past history of its race has *any* influence on the growth-stages of an individual," but only "that the phylogeny can *so* control the ontogeny as to make the latter into a record of the former—even into an imperfect record of it." This, however, is probably what Mr. Bather meant. But there is a very real misrepresentation two or three sentences further down on the same page (238). I have never "maintained that the development of any [=every] individual was a regular progress from the embryo to the adult." Not only am I pretty familiar with Weismann's work on the Diptera, but I have myself worked out in some detail the development of *Culex*, in which the alimentary, respiratory, muscular, and nervous systems and, to a smaller extent, other systems and organs also, exhibit phenomena (histolysis, for instance) which could not by any stretch of the imagination be called "regular progress from embryo to adult." The phenomenon called "metamorphosis" is too familiar for any zoologist to make any such statement as that ascribed to me.

I have already answered Mr. Bather's question elsewhere, but as he says "it is needless to reply" to my "other remarks while the above question remains unanswered," I will answer it again; merely premising that he has so far not replied to any of *my* "remarks" on the subject under consideration, but only to "remarks" which he has mistakenly ascribed to me.

His question is this:—"What cause can have produced these deviations of ontogeny from the path of simple development?" the deviations in question being those which he described in Natural Science, vol. ii., pp. 275 *et seqq*. My answer is that the species have so varied under the guidance of Natural Selection that the *later* stages of development have come to differ more widely from the *corresponding* stages of the ancestors than the *early* stages have come to differ from the *corresponding* stages of the same ancestors. The *vestiges*—I fully admit that they *are* vestiges—still to be found in the embryos of the existing species in the case of *Antedon*, are surviving remnants of the rudiments which existed in the ancestral embryos. They are so modified that, though still recognisable, they do not now any longer follow the same course of development as they did in those ancestors. Mr. Bather will remember that this explanation was given with reference to the "gill-arches" of embryonic birds and mammals (vol. ii., p. 198).

As the *Philosophical Transactions* and the *Annals and Magazine of Natural History* are practically inaccessible to a large proportion of the readers of Natural Science, I will again express the hope that Mr. Bather will take an early opportunity of putting the more interesting and tangible results obtained by Dr. Carpenter and himself before those readers, with such figures as may put them beyond the possibility of being misunderstood. (*Cf.* Mr. Smith Woodward's article on "The Fore-runners of the Back-boned Animals," vol. i., p. 596). I feel sure that by so doing he will facilitate the solution of our present problem.

C. Herbert Hurst.

Manchester, Sept. 10, 1893.

[We shall welcome any such concise statement of facts as is suggested by Dr. Hurst; otherwise this correspondence must now cease.—Ed.]

The Moas of New Zealand.

Natural Science has honoured me with two reviews of my paper on "The Moas of New Zealand," one in October, 1892, by Mr. R. Lydekker, the other in May, 1893, by Mr. H. O. Forbes, and by these I am gratified. I hope, however, I may be allowed a little space for a few additional remarks, because, in the first review, Mr. Lydekker has, unintentionally, I am sure, misrepresented my opinion on an important point; while in the second review there are two misstatements of facts which I ought not to leave unnoticed.

Mr. Lydekker in his criticism (Nat. Sci., vol. i., p. 594) says that as I admit that the Moas probably passed from New Zealand to Australia during the Pliocene (? Pleistocene) period without the passage of any mammalia in the opposite direction, my arguments as to the impossibility of flightless birds, as such, having reached Australia and New Zealand at an earlier period, are by no means convincing. But in my paper I say that the migration from New Zealand to Australia could not have been later than the Eocene period (*Trans. N. Z. Inst.*, vol. xxiv., p. 147); and I point out how, by an isolation of a part of the New Zealand area and its subsequent connection with Australia, this latter country could have been inoculated with Struthious birds without any possibility of mammalia passing in the opposite direction. But it would be highly improbable to suppose that on a part of Australia the Struthious birds had been isolated from the mammalia and then transferred alone to New Zealand. The two cases are quite different. No doubt a little further on in my paper I say that *if Mr. De Vis is right* in referring a femur from Darling Downs to Dinornis, *then* we should have to allow a second migration from New Zealand to Australia in the Pliocene; and this may have misled Mr. Lydekker. But it is, I think, evident that I was very doubtful about this, and now, having examined a cast of the Queensland bone, I find that it differs considerably from any genus belonging to the Dinornithidæ, and resembles more closely the femur of the Emu and Cassowary, especially that of the young Emu,[1] so that there is no longer any reason for supposing a late migration of Struthious birds between New Zealand and Australia.

Turning now to the second review. In my paper I have a footnote regretting that I had not been able to see Mr. Lydekker's "Catalogue of the Fossil Birds in the British Museum," of which "no copy has as yet been received in Christchurch" (*l.c.*, p. 98). Nevertheless Mr. Forbes says, "It is greatly to be regretted by all workers on this most difficult subject that Mr. Hutton did not defer the publication of this valuable paper, in which has been brought together almost all the known information on the Moa, till he had found time to compare his nomenclature with that of Mr. Lydekker's catalogue—a volume which had already reached the Colony before the reading of his paper" (Nat. Sci, vol. ii., p. 377).

Now the first part of my paper "On the Classification of the Moas" was read on October 1, 1891, and the second part, "On the History of the Moa," on November 4, 1891 (both dates appearing on the face of the published paper); while in the books of the Christchurch Museum I find an entry, *in Mr. Forbes's own handwriting*, that Lydekker's "Catalogue of the Fossil Birds in the British Museum" was received on November 18, 1891; and as this is the first copy that reached Christchurch, it is evident that both parts of my paper had been read, and had left my hands some time before the book was received here. As a matter of fact, I was not aware, when I read the first part of my paper, that Mr. Lydekker had been working at the same subject. I saw a notice of his book a few days afterwards, and went to the Museum to ask Mr. Forbes, who was then curator, if he had got it; and on his replying in the negative, I added the footnote to my paper. I mention this because I do not wish Mr. Lydekker to think that I treated in so cavalier a fashion his very valuable work, from which I have learnt a great deal. I could, of course, have written to Wellington and asked leave to withdraw my paper, but it did not seem to me worth while to do this, because none of the new species described by me are identical with those described by Mr. Lydekker, and the only changes I could

[1] On *Dinornis* (?) *Queenslandiæ*, by Captain F. W. Hutton, F.R.S., in *Proc. Linn. Soc. N. S. Wales* [2], vol viii. (1893), pp. 7–11.

have made were a few in the genera, owing to Mr. Lydekker having convinced me that I had put too much reliance on Sir R. Owen's plates.

The second point in Mr. Forbes's criticism to which I wish to refer relates to the age of th ebeds in which *Anomalopteryx antiqua* was found. Mr. Forbes says (Nat. Sci., vol. ii., p. 378), "I shall, however, most willingly admit that I am mistaken as to the age of the strata in which these *Dinornis* remains have been found as being other than "newer Pliocene, or even Pleistocene," so soon as the officers of the New Zealand Geological Survey—who are really the only competent referees in the case—shall have assigned to these gravels a different age." But it was the officers of the Geological Survey whom Mr. Forbes contradicted. It was Sir Julius von Haast, Provincial Geologist of Canterbury, who referred the Timaru lava stream, which overlies the gravels, to the Eocene; and it was Mr. A. McKay, Assistant Geologist to the Geological Survey of New Zealand, who said that these beds are of Miocene age. All that I did in the matter was to point out that Mr. Forbes differed from the officers of the Geological Survey without examining the evidence on which their opinions were founded.

Canterbury Museum, Christchurch, N.Z. F. W. Hutton.
25 June, 1893.

I have to thank the Editor for the perusal of the above observations of Mr. Hutton. In regard to the date of the arrival in New Zealand of Mr. Lydekker's "Catalogue of Fossil Birds in the British Museum," I beg to say that the date entered in the Museum Additions Book does not *necessarily* indicate the exact date of its receipt. As Mr. Hutton says that he came to me for the volume after the reading of the *first* part of his paper, I leave the question there. I handed him Mr. Lydekker's volume, however, immediately on its reaching my hands, *within a day or two* of his asking me for it. At all events, the point I raised remains unaltered, that as Mr. Hutton's paper could not be published for at least five or six months after it was read, and since Lydekker's classification and the descriptions of his new species were in Mr. Hutton's hands before he corrected his proofs, he ought not to have added to the confusion already existing in the classification and synonymy of the Dinornithidæ.

In regard to the second point, I quoted in my paper the latest opinion of the Director of the Geological Survey, which I have now before me, and if I recollect aright the opinion verbally expressed to me by Mr. A. McKay on the last occasion on which he visited me at the Museum.

Henry O. Forbes.

On *Anomalopteryx antiqua*, Hutton, and other New Species of Moa from Enfield, New Zealand.

In the *Transactions of the New Zealand Institute*, vol. xxv. (1892), just received, Mr. Hutton gives a further account of this species founded by him in the previous volume on fragments of a tibia embedded in two blocks of laterite found near Timaru, N.Z., and on two other fragments of a metatarsus he had seen in a photograph, which he assumes to belong to the same species. The photograph is reproduced on plate xvii. of volume xxiv. The species is founded on the tibial fragments marked *a* and *b*; and on those of the metatarsus lettered *c* and *d*. In the same volume, page 125, he states the length of the latter to be "5·5 ? inches," and of the former to be "12 inches." In the new volume he now, on plate iv., figures the two metatarsal fragments from their casts made by me, which he had previously overlooked in the Christchurch Museum. The interest of the specimens rests in their being the oldest—Newer Pliocene or Pleistocene—known portions of *Dinornis*; but the value of *Anomalopteryx antiqua* as a species, and necessarily the value of the deductions drawn from these fragments of bone, will be apparent when I state that fragment *b* of the tibia was obtained at one time (the first find) and the fragment *a* on a subsequent occasion (the third find), and that they came from different "drives" made for blasting purposes, under distant parts of the dolerite sheet. In like manner, the fragments *c* and *d* also were found, as I personally know, in different drives far apart from each other. The chances, therefore, of these fragments all belonging to the same bird are infinitely little.

Each may represent a different species, or even a different genus. Mr. Hutton has, however, boldly deduced the length of the *tibia* of his new species from the one set of fragments, and its metatarsus from the other set. On this compound structure he has built conclusions as to the early forms of *Dinornis*, and as to the pedigree of others. The author observes, on p. 16, " It will be seen that my inference that the Moa bones from Timaru belonged probably to the genus *Anomalopteryx* was a correct one."

If we now turn to his own diagnosis of the genus (tom. cit , xxiv., p. 123) we find it to be based on certain characters of the skull, sternum, and pelvis—bones which, in the present species, are all unknown ; and on the metatarsus being shorter than the femur (here also undiscovered), and on its length being 2·0–2·3 times the girth of the shaft, which again is lost in both fragments. *Anomalopteryx* is further defined by its tibia being " 2·1 to 2·2 times the length of the metatarsus, and its breadth between 4·2–5·1 times the girth." As already pointed out, the tibial fragments are " much hidden in the matrix," and imperfect at both ends, and Mr. Hutton now admits that " the length of the metatarsus is unknown." There is left, therefore, not a single character by which these fragments can be referred to *Anomalopteryx*, as defined by him, or to any other genus or species.

Of the additional species described in this volume as new, nearly all are based on bones exhumed at Enfield, and examined with considerable care by myself. When arranged in a series of several yards in length, the one bone merged into its neighbour so gradually that it was quite impossible to draw any line and say the bones to the right ought to belong to one species, those on the left to another. The bones associated together by Mr. Hutton to form a limb are purely guess-work, and may or may not belong to one species (not to say individual) and the comparative dimensions of its different parts can have only conjectural value. Indeed, most of the separate bones might easily be fitted to different existing species, for it is well-known that the bones of birds of ancient pedigree vary greatly in their dimensions. From descriptions such as are given by Mr. Hutton, unaccompanied by figures, it is impossible to identify the species of *Dinornis* he has established. They merely increase the already almost inextricable confusion which those who are working on this subject are compelled laboriously and unnecessarily to unravel.

HENRY O. FORBES.

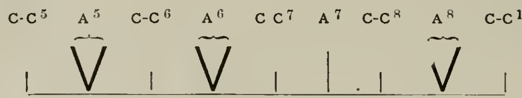
LATENT CONGENITAL VARIATION IN A LUCERNARIAN ?

IT seems to be the fact that the Lucernarians examined by Mr. Hornell, and described in his note in the July number, were abnormal when gathered. On January 6 of this year I received from Messrs. Sinel & Hornell two specimens of *Haliclystus octoradiatus*, normal as to the number of arms, c-cystophores, and septa. They were put into a tank in good order, and containing a fair supply of food in the shape of small crustacea. On the 8th, no abnormality was perceived, and illness prevented my visiting the tank for some days ; but when next seen, one specimen had eleven tentacular arms instead of eight. The other specimen continued normal till death.

The per-radial joining $c\text{-}c^1$ and $c\text{-}c^5$—which were slightly smaller than the others—divided the animal into an upper abnormal and a lower normal half. The latter calls for no observation. Between $c\text{-}c^5$ and $c\text{-}c^6$ were two arms well developed ; between $c\text{-}c^6$ and $c\text{-}c^7$ were also two arms, but that next $c\text{-}c^7$ was smaller than its neighbour ; there was a single arm between $c\text{-}c^7$ and $c\text{-}c^8$; and between $c\text{-}c^8$ and $c\text{-}c^1$ arm ^{8} was duplicated.

As I felt unable to deal with the problem, I offered the animal to Professor E. Ray Lankester, at whose wish it was sent to Mr. Garstang, at Plymouth, who, with regard to the last-mentioned arm, wrote to me thus :—" Arm ^{8} has now only one bunch of tentacles ; but there is an abrasion on one of its edges, near $c\text{-}c^8$, and possibly one of the supernumerary arms has sloughed away from that spot."

Examination showed him that the septa in the upper half were four in number instead of two. The following diagram represents the abnormal half as it was when it reached Plymouth:—



A Arms. C.C Colleto-cystophores.

Mr. Garstang was good enough to preserve the specimen, and it is now at Plymouth, so as to be available for anyone working there.

My first impression was that the supernumerary arms arose by fission of the normal ones (though it is difficult to account for the non-fission of arm 7). This was also Mr. Garstang's opinion; and though my rough notes state that the supernumerary arms were, when first observed, smaller than the others, and arose as outgrowths, the notes were made under such disadvantages that I willingly withdraw them in favour of his view.

Henry Scherren.

TO CORRESPONDENTS.

All communications for the Editor *to be addressed to the* Editorial Offices, *now removed to* 5 John Street, Bedford Row, London, W.C.

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ERRATUM.

Page 106. The lines under the heading "Abyssinian Sub-Region" should be transferred to the heading "South African Sub-Region" below.

NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

No. 21. Vol. III. NOVEMBER, 1893.

NOTES AND COMMENTS.

Science—Educational and Professional.

AT the recent opening of the new buildings for the Department of Human Anatomy at Oxford, a question of considerable interest came into prominence. As everyone knows, Science has two sides, and the two sides, except in the most elementary stages, have a very different aspect so far as study is concerned. All the natural sciences have their applied side. Geology is concerned with agriculture and with mining; Botany with agriculture, with pharmacy, forestry, and so forth; Anatomy and Physiology with medicine; Zoology with veterinary work, pisciculture, etc. As purely educational subjects and as ends in themselves leading to research and the advancement of knowledge, they are at least equally important. The one side appeals to the practical man—the follower in philosophy of Bacon; the other to the idealist, the follower of Plato. But it is a hard world, and as the practical side offers a livelihood, students are attracted to it. This rules especially in the department of Biology, as a medical career attracts so many men. What is a University to do? Its highest function certainly is the Platonic side of science, and the great endowments of the past would seem best used when directed to learning. But what are professors and laboratories without students? Cambridge effects a reconciliation by having a large medical school and by giving many fellowships to Biology men, who research and batten on the fees of medical students. Oxford has a small but growing medical school, but the few biological fellows she has had, have with all haste shaken the dust of Oxford from their feet.

Demonstrators at Oxford.

Another pressing problem at Oxford is the status of demonstrators. The habit of the University has been to consider the demonstrators as mere personal appanages of the Professors. The result of that habit has been that almost every change in the professoriate has been followed by an alteration in the staff. As a general rule, that alteration has been such as to substitute for Oxford graduates, graduates of other universities. Even apart from this, very few demonstrators have stayed at their posts longer than a year or two. So far there are at once advantages and disadvantages in this custom. But what is an unmixed evil, and what is being agitated against at present, is the absolute want of University status possessed by demonstrators. Appointed and removed at the will of the professor, unrepresented on the Councils of the teaching staff, practically unrecognised by the University and totally unrecognised by the Colleges, the posts have nothing to compensate the meagreness of the stipends. If the leaders of Oxford University really want to stimulate Science at Oxford there is a full programme of reform before them. The planks in such a reform might well be:—

Increase of Scientific Fellowships.

Increase of Stipends of Demonstrators.

Improvement of Status of Demonstrators by

(1.) Recognition by the University.

(2.) Recognition by the Colleges.

(3.) Admission of Demonstrators, at least when these are Oxford graduates, to the Board of Faculties, and to the Museum delegacy.

In a word, demonstratorships should be University appointments: like other University appointments, they should be attached to colleges, so that by virtue of office a demonstrator should become a member of some senior common-room.

Science as She is Taught.

Being anxious to obtain some information about popular encyclopædias, we recently set our office-boy to study and report upon them. Our first selection—Saxon's "Everybody's Scrap-Book of Curious Facts," by Don Lemon—was apparently not a fortunate one, and the following extracts will show that our office-boy's report that the statements therein contained are more correctly described as "curious" than as "facts," was not without justification. We told him to commence with the Natural Sciences, and he was naturally startled by finding our respected contemporary, the *Zoologist*, made responsible for the statement that whalebone forms the substitute for teeth, and that sheep have no teeth in the upper jaw; we shall expect a libel action to result from this. The author, however, accepts himself the responsibility for making centipedes into reptiles (p. 189), and placing the teeth of the sea-urchin round the stomach (p. 300), and for revealing to geologists (p. 47) that granite "is from two to ten times as thick as the united thicknesses of all the other rocks," and that from granite all other rocks have been derived; if he stopped here we might have been grateful for the information, but as he goes on to talk about *atoms* of lime, and that granite contains none, our faith is shaken. Of course, many fine old-crusted "Münchausenisms" are trotted out anew, such as the 5,000 year old baobabs (p. 173), and the raining of blood (p. 259); confirmation of the existence of the giants on the earth in those days is still derived from the bones of the Irish Elk (p. 260), odour is yet regarded as immaterial (p. 8), and the author seems to have no suspicion that the humming of insects is due to any other cause than the flapping of the wings. Perhaps it is hardly worth calling attention to so slight a mistake as not knowing the difference between Newfoundland and Labrador, which must have given rise to the statement on p. 128 that the interior of the former is still a *terra incognita*. Geography is not the author's strong point; but neither is mathematics, or he would not have left (pp. 302, 303), as an inscrutable problem, the "curious arithmetical puzzle" of the herring-and-a-half for three halfpence order. We are, therefore, not surprised when he tells us the atmosphere is saturated when it contains 84 per cent. out of a possible 80 per cent. of moisture; in these conditions we should think it as supersaturated as the compiler's brains. Nor ought one to grumble that, when he tries his hands on crystallography, he fails to distinguish between lapidaries and crystallographers, and in describing garnets mistakes polygons for polyhedra (pp. 131, 132).

His "facts" are not only at times inconsistent with the facts, but they are often inconsistent with one another. Thus on page 212 he tells us first that Sunken Lake is the deepest in the world, and next that Lake Baikal is "by far the deepest lake in the world." If he had only put these facts a few pages apart, the office boy would doubtless have done his best to believe them; the juxtaposition of contradictions in this case is the more surprising, as elsewhere the illustrations of the truth of a remark are often given in quite a different part of the encyclopædia: thus he heads one paragraph "statistics are funny" on p. 55, while his calculation as to the population of ancient Rome which proves it, is delayed till p. 80. In spite of all the vast range of the author's knowledge he is very modest and loves simplicity: thus his formulæ on p. 44 for determining ships' tonnage ought to charm the heart of Lloyd's with their exquisite ease and wide applicability; while his confession on p. 19 that he will never be able to understand the principle of the steam ejector shows that he does not rate his own powers very highly; but when he sums up "what's a flame?" in four lines beginning with "Combustion is in some way produced by the union of carbon and hydrogen with oxygen," and ends with "the best philosopher can tell little more," we cannot but excuse our office-boy's comment of "More things in heaven and earth can be explained than by your philosophy, Don Lemon." The author's use of the word "fact" is certainly strangely comprehensive, for we do not quite see how advice as to the selection of a wife (however sound in itself) can be regarded as such; but we cannot grumble at this as, if it were excluded, so might have been the remarks of Professor Blackie, which ought, in future editions, to be rescued from the obscurity of p. 51, and printed in large type on the title page:—" Never force yourself to learn what you have no talent for. . . Be content to be ignorant of many things, that you may know one thing well." This collection of curiosities has, however, we understand, an enormous sale, and its statements are doubtless accepted as "facts," whereby we are reminded of Carlyle's estimate of the population of the British Islands, "30,000,000 mostly fools!"

A Recent Submergence of Western Europe.

Professor Prestwich has communicated to the Royal Society a paper "On the Evidences of a Submergence of Western Europe and of the Mediterranean Coasts at the close of the Glacial or so-called post-Glacial Period, and immediately preceding the Neolithic or Recent Period." (*Proceedings*, vol. liii., pp. 80–89.)

He points out that certain superficial deposits, containing organic remains of a land-surface, and composed always of local materials, are found to radiate from independent centres and to show no signs of glacial action. They are grouped by the author under the general name of "Rubble Drift." This assumes various forms.

As "*Head*" it overlies raised beaches by the Channel, in France, Jersey and Guernsey, and on the North African Coast eastward to Tunis.

As *Osseous Breccia* it covers the lower slopes of the rock of Gibraltar, extends round the borders of the hills encircling Palermo's plain, and is found beneath the high escarped rocks of Malta and in Greece.

In *Ossiferous Fissures* it occurs on isolated hills along the Riviera, in Southern France, at Gibraltar and in Algeria.

As *Loess*, apart from that which occurs within valleys and is due to river floods, it is found on dividing watersheds and the high plains of West and Central Europe.

Differing though this Rubble Drift does in detail, the author considers it to be due to a common cause, and only explicable upon the hypothesis of a submergence, which was at once widespread and short in duration. It allowed no time for marine sedimentation, while the establishment of a marine fauna was prevented by the turbid character of its waters. The resulting deposits come from the wreck of the land-surface only. This submergence was followed by an upheaval, since which the configuration of the land has remained comparatively unaltered.

The ossiferous fissures, found often on isolated hills, in France, at Gibraltar, and elsewhere, contain the remains of carnivores, ungulates, and ruminants. After showing that these remains could not have been collected by beasts of prey, the author points out that only a great and a common danger could have driven together such variously-natured animals. Such danger would be found in advancing waters, driving the animals to take refuge on isolated hills, or in other circumscribed areas, there to meet their deaths by drowning. It is noticed that while certain animals did not survive the rubble drift in the west, they existed to historic times eastwards in Egypt—of the submergence of which country there is as yet no evidence.

When the upheaval followed, portions of the surface *débris* were swept down, detached bones were carried away, other bones were crushed and splintered by rolling fragments of rock; and the drift deposits were dispersed from various centres by the diverging currents that resulted from the gradual elevation of the sea-bottom.

Nuclear Division in Branchipus and Apus.

Mr. J. E. S. Moore, in the *Quarterly Journal of Microscopical Science* for September (vol. 35, pp. 259–283), has published some results of great interest. In the male gland of Branchipus, the first stage in spermatogenesis consists of the enlargement of the cells lining the tube, and the arrangement of their nuclei into a spirem with the chromatic elements all on one side. During this time, a large number of bodies like centrosomes (pseudosomes), appears in the protoplasm at the cell-basis. The spirem breaks up into ten dumb-bell shaped chromosomes, and these arrange themselves in an equatorial plate. But in this process the chief interest is that the observer regards it in the light of Bütschli's froth theory of protoplasm. At first the whole nucleus appears to be made up of drops of a clear fluid, enclosed in films of staining material. These drops gradually run into each other, as the drops of a soap foam grow larger by smaller bubbles breaking into the larger. As this process goes on the strands or meshes of staining material become thicker and thicker, and ultimately break up into the chromosomes. As this running together of the nuclear bubbles progresses it breaks beyond the nuclear limits, and bursts into the cytoplasm. The chromosomes thus are left hanging in a clear plasm, supported by a few strands of the cytoplasm which have not yet been broken through. These irregular strands become reduced to fine threads, and the threads terminate in the pseudosomes. As a result of the tractions of the films of the bursting bubbles, the threads with the chromosomes become pulled into a spindle of which the poles are those pseudosomes which have "the best foothold," on the cell periphery. The other pseudosomes and their threads converge on these, and the result is a more or less regular spindle. The actual centrosomes are a fusion of some of these pseudosomes, and are thus in origin equivalent to the angular spaces in the network exterior to the nucleus. A progressive fusion of the bubbles in the cytoplasm sweeps together the scanty stainable material of the cytoplasm into twenty bodies called *dictyosomes*, just as the fusion of the bubbles in the nucleus swept together the nuclear chromatics into the chromosomes. Thus pseudosomes, centrosomes, and dictyosomes, as well as chromosomes themselves, are all expressions of the froth-like activity of protoplasm.

It is of much interest to find an observer of actual miscroscopic details in karyokinesis connecting the actual observed processes with, on the one hand, Professor Bütschli's Foam Theory of protoplasm, and, on the other, Professor Weismann's idants and reducing divisions. It is truly bringing the phenomena of heredity into close relation with physical phenomena.

The Origin of Dicotyledons.

Botanists as well as geologists will welcome Professor Lesquereux's posthumous monograph on the Flora of the Dakota Group (*Monographs of the United States Geological Survey*, vol. xvii.). Few facts are so puzzling to the evolutionist as the sudden appearance in profusion in Middle Cretaceous times of highly-organised dicotyledons, often belonging to existing genera. The older strata, even the Wealden, give no hint of the existence of any plants of such high type; and yet the Cenomanian deposits of Dakota have already yielded no fewer than 429 species of dicotyledons, without including any herbaceous species, for little has been preserved except leaves of deciduous trees. When the discovery in Dakota of these remarkable deposits was first announced, there was a natural hesitation to accept their Cretaceous age; but, since the stratigraphical evidence has been made clear, botanists have been awaiting the appearance of figures and descriptions, to show to what extent the reference of so many of the plants to still living genera is justifiable. Now that this handsome monograph has been issued, we are better able to judge, and one cannot help being struck with the close resemblance of many of the leaves to those of existing plants; yet, on the other hand, one observes that nearly all of the species are described from leaves alone, and that the few detached fruit yet found have nearly all to be left in the indefinite genus *Carpites*, for they do not seem to be closely allied to recent genera. According to Professor Lesquereux's analysis, the flora of the Dakota group is composed of 460 species, of which 6 are ferns, 12 cycads, 15 conifers, 8 monocotyledons, and 429 dicotyledons. Such a proportion of dicotyledons is far more suggestive of a recent flora than of one of Cretaceous age, and it is singular how few characteristic Secondary genera have been obtained. Professor Lesquereux's suggestion, that we are enabled to refer the origin of the dicotyledonous plants to the beginning of the Cretaceous period, no doubt accords with the facts, as far as they are yet known; but both botanists and geologists will hesitate to adopt it, in face of the highly differentiated dicotyledons existing in the Dakota group.

A Remarkable Orchid.

Mr. F. W. Moore, the enthusiastic Curator of the Glasnevin Gardens, Dublin, has, according to the *Orchid Review*, recently flowered, for the first time in Europe, a plant of *Coryanthes Wolfii*. This remarkable genus attracted the attention of Charles Darwin, who gives a figure of the flower, and describes its strange method of fertilisation, in his work on the Fertilisation of Orchids. It will be remembered that drops of liquid are excreted and caught in a bucket-shaped development of the lip, that bees struggling for a place on the edges of the bucket get pushed in, and their wings being wetted by the liquid, have no means of escape except by a narrow passage which is arched over and temporarily closed by the anther and stigmatic surface. The present species comes from Ecuador, and was named by Mr. Lehmann, the German Consul in the United States of Colombia, well-known as an energetic botanist and orchidologist. He says, writing in the *Gardeners' Chronicle*—"It grows very sparingly, mostly on cacao trees, all over the littoral districts of the Guayas, where it flowers in February and March, when these level lands are mostly inundated. During this season it is beyond the power of man to penetrate the woods there—a circumstance that accounts for the plant not having been seen before. It produces thick upright flower-spikes, 40 to 50 cm. high, with three to six large, wonderfully-constructed flowers, which are yellow, mottled, and stained with brownish red. There are but few plants in the entire vegetable kingdom which are more interesting, and which afford such a varied amount of material for the student of vegetable physiology." The plant has a strong attraction for ants, numbers of which surround its root masses, and are apparently essential to its well-being, as in their absence it seems to do badly. This myrmecophily was observed not only in the tropical South American forests, but under cultivation. The ant is a small species of *Myrmica*, with a strong aromatic smell, and a bite so severe that it requires some courage to meddle with the plant.

Names of Orchids.

In the same journal, which, by the way, contains much to interest the orchid-lover, are descriptions by Mr. Rolfe of new species of *Masdevallia*, *Laelia*, and *Maxillaria* which have flowered under cultivation, and also a note of warning to those who are apt to trifle with nomenclature. *Cypripedium spectabile* is one of the best known and most beautiful of Cypripediums, but it has no right to this name, under which Salisbury described it in 1791, as in so doing he ignored two earlier names, *C. album* of Aiton (1789), which seems to have been discarded as inapplicable because the lip is rose-coloured, and *C. reginæ* under which it was described by Walter in his "Flora Caroliniana" in 1788. *Cypripedium reginæ*, as we must therefore call it, and none will deny the suitability of the name, has a remarkable distribution. Hitherto known only as a native of the peat bogs of Canada and Eastern North America, it has recently been discovered in Western China, on the borders of Tibet, together with the remarkable little *C. arietinum*, also a native of the same parts of North America.

As a genus, *Cypripedium* has fared somewhat badly with the people who discuss nomenclature. In his review of Mr. Jackson's monumental "Index," the editor of the *Journal of Botany* refers to the omission of Ascherson's emendation of the Linnean name, to wit *Cypripedilum*, launched in the Flora of Brandenburg in 1864 to accord with the etymology (πέδιλον, a slipper), since *Cypripedium*, as the German doctor observes, "ist nicht zu erklaren." It is to be regretted that Pfitzer should have taken up Ascherson's name in Engler and Prantl's "Pflanzenfamilien" and elsewhere.

The practice of amending names the etymology of which is not clear to the emendator's mind is a reprehensible one, and apt to lead to still greater confusions. Thus, to correspond with Ascherson's alteration, Pfitzer writes *Selenipedilum* for *Selenipedum* of Reichenbach *fil.*, and also establishes a third genus—*Paphiopedilum*. The new genus must stand, but the other two return to their original form, and so the symmetry is destroyed. Another instance of the same intermeddling is supplied by Lestiboudois' *Heleocharis* for Robert Brown's *Eleocharis*, to explain its derivation from ἕλος a marsh, and dispel any idea of pity (ἔλεος) or a kitchen table (ἐλεός).

American Botanists in Congress.

The September number of the *Botanical Gazette* is filled with reports of meetings. The American Association for the Advancement of Science met at Madison, Wis., on Thursday, August 17, and sat till the following Tuesday. Professor C. E. Bessey presided over the botanical section, and the *Gazette* prints an abstract of his address, as well as an outline of the papers and discussions. In his address, entitled "Evolution and Classification," the Professor upbraids systematists for their unscientific conservatism in retaining the crude system of Jussieu and De Candolle for the arrangement of flowering plants for more than thirty years after the general acceptance of the doctrine of evolution. Evolution has taught us what relationship means, and from the new point of view a natural classification is not merely an orderly arrangement of similar organisms, but an expression of genetic relationship, in which primitive forms will precede those derived, and the relationship of the latter be positively indicated. Just as Carl Linné's artificial system was in general use among botanists long after the construction of a natural system by Jussieu, so in turn this natural system has persisted by the help of conservatism and reverence, till ceasing to be natural it has become a makeshift, and "is now as much a clog and hindrance to the systematic botany of the higher plants, as was that of Linné sixty years ago."

There is doubtless a great deal of truth in these remarks, but what has Professor Bessey to offer us in place of our present system? It was admitted from the first that the Apetalæ formed an artificial group; a residue, in fact, of families whose places in a natural system could only be determined by further study. This study is now progressing. Treub has shown, by his work on *Casuarina*, that one family at any rate has affinities with groups other than the Polypetalæ among which Professor Bessey would apparently distribute all the apetalous orders. Quite recent work on the Cupuliferæ has led to somewhat similar results, and we cannot but think that until other families of doubtful affinity have been subjected to the same close investigation, it will, on the whole, be better to keep them apart. Of course in the case of those forming the series of Curvembryeæ, and others, relationships are more apparent, and there is no objection to ranking them with their polypetalous allies; but until a more truly natural system has been satisfactorily elaborated, systematists will not be willing to drag to pieces their herbaria, repeating the process every six months to suit the peregrinations of a few orders of doubtful affinity.

Following the botanical proceedings of the Association is an account of the proceedings of the Madison Meeting of the Botanical Club connected with the Association, in which the check list ordered to be prepared by the committee on nomenclature occupied the chief place in the discussions. The Botanical Club met on four occasions during the session of the Association. Finally, we have the proceedings of the Madison Botanical Congress, which occupied two days immediately after the close of the two former functions. The desired international character of the assembly was not realised, the attendance of European botanists falling much below the expectations of the organising committee, and it was therefore decided that questions of nomenclature should not be discussed. Professor Greene was elected President, and M. Henry de Vilmorin, of Paris, one of the Vice-Presidents.

In the September number of the *Journal of Botany*, Mr. Britten makes some amends for the omission of Gilbert White's name from the "Index of British and Irish Botanists," by conclusively demonstrating his claims to be included. A copy of Hudson's "Flora Anglica" has recently come to light with White's autograph on the fly-leaf, and a note, also in his hand, that all plants occurring within the parish of Selborne are marked by a cross. Several corrections and a few MS. notes show that he used the book a great deal. Moreover, Mr. Bell, in his edition of Selborne, states that he had a catalogue of Selborne plants in White's handwriting. At the end of his note Mr. Britten prints a list of the 439 species indicated by the Selborne naturalist.

In our last number we briefly drew attention to the stirring address by Mr. Teall on Uniformitarianism: an antidote has already been provided.

In an article on "The Position of Geology" (*Nineteenth Century*, October), Professor Prestwich offers a vigorous protest against "the dwarfing influence of Uniformitarianism." He admits that the forces acting upon the surface of the globe in past times have remained the same *in kind*; but he strongly opposes the notion that they have remained the same *in degree*. "What (he asks) if it were suggested that the brick-built Pyramid of Hawâra had been laid brick by brick by a single workman? Given time, this would not be beyond the bounds of possibility. But Nature, like the Pharaohs, had greater forces at her command to do the work better and more expeditiously than is admitted by Uniformitarians." Professor Prestwich complains, and we think rightly, that many arguments with regard to geological time and the rate of denudation are based on very limited evidence; they furnish standards applicable to present changes, but they give no measure of the amount and rate of work that could be done. We must interpret phenomena by the light of the facts themselves. Those who claim vast periods of time for the Glacial and post-Glacial periods, give no explanation why such animals as the Reindeer, the Musk-ox, and the Glutton survive unchanged. He concludes that Uniformitarian measures of time "have probably done more to impede the exercise of free inquiry and discussion than any of the catastrophic theories which formerly prevailed"; they "hedge us in by dogmas which forbid any interpretation of the phenomena other than that of fixed rules which are more worthy of the sixteenth than of the nineteenth century."

"The Work of the Geological Survey" forms the title of a paper read, by Sir Archibald Geikie, before the Federated Institution of Mining Engineers (*Transactions*, vol. v., p. 142). After a short sketch of some early geological maps, the writer gives an account of the method of mapping, and of the various kinds of map issued by the Geological Survey. Other branches of the Survey work are dealt with, and it is stated in conclusion that "From the beginning of its existence the Survey has been continually referred to by all branches of the Government Service for information regarding questions in which a knowledge of geology is required. The sinking of wells, the choice of sites for forts and Government buildings, the placing of graveyards, the selection of materials for buildings or roads, the nature of soils and subsoils, with reference to matters of drainage—these and many other subjects have been reported on. Nor has the general public been backward in application for similar information. The offices of the Survey are always open, and every assistance which can be rendered to enquirers is placed freely at their service."

An important memoir on the Jurassic rocks of the Southern Jura by Dr. Attale Riche has lately been published (*Annales de l'Université de Lyon*, vol. vi., 1893). The beds described are the Bajocian, Bathonian, and Callovian, which the author includes as "Jurassique Inférieur." In this respect he departs from the ordinary grouping, which places the Lias in the Lower Jurassic. The local details furnished by the author are valuable ; while his Comparative Table of formations, which places beds with *Ammonites parkinsoni* on a level with those yielding *A. arbustigerus*, is calculated to yield matter for discussion. In this way *A. parkinsoni* comes in both Bajocian and Bathonian formations. He ends the Lias with the zone of *A. opalinus*, and includes the representatives of our Cornbrash in the Callovian. The work shows the difficulties that attend all attempts at very minute correlation of strata in different areas.

A short paper just published by Mr. W. H. Dall is likely to be overlooked as of purely palæontological interest unless his last paragraph is read. Mr. Dall, writing on a subtropical Miocene fauna in arctic Siberia (*Proceedings of the United States National Museum*, vol. xvi.), remarks that "it is perhaps very late in the day to refer to the hypothesis which explained the warm water Old Miocene of the North Atlantic shores by assuming a shifting of the polar axis, so that the pole at that time would have been situated somewhere in central Siberia. That hypothesis has few, if any, friends at the present time ; but it may not be amiss to point out that, if it were necessary to put a quietus on that moribund speculation, the presence of a warm water Old Miocene in eastern Siberia, such as our present fossils indicate, would be quite sufficient to prove that no polar conditions in the modern sense could have existed there during that epoch of geological time."

Subtropical, or at least warm-temperate, Miocene plants have already been obtained in Greenland and Spitzbergen, and the North Pole was evidently completely encircled by a warm belt such as no shifting of the axis will account for.

Authors should, if possible, give a clue in the titles of their papers to the subjects dealt with. In the *Geological Magazine* for October, we find the first part of a paper on "Some Cretaceous Pycnodont Fishes," by Mr. A. Smith Woodward. Imagine our surprise when we discovered in it accounts of Kimmeridgian, Portlandian, and Purbeckian Fishes! Some Neocomian and Cretaceous forms are also described.

I.

Geology in Secondary Education.[1]

THE need for a selection of subjects in modern education becomes pressingly apparent, and there is consequently every danger of specialisation at too early an age. The result of this will be that men and women will grow up as students of some branch of natural science, literature, or mathematics, with even less in common than is the case between the several groups of educated persons at the present day. While every thorough worker, knowing the influence of his own favourite study upon himself, is apt to put forward the claims of that study as the real essential in educational progress, it is with all seriousness that I assert that, in general secondary education, Geology should receive a recognised position.

Elementary Chemistry and Physics may be accepted as being now taught in all self-respecting institutions, such as the great day-schools, the endowed boarding-schools—which often usurp the title of "public schools"—and similar progressive seats of learning. In the following suggestions, everything that is put forward is intended to apply equally to girls as well as to boys, the time having long passed when it could be maintained that knowledge which enlarged the views of the one sex would unfit the other for the affairs of life. This paper is also aimed somewhat at the future, when secondary education will be brought within the reach of all classes, perhaps by a wider system of scholarships open to pupils of the board-schools.

Given a fair grounding in the facts of elementary chemistry and physics, the application of these facts to the great natural world on which we depend leads us directly to geology. The common minerals, the common rocks, their common modes of association, are capable of direct observation, and offer material for consideration wherever the future lot of the student may be cast. I am now taking the case of the ordinary individual, whose leisure-time is becoming in general increased. The correct understanding of one's surroundings is capable of adding a new pleasure to existence, and of providing a rational occupation—which is one of the great ends of education.

The amount of knowledge that the average schoolboy possesses about the Punic Wars, or the externals of Greek mythology, is some-

[1] An introduction to a discussion at the meeting of the British Association at Nottingham, September, 1893.

thing awe-inspiring, at the least; yet he comes up to a University in ignorance of what has made the features of the landscape which he has seen all his life from the door of his country home, and of what agents have brought together the pebbles of his summer bathing place.

If the utility of such knowledge be questioned, we open up an attack upon the vast body of information crammed into unsuspecting childhood. The only answer is to admit the utility of all knowledge whatsoever, and then to assert the special desirability of that branch under consideration.

At the present moment much attention is given to technical education, which has two main objects—the improvement of the workman and the improvement of the object produced; but there are many who look to such education primarily as a means of acquiring wealth—and also of competing with the Germans. In this paper I regard education as a means of acquiring happiness and of living peaceably with one's neighbours; so that the financial return may be disregarded until special cases are considered in the discussion. National secondary education, I take it, should insist on the dependence of the individual on his fellows rather than on the dependence of the masses on an individual; in other words, it should enable a man to realise his position as a member of a community, to live without grasping, and happily upon a moderate income.

Now Geology becomes indispensable for a correct appreciation of our relations to our surroundings and to the past. In a general course of instruction during, say, the last year of secondary education, when the pupils are of the age of 16 or 17, I would treat of the different minerals of which rocks are made, always bearing in mind the materials to be found in the locality in which the teaching is being given. I would deal as little as possible with the more refined aspects of these minerals—such as their crystalline systems—beyond an indication of the facts of symmetry and of how minerals, apparently similar, may be ultimately distinguished by their crystalline forms. I would lay much stress on the character of hardness, using the thumb-nail and the pocket-knife, and on simple chemical means of determination. It may be worth noting that I have always found girls most apt in matters requiring the elements of crystallography, while boys, as a rule, are indifferent to symmetrical beauty.

In dealing with rocks, I should exclude the use of microscopic sections. For our broad untechnical purposes, it would seem better to grind down a surface when structure has to be exhibited, and to pick a rock to pieces, with the aid of a hand-lens or a dissecting microscope, when its constituents have to be determined. The methods used in more elaborate work might, however, be hinted at; but in every week's lesson the teacher will have to remember that he is training men and women, and not miners or geologists.

The mineral analysis of some ten common rocks may thus be discussed; the teacher will do well to avoid debatable terms, or those dependent on purely microscopic examination. Every encouragement should be given to the idea that rock-specimens are parts of the great masses round us, and that Geology is a study of the open air, natural in the highest sense, and not an exercise in more or less classical terminology.

The modes of origin of these rocks may be described side by side with their mineral characters, an amount of physical geology being thus introduced to enliven the bald statement of their constitution. Thus an account of modern marine shell-banks may precede the discussion of limestone, or may proceed naturally from the observation of fossils in a rock; while an account of the main features of an active volcano will add vast interest, and even dignity, to the details of a lump of basalt.

The structures of mountains, the carving of valleys in high plateaux, the rounding of pebbles, and the sifting of materials in the streams, can all be illustrated by local examples, or by reference to the customary excursion-resorts of the people. London and the south-east of England form a district less favoured than most parts in the matter of variety of structure; yet the broad facts of physical geology can even there be dealt with practically.

But when the proofs of what were formerly called "revolutions" in the surface of the globe have been made plain, and the slow and gradual nature of those changes has been impressed upon the student in the field, the review of the past history of life becomes the main object of his study.

For the public at large, this portion of the subject has, to my thinking, the greatest educational value; I urge, indeed, the fundamental importance of geology on account of the desirability of giving everyone, from the peer to the proletariat, an outline of the history of life upon the globe.

It is impossible to disguise the fact that the teaching of human history is liable to be both partial and partisan; and that, under existing systems of examination, a pupil may be well read in the events of one century of the history of his own people, and yet be entirely ignorant of the causes that have moulded the popular opinions of his own time. Firstly, then, Geology introduces the student to a history in which one is not called on to take sides, a history the compilation of which is the outcome of enquiry rather than enthusiasm—a remark that holds good, at any rate, until we reach the Glacial Period. The pupil learns, then, the beauty of facts, and that what we call truth is a conscientious and necessarily imperfect deduction from them. The very imperfection of the record will cause him to reason for himself, and to recognise fully the difficulties of the process. Thus, perhaps, his judgment of things in general will become less personal, less harsh, and less redolent of the dogmatism of a "school."

But any natural science will similarly benefit and broaden our egotistical souls; the peculiar claim of Geology is its direct bearing upon history. To the average schoolboy the Greeks and Romans divide the honours of antiquity; the classical models of heroic virtue are so marked out and emphasised that it is often doubted, in serious works and popular lectures, whether humanity has altered for the better since the days of Horatius Cocles. The gloomy remark that human nature will always be human nature is continually regarded as an argument in favour of repressing the species, rather than of providing loopholes for its development. To present the ordinary man or woman with a picture of the Palæolithic epoch, and at the same time with a history of life in which, on any reasonable scale, the whole of recorded human history, including the Chinese, is too short to be taken as a unit—this surely is calculated to give one room for hope, and to shake one's faith in the immutability of human nature.

One more point of view. The enormous past, which we cannot deny to have been progressive, tends to give a certain solidarity to our conception of the human race. Man becomes a prominent feature along a particular and limited horizon of the earth's history, just as the Romans did in the recent history of Europe. The race acquires an importance to the geologist, standing, as it does, at the end of so vast a series of life-changes; and he who learns to respect the race may in time become less callous about the extinction of an individual. I do not mean to assert that a diffused knowledge of, let us say, the Eocene Period would have prevented the hecatombs of Waterloo or Weissenburg; but it would have gone far to make such tragedies more generally regretted, and more generally hateful. The stratigraphical position of man, when it is once realised, makes him appreciate both his present possibilities and his dangers. If Geology helps him to recognise the differences between groups of men, as, at the most, specific rather than generic, the study will have done much to promote the harmony of nations.

I would ask, then, those interested in Geology to watch the progress of public education, and to endeavour to secure a place for this science side by side with the historic and human studies of later years of the curriculum. Personally, I have found it most interesting to note the intrusion of completely novel ideas—in my own case in Colleges for women—when such a subject is superposed on the ordinary courses for an Arts degree. Although the foundation of Chemistry and Physics is still in many cases slight or absent, the results, as far as I can observe them, are full of encouragement and hope.

GRENVILLE A. J. COLE.

II.

Natural Science at the Chicago Exhibition.

SCIENCE of any kind is lamentably absent at the World's Fair. In this century, which we delight to patronise as "The Age of Science," and in a country where the educational method of exhibition has been brought to such perfection as it has in the larger American Museums, one would have expected something more than a heterogeneous assemblage of exhibits, some good, many bad, and most indifferent, which, in the expressive language of the day, are planked down anywhere. Organisation and arrangement must have expended all their strength on the outward appearance of the Fair, which is indeed admirable, and have had none left to tackle the far more important problem of displaying the exhibits in the most instructive manner. In this respect the only satisfactory building is that belonging to the Government of the United States; and had the same intelligence which directed that, been, as was originally intended, brought to bear on the rest of the Exposition, the result would have been as gratifying to the earnest student and the scientific visitor as it now is to the pleasure-loving and wonder-seeking public of the World.

Our concern, however, is more particularly with those branches of Science known as Natural, and it may prove instructive to consider how such are represented at Chicago. The exhibits of this kind may be divided into the Natural Science exhibits and the Natural History or semi-scientific exhibits: the latter largely preponderate. As no one visitor can hope to discover everything of interest to him that may be hidden in the nooks and corners of the White City, we have not scrupled to draw when necessary on an excellent article that appeared above the well-known initials of W. H. D., in *The Nation* for September 14, 1893; quotations therefrom are distinguished by inverted commas.

"In ZOOLOGY the Fair offers a rather meagre display, which is made still more obscure by being broken up into small exhibits, many of which are found in most unexpected places." Most of them, however, are, or are supposed to be, contained in the Anthropological Building, and among these the most representative general exhibit is that of Professor Ward of Rochester. "Here a very excellent series of specimens suitable for a teaching museum are brought together, well and clearly labelled, and attractively arranged. The model of the mammoth here attracts much attention, and the taxidermy throughout is good, if not remarkable." Among the Invertebrata of Professor Ward's collection, the Corals and Echinoderms, notably the dried and beautifully-mounted specimens of *Pentacrinus*, are worthy of attention: for exhibition purposes these latter are certainly superior to spirit-specimens, as they can be clearly seen. In the East Gallery of this building are the faunal exhibits of Maine, Ohio, Colorado, and Ontario; also "a number of specimens of taxidermy exhibited by private persons, none of which needs special comment; the general average is poor and inartistic. The furriers in the Liberal Arts Buildings have here and there a well-mounted fur animal; a few may be found in the Leather Building; and small collections representing State faunas may be found in most of the buildings erected by the several States or foreign countries. Most of these are poorly mounted and very imperfectly labelled. Occasionally a rare creature may be detected among these forlorn representatives of their kind, as in the case of the Liberian hippopotamus, a mounted specimen of which is included in the exhibit of that little African State. The exhibit made by the State Museum of New York is creditable, clean and well labelled, one of the best of the minor collections." The collection of stuffed animals shown by British Guiana is also worthy of special mention.

It is, as above indicated, in the Government Museum that the most scientific exhibits are to be looked for. Here "the National Museum exhibits a mounted series showing examples of all the families and most of the genera of American mammals and a number of groups of North American mammals, accompanied by accessories indicating the natural surroundings of the species. Among these the most interesting are those containing the Rocky Mountain sheep and goats, the woodland and barren-ground caribou, and the Pacific sea-lions. A fine walrus might have been more lifelike if the taxidermist had had a better guide than Elliott's caricatures of this unfortunate animal, which, in addition to extinction, seems to be doomed to posthumous misrepresentation. The most important among the exhibits in this series is the mounted skin of a very good example of the Alaskan sea-otter, perhaps the best existing specimen in any museum. A number of large African game animals are also shown, some of which are rare. Similar series, illustrating the American families of birds, reptiles, and batrachians, fishes, insects, etc., are also exhibited by the Museum, as well as two very attractive collections of the birds of Paradise and humming-birds. Several groups of species, formerly common but apparently now verging on extinction, comprise the Carolina paroquet, the wild pigeon, and the ivory-billed woodpecker. There are also spirited groups representing the courtship of the prairie chickens, and the flamingo with its singular nests." We, in England, are familiar with such natural groups of birds, and excellent examples of this sort of mounting may be seen at the American Museum of Natural History, New York; "but the restricted space and the small amount of money available at Washington have hitherto prevented the National Museum from extending to birds the treatment which has been so successfully applied to the larger mammals." The popularity of this Smithsonian exhibit may be gauged by the difficulty that a visitor experiences in forcing his way through the almost immovable crowd. One portion, however, he will have almost to himself; yet it is by no means the least interesting. This is the admirable Synoptic Series of Invertebrata, arranged by Mr. Lucas, where every help is afforded by means of well-expressed labels. Adjoining is the no less instructive series illustrating the osteology of the vertebrata. This shows the homologies of the principal bones by means of compared articulated and disarticulated skeletons of the chief vertebrate types, the bones of the cranium being distinguished by colours.

Those who visited either the Fisheries Exhibition at South Kensington, or the similar division of the Philadelphia Exposition, will learn little new at Chicago. The Fisheries Building is truly, as it has been called, "a poem in architecture"; but, except for the fisheries of N. America, the exhibition is small and mostly unimportant. "The reason," writes Mr. Dall, "is not far to seek. The United States and Canada collectively have nothing to learn at present from foreign countries, and no foreign dealers in fish products have any reason for supposing that they can gain a foothold in our markets, except for sundry specialties like cod-liver oil. Consequently the commercial incentive is lacking, and, apart from such countries as New South Wales and Japan, which have made an exhibit as a matter of national pride and in evidence of the state of their industries, the foreign fisheries are very imperfectly represented." The exhibit of the Fish Commission in the Government Building is good, "though less prominent than in 1876, and the practical rather than the scientific side of its work is emphasised. Similarly, the collection shown by Japan is more interesting from the economic or anthropological side than as illustrating the ichthyology of the empire." Pounded shark and dried cuttle-fish do not strike the Western mind as particularly appetising; but with cockroach catsup they are by no means bad. One doubts, however, whether the enterprising Japanese firm who are trying to introduce dried sea-weed as a relish will meet with the success that they deserve. Some of the Japanese methods of fishing are so curious that they might have been illustrated at Chicago, but, as they are not, one cannot now describe them. "There are a few interesting models of fish traps and weirs in the collection shown by the Sultan of Johore in the Plaisance, but these objects belong under the head of Anthropology rather than that of Ichthyology."

Among the more obviously attractive exhibits in the Fisheries Building are the skeleton of a Pacific Humpback Whale, 47 ft. 6 in.

long, and 48 ft. in girth, the tent used by Lieut. Peary on his present Arctic expedition, and the Aquaria. In these latter the foreigner will probably be most attracted by the fresh-water fish of America; while, as Mr. Dall remarks, the visitor from the Western States will learn much about the salt-water fish so unfamiliar to him. Certainly, it was round the sea tanks that crowds congregated. The fresh-water tanks have, unfortunately, suffered from virulent attacks of *Saprolegnia*, the fish fungus. "This exhibit, as a whole, is that of tanks of water with fish in them, and not aquaria in the strict sense of the word, which implies a balance of conditions between the water plants and the animals, as in a state of nature."

"Many of the States show more or less dilapidated stuffed fishes as a part of their faunal exhibit, but California has an excellent series of models, by Denton's method, of her chief economic species. These are well coloured and properly labelled. New South Wales has a good series of alcoholic fishes, in the same excellent order as the rest of her exhibits," as well as a striking set of coloured drawings.

"The shell-fisheries are almost unrepresented in the Exposition except by the canned product. A few unlabelled and poorly-preserved shells appear in the Brazilian exhibit, a few oysters and pearl shells in the collections of New South Wales and Mexico; Wisconsin shows a pretty series of fresh-water pearls in (of all places) the Mining Building; and Tiffany has a fine series of pearls in their natural state, beside those worked into ornaments—but this is about all that is visible in this line, excepting the series of shells contained in the Ward exhibit in the Anthropological Building."

In the Government Building the Bureau of Animal Industry has an admirable exhibit, which shows how well science may be combined with practice. Part shows the distribution of animal life according to elevation, and part animals which are either beneficial or injurious to agriculture. Most noticeable here is the exhibit of the Entomological Bureau. "Probably no portion of the whole Exposition better illustrates the scientific modern method of presenting such matters to the public eye. The insect is shown fully labelled in all its stages and varieties, with its food plant and cocoons, with illustrations of its ravages when injurious, or beneficial methods of work when useful, supplemented by a small map showing its geographical range, and often, when of economic importance, accompanied by enlarged anatomical models. Nothing more clear, instructive, and satisfactory can be imagined. The experiment stations of the several States in the Agricultural Building show a collective exhibit which is very creditable, though less efficiently displayed than that made by the Government; and there are the usual miscellaneous collections of attractive butterflies, etc., to be found in many of the State exhibits and in those shown by foreign Governments."

The exhibits and collections hitherto alluded to fall, for the most part, under the head of semi-scientific. Those of deeper scientific interest are chiefly to be found among the Educational Exhibits in the Manufactures and Liberal Arts Building. Here such institutions as Harvard, Johns Hopkins, Princeton, Columbia College, and the University of Pennsylvania illustrate the methods of their science-teaching and of the original research carried on under their auspices. To a scientific man this is the most interesting part of the whole Exhibition; for we are all of us students, and a few of us are teachers. Still one must regret that so small a part of the Exhibition is on these lines, and that foreign countries are so little represented. Japan, indeed, gives evidence of her new birth in this direction as well as in others; but where are the Laboratories and Museums of our old Universities? where is the Royal College of Science? where are Bonn, Heidelberg, Freiburg, Liége, Paris, Upsala, Dorpat, and Bologna? Surely the heirs of Galileo need not shun the land of Columbus. What an exhibition rises before the scientific imagination at the mere mention of these few names! But, alas! men of science are too poor even to advertise themselves, so we must make the best of Chicago, and that which the American Universities have been good enough to do for us.

From these exhibits each visitor will carry away thoughts in accordance with his own needs. The following are what appear in my note-book. A giant microtome, used chiefly for cutting microscope sections through the entire brain, exhibited by the University of Pennsylvania. The object to be transected is fixed at the end of a very heavy lever and is allowed to sink down upon the edge of a broad knife, the blade of which is parallel to the side of the lever. The section as it comes off is caught on a sloping sheet of paper. The Biological Laboratory of this University makes a regular practice of selling photographs of preparations, specimens, and drawings; and a catalogue of such, which might prove useful to lecturers, may be obtained from the Secretary. Lecturers will also be interested in some models of brains exhibited by the Physiological Department of the Loomis Laboratory of the University of New York City. These models are first of all constructed in clay; the clay is then covered over with strips of newspaper soaked in glue, which form a shell about a quarter of an inch thick. When the structure is quite dry, the clay is removed, and the hollow paper model remaining is appropriately coloured in oils. Such models may be made any size, are very light, and will stand rough usage. In the Harvard Zoological Department, W. McM. Woodworth exhibits some enlarged wax models of microscopic objects which are constructed from thin sections by an ingenious method. A camera drawing is made of each section in the series on some thin paper. The drawing is then attached to a sheet of wax which bears the same proportion to the thickness of the original section as the drawing does to its area. Each sheet of wax is trimmed to the outline of the drawing, and the successive sheets are then stuck together. The details of manipulation, which cannot be described here, are fully illustrated by specimens and apparatus.

There is no regular exhibit of scientific instruments, except that shown by several German firms in the Electrical Building. Here, too, are the excellent anatomical and embryological models made by Fr. Ziegler, of Freiburg i/B. His series now numbers 279, and costs about £120: which is not dear, considering its educational value.

Under the auspices of Harvard there are carried on in the Anthropological Building an Anthropometric and a Psychometric Laboratory. Here, in return for a dollar, one may have almost every physical and mental character tested. With the anthropometric tests Mr. Francis Galton has pleasantly familiarised us in England; but the psychometric ordeal is somewhat of a new departure, worthy of a more detailed account than is possible in this article. The casual but curious visitor to Chicago is constantly checked by some psychologic puzzle, either a pathetic appeal to choose his favourite colour among several crude abominations, or an entreaty to determine which of several meaningless geometric figures most appeals to his artistic sense of beauty. In a conscientious endeavour to advance Science by submitting to these mental tortures, I lost several hours that might have been as usefully employed in the Midway Plaisance.

To turn to GEOLOGY. One cannot complain of lack of material; collections of Fossils and Minerals are to be found in at least eighteen buildings. The most interesting of these are the Stratigraphical series of Fossils exhibited by the United States Survey in the Government Building; the educational collection of leading Fossils exhibited by Professor Ward in the Anthropological Building, and the very instructive set of specimens illustrating Dynamical Geology, in the Mining Building; his systematic collection of Minerals and set of Meteorites in the same building are also fine; the magnificent set of local fossils, including the Worthen Collection of types, in the Illinois State Building; the G. F. Kunz collection of Minerals and gems. A smaller collection, but one of the best shown by a private individual, consists of the Fossils exhibited by Mrs. A. D. Davidson in the Woman's Building.

The Mining exhibits of most of the States and Countries are either advertisements, commercial, or clap-trap. When the best that Great Britain can do is to hew a statue of Liberty in a lump of rock-salt, one begins to wonder whether we have ever boasted a School of Mines or a Museum of Practical Geology. True that there is a good collection of Economic minerals by Mr. B. H. Brough, which we are glad to see has been awarded a prize. I am told, too, that our Geological Survey has an exhibit somewhere; but what geologist would ever look for a geological map under the head of Liberal Arts? The German and Japanese exhibits are the only ones of any scientific pretensions. The merit of the German miners is, however, sufficiently well-known to Englishmen, and the geological work of Japan is important enough to demand treatment in a separate article.

The Standard Oil Company has an interesting exhibit in the form of a large scale model of a geological section across Ohio, Pennsylvania, and New York, showing the positions of their chief wells, and the relations of the oil-bearing shales. Except for a few geological maps, this is the only exhibit of the kind in the whole of the Fair. *Apropos* of the Oil Company, it may be mentioned that the twenty huge boilers that supply all the power on the grounds are heated by petroleum brought a distance of eighteen miles in a pipe.

In the Anthropological Building, Professor Wright illustrates by numerous specimens and photographs the relations of man to the Glacial Period in Ohio, of which he gave an account in the *Popular Science Monthly* for May of this year. Here, too, are a couple of marvellous exhibits. First, some footprints of various animals from a quarry of what appears to be Tertiary Sandstone at Carson, Nevada, among which certain large depressions, about the size of an elephant's footprint, are confidently assigned to Man. This, of course, upsets all the conclusions of the geologists and biologists. There have never been wanting cranks in America to uphold the existence of Man in any geological period, from the Silurian downwards; but we hardly expected to see their "notions" seriously admitted into the World's Columbian Exposition. And yet, close by, is an even more childish case, entitled Freaks of Nature, and containing such rarities as Noah's Canary, Mother Eve's Mitten, and Little Ham's Pegtop. At the end of the same gallery is an enormous map, illustrating the psychic lines of force that govern the structure of continents, from which the future course of events on our planet may be confidently predicted. Truly the Americans are a great nation, and the Anthropological Building is a great building, so they can afford to amuse us with a little folly.

Botany, considered as a Science, is not largely represented at the Fair, although the Botanist should find plenty to interest him in the Horticultural, Forestry, and Agricultural Buildings. In the first of these, the Japanese garden, with its dwarf trees, attracts some attention. Here one may see a pine tree 100 years old, and only 2 ft. high. The process of dwarfing appears to consist chiefly in eliminating a large number of the leaves and pruning the fresh shoots. The Forestry Building is chiefly devoted to polished woods, many of which are also to be found in the various State Buildings. The most interesting botanical exhibit is probably the selection from a wonderful series of glass models of flowering plants, shown by Harvard University. These are made by Blatschka, of Dresden, in his inimitable manner, and represent the plant as fresh as a daisy, as well as enlarged anatomical details. Nothing so beautiful exists in any other Museum of Natural History. After these, one cares little for the ordinary herbarium; but those who wish to see dried plants will find a few in the State Buildings of California and Michigan, as well as in the Woman's Building.

The scientific exhibit in the Woman's Building is, indeed, chiefly confined to botanical collections of the ordinary type, the only other specimens of interest being Mrs. Davidson's fossils. If Woman chose to divorce herself from Man, and to have a house to herself, she need not have shown herself as weaker than she is. One can imagine something a great deal better than this rather amateurish natural history display that poses as a scientific exhibit. Some papers by Miss Cora Clarke do not represent all the scientific activity of *la belle Americaine*; while foreign ladies seem to have been too shy to put in an appearance at all. To speak only of the English ones, surely the Misses Barton, Buchanan, Crane, Donald, Johnson, Raisin, and others might have exhibited something, if not for the sake of their sex, at least to the honour of their country. As for so eminent a writer as Madame Pavlov, as she has not yet described any fossil pigs, it is not to be expected that Chicago has ever heard of her.

To include ANTHROPOLOGY and ETHNOLOGY under the head of Natural Science, and adequately to describe their manifestations at Chicago, would double the length of this article. For, in one sense, the whole Fair is an Ethnological collection. There is, however, so little real science about any of the exhibits, that we need only allude to the more important. North American Indians are well represented by both recent and prehistoric relics in the Anthropological Building, where also are to be found some rare Mexican and Aztec curiosities. Reproductions of burial grounds, with real live mummies, serve as a Chamber of Horrors and attract a goodly crowd. These have been arranged by the Bureau of Ethnology, under the skilled direction of Professor Putnam, who also shows a fine series of human skulls. Outside the Anthropological Building is a hill, made of tinned iron, which contains an accurate copy of the caves and remains of Colorado Cliff-dwellers. The Australian and African weapons and utensils, and theJapanese prehistoric relics, are exceedingly interesting. Some of the State Buildings and a few Foreign Buildings also contain good collections of prehistoric relics; but, after all, the chief Ethnological interest of the Fair is living, and all around one, notably in the Midway Plaisance. Turks and Egyptians, Dahomeyans and Samoans, Javanese and Japanese, Chinese and Hindoos, meet and mingle in good fellowship with French and Russian, German and English, Italian and Swede, and with that perplexing product of Western civilisation—the American negro. It is all very well for the Dryasdusts to scoff at the pleasures of the Plaisance; but it is as superior to the legitimate collections of the Exposition as a zoological garden is to a row of stuffed animals, or an aquarium to a collection of fish-bones. Nor must we forget the forty beauties, in their national costumes. If the World's Fair offered no other object of interest to the naturalist, it would at least enable him to study the fair of the world.

F. A. BATHER.

III.

The Place of the Lake-Dwellings at Glastonbury in British Archæology.[1]

THE discoveries made by Mr. Bulleid in the Lake-dwellings at Glastonbury are of great historical value, and give a remarkable insight into the condition of the inhabitants of Somerset in the Iron Age. Moreover, their archæological horizon is clearly defined, and they lie close to the frontier which divides the Prehistoric Archæology from the history of the British Isles.

The dwellers in the Glastonbury marshes were spinners and weavers, and used whorls of stone and earthenware in twisting their thread, and weights to keep the warp tight on the loom while they worked in the weft with bone shuttles. The weft was pushed home with the weaving combs, which are both abundant and perfect. The weaving comb probably was the ancestor of the comb worn for ornament in the head-dress of later times. Numerous wooden fragments of a kind of frame probably represent the loom. Flax, in all probability, was the material which was woven, although no direct proof has been met with in this settlement. They used bone-needles for sewing.

They worked wood with great skill with the saw, the bill-hook, the knife, and the gouge, and probably also with the axe and the adze, although the two latter can only be inferred from the workmanship of the boards, and squared parts of the platform.

They also used the lathe, and are proved by the "chucks" of Kimmeridge Clay to have turned ornaments of Kimmeridge shale. Some of these have been discovered.

The lathe-turned vessels, some bearing the marks of a punch found in the settlement, prove that pottery-making was also carried on. Crucibles, and the remains of tuyères, imply that smelting was also carried on, and a piece of blue glass slag may perhaps imply that glass-working was also practised. A file implies also metal-working.

They used rings of jet, amber, and glass, and of bronze, and bracelets of bronze and Kimmeridge shale, and beads of glass, and fastened their clothes together with safety-pins and split-ring brooches of bronze, and with bone links similar to those found in Romano-British caves, such as the Victoria Cave in Yorkshire. They also used amulets of bone, among which is a roundel fashioned out of a human occipital.

[1] Abstract of Address, British Association, Nottingham, Anthropological Section.

Their huts were round and, as at Mount Caburn, near Lewes, composed of wattle. They grew wheat, and had sheep, cattle of the small *Bos longifrons* breed, pigs, horses, and dogs. They did not, however, rely wholly on the domestic animals for food, but, at times, ate the stags, roedeer, beavers, and otters living in the district.

They ground their corn in well-fashioned querns, and boiled the food by putting hot stones into the pots filled with cold water. It is strange to note among these the "potato stones" of the neighbourhood.

A spur of a fighting-cock renders it probable that they were given to cock-fighting like the ancient Gauls.

They rode or drove horses with iron snaffle-bits, and fought at close quarters with daggers, halberts, and bill-hooks, and at a distance with slings. Vast numbers of clay pellets (= the Roman "glandes") for slings, both burned and unburned, have been met with.

The position of the settlement in the marsh implies the fact that warfare was the normal social condition, and testifies to the danger of attack from neighbouring communities.

A fragment of a human skull, long, and with a low forehead, and strong frontal sinuses, implies that some of the inhabitants belonged to the long-headed section of the Britons. It may further be remarked that a shaft of a human humerus, gnawed by some weak-jawed carnivore such as the dog, was also found in one of the huts.

It remains now to sum up the place of these remains in British Archæology. The pottery is distinctly of Southern derivation and of the Late Celtic type, which belongs to the late period of the Iron Age, before the Roman influence had fully penetrated into Britain. Although the split-ring fibula and the bone links are identical with forms of Romano-British type, the absence of Roman pottery and of coins implies that the Roman civilisation had not yet arrived in the Isle of Glastonbury. Roman pottery, it may be noted, abounds in other sites in the district. On a comparison with the Late Celtic remains found by General Pitt-Rivers at Mount Caburn, near Lewes, it will be found that the iron tools and weapons, the earthenware "glandes," the pottery, and the various other articles, and the wattle-work, are practically the same, and belong, therefore, to the same age. The whole group of domestic animals, including the fighting-cock, is also the same in both. The safety-pin brooches, too, are of late Celtic type, and similar to that found in the Late Celtic cemetery at Aylesford, explored by Mr. Arthur Evans. We may, therefore, fix, with tolerable certainty, the age of these Lake-dwellers as being just before the time that the Roman influence was directly felt in the West of England, and certainly before the Roman Conquest. The discovery is most important. When fully worked out it will probably throw a flood of light on the history of pre-Roman Britain.

W. Boyd Dawkins.

IV.

The Air-sacs and Hollow Bones of Birds.

NOT very long ago the problems connected with this subject were settled in a very offhand way. The heated air in the sacs being lighter than the surrounding air, made the bird a balloon, and so flight was easy. For the same object, the bones were hollow and marrowless. Thus a clear and interesting solution seemed to have been found for a great problem.

This theory has withered beneath the cruel light of fact. A bird can carry only a very small amount of air in its sacs, and the difference in weight between a few cubic inches of heated or cold air is too infinitesimal to be worth considering. The sight of an eagle flying off with a lamb ought to convince anyone who cannot otherwise be convinced, that the saving of the tiniest fraction of an ounce of weight would make no difference. True, air within the bird, whether heated or not, will expand its volume, lessen its specific gravity, and so fit it better for floating, but it could not help it to rise, and this is the real difficulty. Moreover, many birds, for instance the swallow, which fly to perfection, have all their large bones solid.

We must, therefore, look for some sounder theory, and first it will be well briefly to survey the facts. The lungs of birds open out into great membranous expansions which lie within the body-cavity next to the ribs or extend far back between the kidneys and the intestines; and, in addition to this, in many cases, the membrane finds its way into the bones as the marrow dries up, sometimes even to the very extremities of the limbs, and in some instances under the skin also, into some of the feathers and between the muscles. Anyone who doubts the connection between the chambers within the bones and the lungs, may convince himself by a very simple experiment. He can take a dead bird (any bird which has some of its bones aërated), break the humerus, and, after tying up the trachea, blow down the bone through a tube, when all the air-sacs will expand as promptly and completely as if the tube were inserted into the trachea.

Before now, a wounded bird, whose windpipe has been stopped with blood, has been known to breathe through a broken and exposed bone. It is beyond a doubt, then, that the hollow bones are lined with expansions of the bronchial membrane; it extends, in fact, even to some parts of the skull.

I shall first discuss the functions of the air-sacs proper as distinguished from their extensions into the bones. It cannot be questioned that they exist partly for purposes of respiration. The lungs themselves are small and inelastic, and the diaphragm, whether it represent that of mammals or not, certainly does not do the same work.

By the action of muscles the body-cavity is expanded, a vacuum is created in the sacs, and the air rushes into them through the lungs. But it is difficult to believe that the air-sacs fulfil no purpose beyond that of respiration. If that is the case, they are far larger than necessary.

I have measured the cubic content of the lungs of a pigeon as nearly as I could, and found it to be $\frac{1}{4}$ inch. The aggregate length of the air-sacs was about $3\frac{3}{8}$ in., the depth about 1 in., the breadth about $\frac{3}{8}$ in. This gives a cubic content of $1\frac{17}{64}$. According to this estimate, therefore, the air-sacs can hold more than five times the amount of air the lungs would hold, even if they were mere bags. And probably this estimate is decidedly below the mark. This disproportionate amount of air is of some use in breathing. When the bird exhales, comparatively fresh air from the sacs is driven into the lungs, so that exhalation and inhalation alike renew the supply of oxygen; but even when this is allowed for, the air-sacs are far more spacious than is necessary for breathing alone. We have to consider, therefore, whether they do not fulfil some useful purpose in addition to their respiratory functions, and if we reflect upon the subject, it is difficult to avoid coming to the conclusion that a bird's temperature is regulated mainly by the lungs and their extensions. This will seem more than probable if we consider the means by which the temperature of the human body, when in health, is kept almost at the same point, however much that of the surrounding air may vary. Heat is lost (1) through skin by conduction, radiation, and evaporation; (2) by respiration; (3) to a small extent through the excreta. Besides this the temperature of the body is partly controlled by the vaso-motor nerves which regulate the flow of blood to particular parts. And, that this is not the only way in which the nervous system governs the temperature, there is indirect evidence in the fact that when a warm-blooded animal is subjected to urari poisoning it behaves like a cold-blooded animal. It has no longer any power of generating heat within itself in order to withstand external cold, or, when exposed to heat, of keeping itself cooler than the surrounding atmosphere.

Whatever the exact nature of this nervous apparatus may be, it is certain that in man the skin plays a very important part in the regulation of temperature, and that it is mainly by evaporation that it does its work.[1] Radiation is constant, but is much checked by

[1] Dr. Michael Foster ("Text-book of Physiology," p. 464, 1883 edition) writes: "It has been calculated that the relative amounts of the losses by these several channels are as follows: in warming the urine and fæces about 3, or according to others 6 per cent. By respiration about 20, or, according to others about 9 only per cent., leaving 77, or alternatively 85 per cent., for conduction and radiation and evaporation from the skin."

clothing; it is only occasionally that we lose much heat by conduction, for instance, when we touch cold iron; evaporation never entirely ceases, and it varies in amount according to the needs of the body. Man, in fact, is like one of the porous earthenware pots used in India for cooling water. Put them in a hot, dry wind, and, the rate of evaporation increasing, the water cools all the more rapidly, and the Sahib's bath is ready all the sooner. Great heat can be endured if only it is dry heat. A French physiologist once stopped a considerable time in a stove heated to 160° Fahr.; but this is far below the record. In 1760, according to the testimony of two French academicians, a woman entered a stove heated to 237° Fahr. and stayed there 12 minutes. Doctors Fordyce and Blayden were able to remain in a chamber heated to 260° Fahr. I have been told that a man who earned his living by feats of this kind found himself compelled to rush precipitately from the heated oven because some one, who was more scientific than kind, had placed a can of hot water in one corner. Everyone knows how oppressive the heat of a hot-house is. The heat of the vapour-baths in Russia is said sometimes to rise to 116° Fahr.; but between this and 260° there is a great gulf. When the air is moist evaporation is checked, and the human system has greater difficulty in keeping its temperature down to the normal.

Birds have, probably, nerves or nerve-fibres similar to our own exercising a control over their temperature; but evaporation from the skin cannot go on except to an inconsiderable extent since they have no sweat glands. Radiation on any large scale is prevented by the thick covering of feathers; but it is a question whether one object of the bare patches, called apteria, may not be to allow of the free access of air. Their temperature is very high, varying from 100° Fahr. to 112°. Every warm-blooded animal, we may take it for granted, has an efficient heat-regulating system. The higher its temperature the more efficient, we may assume, the system to be. What, then, in birds takes the place of radiation from the skin? We may get a hint from watching a dog when taking vigorous exercise. Dogs perspire through the tongue, and also, I believe, through the feet, but not through the skin generally. The greatly-increased rapidity of breathing is the chief means by which they keep their temperature down. All the expired air is about the temperature of the body. From this the cooling effect of rapid breathing may easily be gathered; and here we have, I believe, the explanation of the great size of birds' air-sacs, and also, perhaps, of the long ramifications of the trachea which we find, for instance, in the crane. The greater the amount of air breathed in and out, the more the body will be cooled. In man and in birds the method is in reality the same; but in birds evaporation and radiation take place mainly from the lungs and air-sacs, in man mainly from the skin. If we watch a bird's breathing we obtain evidence of this; when standing still it breathes about 20 times in the minute, whereas an adult man in a sitting posture takes only from 13–15 breaths in the same time. During flight a bird's breathing must be far more rapid, and it is impossible to avoid the conclusion that its temperature is kept equable by this means.

There remains the difficult question of the aëration of bones. It is possible that this also may be of some slight use in the regulation of temperature; but if this is so, it can only be a very secondary object, since the air cannot be expelled from them at will. Before proceeding to discuss the problems connected with pneumaticity, I will briefly set down the main facts.

(1.) Many small birds that are first-rate flyers have either marrow in all the large bones, or else in all except the humerus.

(2.) Most of the big strong-flying birds have a great deal of aëration.

(3.) The hornbills, which, according to good observers, are very poor flyers, are as pneumatic as any birds, or, perhaps, more so than any.

(4.) Birds which dive have solid bones or only the humerus aërated.

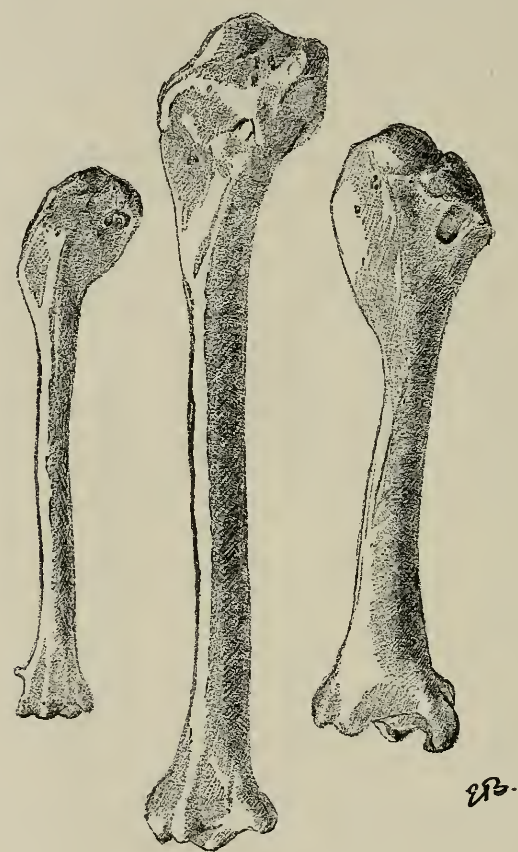
(5.) Birds which spend much of their time in the water without diving have, at least in all the cases which I have been able to investigate, nearly all the bones solid.

(6.) There are great differences between nearly-related species, *e.g.*, the gannet has an extraordinary amount of aëration, while its near ally, the cormorant, has only the humerus pneumatic. The hornbill is not very distantly related to the swift, which is singularly deficient in aëration.

(7.) The bones of birds that are highly pneumatic are, relatively to their length, larger in girth than those of birds in which little aëration is found.

To find one's way through these facts is not easy. But one point must strike anyone. Great pneumaticity might be an inconvenience to a diver. As it is, he can regulate the amount of his body that appears above the water, sometimes sinking till no more than his head is visible. This, no doubt, he effects by driving the air from his sacs. But aërated bones would certainly not help him to vary his specific gravity, and they would make it more difficult for him to swim under water. Probably, the marrow in the bones serves a very important physiological purpose. Divers are frequently exposed to great cold when in the water. They are protected against this by a peculiarly thick coat of feathers, and by a deep layer of fat beneath the skin; and I cannot help thinking that the marrow also helps to maintain their warmth. It is held that in it a large proportion of the red blood corpuscles are generated, and unless they are very thick in the blood, a high temperature cannot be maintained. But if the marrow is a factory of red corpuscles, what substitute for this have birds whose bones are marrowless? Though, as a rule, exposed to less cold than water-birds, they have a very great power of generating heat. Birds, as a class, have more red corpuscles than any other animals. Is the spleen, the other great red corpuscle factory, more developed in birds which have little or no marrow? These physiological problems are of the greatest interest, and I only wish that I were in possession of facts that would throw light upon them.

Putting physiology out of sight, I am going now to consider why it is that, among birds of powerful flight, we find differences so great



a. b. c.

Humerus of Pomatorhine Skua (*a*), Vociferous Sea Eagle (*b*), and Rhinoceros Hornbill (*c*).

in the amount of aëration, and why such a poor flyer as the hornbill, is, in respect of bones, so well equipped for aërial navigation. To put physiology aside, is to assume that if hollow bones are advantageous to a bird, Natural Selection can bring it about that they become hollow, and that the bird is able to dispense with the marrow. This would indeed be a bold assumption, did we not know that it is an accomplished fact. The bones are hollow and the processes of life continue.

We shall find that as the wing lengthened, so as to make a longer and stronger stroke possible, the bones became larger in girth, larger not only absolutely, but in proportion to their length; and that a decrease of weight accompanied the increase of strength, mainly through the drying up of the marrow, but partly through a reduction, if we allow for the increased size of the bones, in the thickness of the hard, osseous shell. I shall give, first, a few measurements to show that in the case of birds whose bones have little or no aëration, the girth of the bones is, relatively to the bulk and weight of the body, considerably less—

		Girth of Humerus. inch.			Girth of Humerus. inch.
Bones highly pneumatic.	Screamer	$1\frac{5}{8}$	Bones very little or not at all aërated.	Logger-headed Duck	$1\frac{1}{16}$
	Rhinoceros Hornbill	$1\frac{3}{8}$		Scoter Duck.. ..	$\frac{13}{16}$
	Golden Eagle ..	$1\frac{5}{8}$		Nestor Parrot ..	$\frac{3}{4}$
	Vultur monachus ..	$2\frac{1}{4}$		Red-throated Diver	1
	Marabou Stork ..	$2\frac{15}{16}$		Spur-winged Goose..	$1\frac{5}{16}$

These measurements speak for themselves, even without any exact statement of the weight of the birds; but the following illustration and the accompanying tables will do more to explain the problem of hollow bones. The shoulder bones of a skua gull, which has scarcely any aëration, of an eagle and a hornbill, both of which are highly pneumatic, are placed side by side. The greater girth of the hollow bones in proportion to their length is at once clear. But to bring this out more clearly, I have taken the wing-bones of the skua as a model and calculated what would have been the length of the same bones and of the whole wing in the eagle and the hornbill, if they had been built on the same lines:—

	Girth of Humerus.	Girth of Ulna.
Pomatorhine Skua	$\frac{25}{32}$ inch ..	.. .. $\frac{5}{8}$ inch
Vociferous Sea Eagle	$1\frac{5}{16}$,, ..	.. .. 1 ,,
Rhinoceros Hornbill	$1\frac{3}{8}$,, ..	.. .. $1\frac{1}{4}$,,

	Humerus.		Ulna.		Aggregate length of Wing bones.	
	Actual Length.	Length proportionate to girth.	Actual Length.	Length proportionate to girth.	Actual Length.	Length proportionate to girth of Humerus.
Skua	$4\frac{3}{8}$	—	$4\frac{9}{16}$	—	$13\frac{5}{8}$	—
Eagle	$6\frac{3}{4}$	$7\frac{7}{20}$	$7\frac{15}{16}$	$7\frac{3}{10}$	$20\frac{7}{16}$	$22\frac{1}{20}$
Hornbill	$4\frac{5}{8}$	$7\frac{7}{10}$	7	$9\frac{1}{8}$	$15\frac{7}{8}$	$23\frac{49}{50}$

Thus, if in the eagle's humerus length were proportioned to girth, as in the skua's, the bone would be more than $\frac{1}{2}$-inch longer; on the same principle the aggregate length of the wing-bones would be greater by more than $1\frac{1}{2}$-inch. The increase of length in the case of the hornbill is far more startling.

If now we take a saw and cut the humeri of the skua and eagle from end to end, we shall find that the walls of the latter are not thicker in proportion to the greater girth of the bone. The girths are in the ratio of 25 : 42; while 3 : 4 represents the ratio of the thickness of the walls, the measurements being $\frac{3}{100}$ and $\frac{4}{100}$ inch. We can now see why small birds have so little aëration. In their case there would be no great reduction of weight, since the exterior shell of the bones forms a great part of their bulk. In the case of a larger bird, with bones many times multiplied in size, but the thickness of the walls increased very little, the removal of the marrow will be a great advantage. This will be clear if we take two cubes, a side of one of which is twice the length of a side of the other. Then the face is four times as large and the cubic content eight times that of the smaller cube. This will be true of other figures, so that if the average diameter of one bone be double that of another, and if the length also be double, its cubic content will be approximately eight times as great. And as the walls thicken very little with increased girth nearly all the enlarged interior can be filled with air. Clearly, therefore, a large bird has much more to gain by dispensing with marrow than a small one.

The eagle, then, has gained in lightness. It must also have gained in strength, for increased length of wing means an altogether disproportionate increase of work. The longer the wing, the greater the pace at which its extremity will move, and the resistance of the air increases as the square of the velocity.

The hornbills are a puzzle. The extreme shortness of the hand bones, a ridiculous anticlimax following upon so grand an ulna and so portentous a humerus, might suggest that they were once better flyers, and that the wing is slowly undergoing reduction. But the mountainous beak seems to show that colossal bones are an ancient heritage of the family, and that even feeble flight might have been difficult had they not become hollow. In either case they have been very great gainers by aëration.

Anyone who wishes to see clearly the relation in birds' bones of slimness to solidity, and of large girth to aëration, should inspect collections such as those at the Royal College of Surgeons, or at the Natural History Museum at South Kensington, where a large number, representing different families, may be seen side by side. It is easy, then, to see that big, long-winged birds have wing-bones thicker in proportion to their length in order to bear the far greater strain upon them: the aëration of the bones has obviated the natural increase of weight, which would have been a serious hindrance. But there remains the perplexing physiological problem: what organ of the body does the work that, in mammals, and presumably in birds with solid bones, is done by the marrow?

F. W. HEADLEY.

V.

On the Ætiology and Life-History of some Vegetal Galls and their Inhabitants.

THIS paper is to be regarded as of a preliminary character, merely treating of the subject of galls in its more popular aspects. It embodies the results of observations made by the writer in his scanty leisure of the past twenty-five years.

With regard to their origin, galls (in the restricted sense in which the term is here applied) are complex organisms, resulting from the co-operation of a plant and an animal; and to determine the extent and *modus operandi* of these two factors in their production is one of the many interesting problems which this study suggests, but for the solution of which no complete answer can as yet be given. Why, for instance, from the action of one species of insect, a large, irregular excresence should be produced; or why, from that of another, a smooth, spherical gall, or a scaly bud, or a circular disc, is a mystery which, for the present at least, science is powerless fully to unravel. It is, however, but a special instance of the universal problem, as to the cause by which normal organic structures are produced in normal organisms.

Though abnormal with regard to the plant, inasmuch as their presence is exceptional and foreign to the performance of its proper functions, galls, in themselves, are nevertheless as normal as any other organisms. Each has its own characteristic form, its special habitat, and its proper office.

Composed at the outset, like all vegetable growths, of cellular tissue, galls undergo more or less modification as they pass through the several phases of their life-history. Some, at maturity, are hard and woody; others soft and succulent. Their colours are bright red, or green, russet-brown, or white, or yellow; with, oftentimes, such nice gradations and harmonious blendings of all these, as to give to them the aspect of ripe fruit or quasi-flowers. Though, as a rule, more or less globular in form, their shapes vary considerably. Some are smooth and regular, others rough and amorphous. In one, we have the form of an elongated cone; in another of a cup or goblet; a third is urn-shaped; a fourth discoid; a fifth reniform. Some, as the oak-apple, are composed largely of a mass of spongy parenchyma;

others, like the woolly gall of the oak, are surrounded by a thick coating of cottony-down ; while in the case of the Bedegnar of the rose, we have a tangled mass of branching-filaments, compact yet free, and so compressible as to have obtained for this gall the names of the "Rose-Sponge" and "Robin's Cushion."

Few plants, it is said, are altogether free from these parasitic growths. Of those indigenous to Great Britain, a list of over 150 gall-bearing plants has been given. Such growths are found alike upon trees and shrubs and herbs. No organ escapes their deleterious presence. They appear on root and stem, on branch and leaf, on bud and flower, and fruit.[1] Every part, in turn, pays tribute to the invaders.

Of our native plants, the Oak (which is especially rich in varied forms of animal-life) produces the greatest number and variety of galls. From the oaks of Central Europe Dr. Mayr has described and figured ninety-eight specific galls.[2] Upon those of Nottingham and the immediate neighbourhood I and my friend, Dr. W. H. Ransom, F.R.S. (to whose initiative I owe my early interest in this subject), have noted some twenty-nine or thirty, out of a total of forty-one known British oak galls ; while upon a single leaf I have counted more than 2,300 distinct gall "spangles"—those of *Neuroterus lenticularis*. Though many tumour-like galls are due to the action of parasitic fungi, Acaridæ, and other as yet imperfectly classified organisms, the great majority are produced by various orders of Insecta. Among these, the busiest gall-makers are found among the Cynipidæ (or Gall flies), and the Tenthredinidæ (or Saw-flies), two subsections of the Terebrant Hymenoptera. The galls of these insects are (so far as I know) invariably closed galls, from which the larvæ, in some cases, and in others the imago—then furnished with mandibulate jaws—eat out their way, when mature, and escape.

In the case of the Cecidomyidæ (or gall-midges) and other families of Diptera, as well as among gall-producing Aphides—all of which have suctorial mouths—the galls, on the other hand, though in most instances closed for a time, decay or dehisce so soon as the contained insect has reached a stage when its exit from the gall becomes necessary ; but for this wondrous adaptation and provision, the home of the little occupant would, of necessity, become its prison-house and grave.

Limiting now our attention to a few only of the typical galls referred to, let us endeavour to trace out some of the more salient features of their life-history.

And first, of the "Oak Nut," or "Marble gall," as it is sometimes called. This is a simple, unilocular gall, due to the puncture of *Cynips lignicola* of Hartig, or *Cynips kollari* of Geraud, the largest

[1] Malpighi, quoted by Réaumur, vol. iii., p. 418.

[2] "Die Mitteleuropäischen Eichengallen," 1870.

member of our British Cynipidæ. It is said to be indigenous to France, and to resemble very closely the Aleppo gall, or galls of Commerce (*Gallæ tinctoriæ* of Olivier).[3]

It first appeared in this country, or was first noticed here, some 50 or 60 years ago, and was then limited to Devonshire and other of the South Western Counties. Gradually it extended itself inland, and is now, I believe, to be found abundantly in nearly all parts of Great Britain. It is the only one of the group to which the ink-gall belongs that occurs so far North as England or even Northern Germany.[4]

Each gall, with regard to position, occupies the place of a terminal or axillary bud, from the side or growing point of which it is developed; but I have occasionally met with instances in which one gall has grown out of or been superimposed upon another during its development. In its early condition, the gall is more or less cone-shaped, with pointed apex, and is slightly pubescent. It is then of a bright green colour, the surface spotted thickly over with crimson. As development proceeds, the crimson spots, which assume a scale-like aspect, separate wider and wider from each other; and the pointed apex, though persisting for a time, and in some instances altogether, usually disappears, leaving the gall at maturity more or less spherical in form, and lightish brown in colour.

In ordinary seasons, the young galls are first met with in July, and mature in August or September. This year, owing to the exceptionally bright weather of the spring, I found them as early as the 20th of June, and even at that date they had attained a diameter of from half to three-quarters of an inch.

The ovum of the gall-producing insect is, in this case, deposited in or near to the young bud of the oak at an early stage of its development, before any part of the cellular tissue is differentiated into leaves or other special organs; except, it may be, the few embryonic scales by which it is at times surrounded.

What is there, we may ask, in the casual presence of the little ovum; in the action of the developing larva; in the mechanical puncture of the parent cynips; or in the deposit of a tiny drop of irritating fluid by which it is said the ovipositing is accompanied; what is there, I ask, in any one, or more, or all of these, or, it may be, in the action of some other factor yet to be discovered, that impels those wonder-working changes by which the gall itself is produced, and by which its future growth and development are accompanied? What is it that, in the presence of this new agent, leads the succulent cells to resign their normal tendencies, and, instead of differentiating into

[3] The Gall-producer is *Diplolepis* or *Cynips gallæ tinctoriæ*.—"The gall of *Cynips tinctoriæ*," says Dr. G. L. Mayr, "occurs in the southern half of Europe, and near Vienna is frequently met with; that of *C. kollari*, however, is found as far as the German Ocean."—7 Entomol., 242.

[4] Zool. 4964, sec. 7, Entom., 245.

stem and leaves and blossoms, as otherwise they would do, to group themselves about this foreign organism, in radial and concentric layers, each endowed with new and peculiar properties, each destined to perform a new and special office?

Attempts have, of course, been made to answer these and cognate questions; but the explanations have, in great part, been little more than crude conjectures; and in no case, so far as I know, have they been of a character to meet, in any complete degree, the scientific requirements of the problem.

Redi[5]—who, like Von Helmont and the other vitalists, explained all organic phenomena by reference to the guidance of a distinct spiritual entity or Archæus—believed in a vegetative soul in each plant. This soul he considered presided at the formation of the egg, the insect, and the gall; and so determined their specific characters and their several relationships. Such views, at this day, we shall find it difficult to appreciate. They are, as Lucaze Duthiers has said, but the dreams "de l'esprit philosophique d'un autre temps."[6]

Réaumur, whose "Memoirs pour servir a l'histoire des Insects," (published A.D. 1738) contain so many curious and interesting details, was early led to refer the growth of galls to the suction of insects, having been himself much impressed by those produced by Aphides on the leaves of elms and limes. This suction, he believed, determined an increased flow of sap to the part affected, and, as a consequence, excess of vegetation and the production of the gall. As, however, both original puncture and subsequent suction not unfrequently take place in plants without any gall resulting, or without, in fact, any hypertrophy whatever, this can scarcely be regarded as the true explanation.

Neither, as Réaumur also supposed,[7] can the increased heat evolved by the developing egg be looked upon as a sufficient cause of the growth of the gall, inasmuch as no development, in the sense in which he used that term, takes place in the embedded ovum.

Malpighi,[8] who gave some considerable study to this subject, assumed a fermentation to be excited in the acid of the oak by the poison of the Cynips, and in this way sought to account for the production of the gall. He, however, like Redi and other of his contemporaries, was largely influenced by the prevalent spirit of the times, which saw, or thought it saw, fermentations in all things.

Lacaze Duthiers, whose "Researches on Galls," published in the *Annales des Sciences Naturelles* for 1853, give evidence of much painstaking study of this subject, adopts the hypothesis of a specific poison, a special *lignis virus*, as the main cause of the gall. It is, he says, a fact that all Hymenoptera have a poison gland connected

[5] Redi, born A.D. 1626.

[6] "Recherches pour servir a l'histoire de gallas," par M. Lacaze Duthiers. *Ann. des Sci. Nat.*, 3rd series, Botanique, vol. xix., pp. 284, published 1853.

[7] *See* "Memoirs," vol. iii., p. 478. [8] A.D. 1628–94.

with the ovipositor, from which it is easy to make them expel a drop of liquid by irritation. The presence of this fluid he considers to be the first essential fact in the order of causation; and he proceeds to illustrate its mode of action by reference to the analogy which he says exists between it and some well-established facts in animal and vegetable pathology. When the surgeon, charging his lancet with a drop of vaccine lymph, or with fluid " d'une ulceration syphilitique," introduces the point under the skin of a healthy subject, it is invariably followed, whenever morbid action is set up, by the reproduction of disease akin to that from which the virus has been taken, whatever may be the form of the incision, or the quantity of the fluid inserted. The specific quality of the morbid poison in these cases is, he says, an accepted doctrine—neither the result nor the evidence is questioned. Taking another series of facts, viz., those relating to the stinging of bees, wasps, scorpions, and other animals, he shows that the results, though in many respects akin to one another, are nevertheless specifically different. The swelling in one case is slight and temporary, in another it is large and persistent. It is the same with regard to the attendant pain. That resulting from the sting of a bee will continue for an hour or two, while, in the case of the scorpion, it may last for years.[9] No one, he says, denies the relation of cause and effect in such cases. Why, then, should we do so in that of the specific virus of the Cynips and the resulting gall? Once this first step taken, once the doctrine of a specific poison admitted, and we are in a position, he thinks, not only to explain the occurrence of galls in general, but to account also for their specific forms. No one, he says, has any difficulty in understanding how the normal forces of the plant vary with the poison, or how all secondary characters are due to the mode in which the poison acts in the developing vegetable tissues.

This view of Lacaze Duthiers is the one most generally entertained, and it has for its support the concurrent authority of Hofmeister,[10] and Darwin,[11] Sir James Paget,[12] Sir John Lubbock,[13] and other distinguished writers.

Whether or not, however, it can be regarded as the correct one, is, in a measure at least, doubtful. The analogy upon which it rests is by no means perfect. The presence of the ovum (not found in any of the cases referred to by Lacaze Duthiers) is, it may be, as necessary

[9] The injurious effects of the sting of the scorpion are said by Mr. Andrew Murray, F.L.S., to be much exaggerated, the pain being in some cases less than that of the sting of a wasp, and of incomparably shorter duration. *See* " Science Handbook of Economic Entomology—Aptèra." South Kensington Museum, 1877.

[10] " Allgemeine Morphologie der Gewächse," p. 634 (1868).

[11] " Origin of Species," 5th ed., p. 572 (1869), and 6th ed., p. 6. " Plants and Animals under Domestication," 1st ed. (1868), vol. ii., pp. 382–4 418.

[12] " An Address on Elemental Pathology," Brit. Med. Assoc., Aug., 1880.

[13] " On the Origin and Metamorphosis of Insects," *Nature* Series, 1874, p. 10.

a factor in the production of the gall as is the deposit of a specific virus; while it must be remembered that galls, in many cases, result from the action of other animals than Terebrant Hymenoptera, as, for example, of *Chærmes*, *Cecydomiæ*, and *Acari*, where no such poison-gland as that referred to exists.

Very early in our investigations Dr. Ransom and myself arrived at the conclusion that another agent, as potent as that of this hypothetical virus, was essential to the production of at least some species of vegetable galls, such agent being the presence and action of a living larva—a conclusion communicated by me in a paper read before the Natural Science Section of our then Literary and Philosophical Society, on the 4th of April, 1877.

Subsequently to that date, the subject received exhaustive study at the hands of Dr. M. W. Beyerinck, of Utrecht, whose "Observations on the Early Development-Phases of some Cynips Galls" were published by the Royal Academy of Sciences, at Amsterdam, in the year 1882. In these, Dr. Beyerinck, as a deduction from the like series of facts we had ourselves observed, submitted the opinion that, in the cases referred to, the action of the Cynips larvæ, and not the injection by the parent Cynips of a specific virus, was the sole cause of gall-formation.[14]

Whether so or not, is in a measure, at least, doubtful. This, however, I think we may safely conclude, namely: that while, on the one hand, in those chemical and other forces which produce growth, greater activity is induced by the stimulus of the injected fluid—assuming such fluid to be actually present—so, on the other, those mechanical conditions which determine form in organic beings are furnished, to a large extent, by the contact of the included ovum, and by the activities of the embryonic larva.

As the nut-gall approaches maturity—which, under favourable circumstances, it may do within a fortnight or three weeks of its first appearance, early in July—the cellular tissue of which it is at first composed becomes differentiated into five principal layers. The innermost of these, surrounding the central chamber, and known as the alimentary layer, is composed of thin-walled cells, filled with protoplasm, minute starch granules, oil, and albumen. This layer disappears *pari passu* with the developing larva, which finds in its substance the food-elements necessary to its growth. At first the chamber-walls are in direct contact with the very young larva, whose increase is, in the first instance, due to absorption or to suction.

Next in order is the protective layer, or *Couche protectrice*, built up of hard, compact cells, with strong, thick walls. Around these are arranged, in radial series, bundles of elongated cells, forming a sort of *chevaux de frise* or abatis—the whole constituting a fortified casemate or stronghold for the protection of the occupant and her commissariat

[14] *See* "Beobachtungen über die ersten entwicklungsphasen einiger Cynipiden gallen" von Dr. M. W. Beyerinck, p. 8.

from the attacks of parasitic and inquiline enemies. How far it is effectual for this purpose will be seen in the sequel. Together, these several layers constitute less than one-half the diameter of the gall, the remainder being composed of loose cellular parenchyma, rich in tannin, over which extends a thin epidermis, formed of flattened cells, and resembling, in general aspect, that found on other parts of the plant. The colour of the gall, when young, is due to the presence of chlorophyl granules in the *Couche cellulaire*, these being readily seen through the thin transparent cortex. Later on, these epidermal cells become thickened and coriaceous, and the colour of the gall changes to light brown.

Passing now from the consideration of the nut-gall to that of the oak-apple—the *Pomme de Chene* of Réaumur—we find, in lieu of a single central cavity, a more or less numerous series of closed loculi, each containing a single occupant, the whole embedded in a spongy mass of cellular tissue, surrounded by a common cuticle or epidermis. Like the nut-gall, the oak-apple has its origin in a bud, terminal or axillary, and usurps the place of what, under normal conditions, would be a stem or branch. It is among the best known of our British galls—appearing early in the spring, in some cases even before the scales of the leaf-buds separate or the first tender green of the year is seen. In favourable seasons it is met with as early as the third week in April, already a blush of crimson on its delicate cheek. About the middle of May, or by "Royal Oak Day"—a time when, according to custom,

> "Custom which all mankind to slavery brings,
> That dull excuse for doing silly things,"

many of our king-and-constitution-loving ancestors were wont to use it in the decoration of their May-boughs—the oak-apple attains, in favoured situations, its full development. In very fine specimens it measures from two to two-and-a-half inches in diameter; though in size it varies considerably, and in form is less regular and globular than the marble nut-gall.

About the last week in June, or the first in July, the normal insect—*Cynips terminalis* of Fabricius, *Teras terminalis* of Marshall—makes its escape from the gall, and, after a brief but active existence of four or five days, devoted to the intercourse of the sexes, sickens and dies,—a result to which, so far as my experience goes, there is no exception: not a solitary insect survives.

At the time of our first investigation of this subject, the generally-accepted opinion was, that the eggs of *Teras terminalis* were laid in the summer or autumn of the year in which the gall appeared in the spring; and this at a time when the axillary and terminal buds of the summer shoots, close shut in their envelope of scales, had yet so far differentiated as to show a well-marked series of embryonic leaves. To this view, however, a difficulty presented itself. Our investigations showed that, at the time the insect died, these buds had not attained that stage of development at which it was clearly seen they had arrived when the ovipositing must have actually taken place. Here, then, was a case in which fact did not agree with theory; our business, therefore, was, by further observation and experiment, to seek a solution of the difficulty. Accordingly, between the 16th of July and the 13th of December, 1876, we made a daily examination of buds from freshly-gathered twigs, by cutting these in section from apex to base, so as to expose the interior of the bud. No trace of eggs, however, appeared until the latter date, when the first group was met with. Subsequently, they were found in increasing numbers on each examination. In this way, between the dates referred to, I myself bisected 4,100 of these buds, many of them being examined under the microscope. Dr. Ransom, in like manner, opened 2,250, together 6,350 in all.

The sought-for result being thus arrived at, a new difficulty presented itself. If the gall-producing cynips died, as it was found to do early in July, how could it possibly lay its eggs in the December following? To this our own investigations gave no present answer; but a solution of the problem shortly came from Dr. Adler of Schleswig, whose researches on the alternation of generations of Gall Wasps (published in the "Zeitschrift für wissenschaftliche Zoologie" for 1880) established the fact that *Teras terminalis*, in issuing from the gall, proceeds not, as was supposed, to puncture the young buds of the oak, but to make its way to the roots of the tree, and there to deposit its eggs. In association with these a new and specific gall is formed, from which, in due course, issues a swarm of females (and none but females, for no male has ever yet been found) so unlike the parents which produced them, as to have been classed by entomologists, not simply as a different species, but as a distinct genus. These new husbandless females are altogether destitute of wings, no nuptial flight being called for, and have hence received the name of *Biorhiza aptera*.

Thus, between these two distinct and well-marked forms, we have, as you will see, a conspicuous case of heterogenesis, or alternation of generations—the bisexual giving rise to the agamic, the agamic, in its turn, to the bisexual.[15]

Let us now follow one of these agamic females, and see the kind of work she is called upon to do. Eating her way through the hard, subterranean gall in which she has passed her successive metamorphoses, she struggles forward through the intervening ground, creeping upwards to the now leafless branches of the oak. Here, by a dexterous use of her terebra, preparatory to ovipositing, she makes a transverse cut across the axis of a winter bud, above the circlet of

[15] In certain *Lepidoptera* (Psychidæ and Tineidæ) parthenogenesis appears to be a normal process; indeed, so far as known, the only process, for of some species the males have never been found. (Herbert Spencer's "Biology," vol. i., p. 215.) This agrees with *C. kollari* and other of the "Cynipidæ."

scales, so as completely to separate the cone-like apex with its appendages. In the space thus prepared (for the severed cone is still retained *in situ* by the enclosing scales), a variable number of eggs is deposited; sometimes, it may be, but a dozen or two; at others, as many as 200 to 270 or more. It is not altogether without wonder that one realises the possibility of a lodgment being found for so many eggs in so small a space, a space not one-fifth of an inch in transverse diameter across the bud, and less than one-fiftieth of an inch in depth.[16]

Each egg consists of a white, semi-transparent, pyriform body, with a long, silvery filament or pedicel at its smaller end. To the base of the detached cone, the distal ends of the several egg-pedicels are, as a rule, anchored, so as to leave the ova pendent, with their broad ends downwards, towards the fixed surface of the cut axis.

As winter passes away, and slumbering vegetation begins to feel the stir of new activities, a further phase in the life-history of the gall and the gall-insect commences. Fed by streams of nutritive sap which now circulate through the tree, the young, uninjured buds begin to swell, and the green axes to elongate.

With those, however, which have been severed by the cynips, another and a different result takes place. Should ova, notwithstanding the incision, fail to be deposited, or, if laid, perish during the winter, no growth, normal or abnormal, takes place from the divided axis. This remains brown, dry, and inactive. But if, on the other hand, healthy ova are present, and these, in due course, hatch out their living embryos, then, by the action of these upon the dormant tissues, new and peculiar powers of growth are manifested in the cut axis—powers which result in the production, not of a normal branch, but of an abnormal, tumour-like gall.

Cognate facts with regard to the galls of *Cecidomya verbasci*, found on the stamens of the Figwort and hoary Mullein (*Scrophularia canina* and *Verbascum pulverulentum*), have been noted by M. Leon Dufour.[17]

"From meteorological influences or other little-understood cause, it happens at times, he says, that the larva of this insect dies soon after leaving the egg. Then the parts in course of enlargement tend to recover; the fundamental excitement, which would be continued by the suction of the embryo, stops and fades; the swollen tissues (subjected again to normal physiological laws) contract and shrivel; the sap loses its morbid exuberance, resumes its normal course, and at last, though slowly, the stamens re-enter upon their generative functions, while the lobes of the corolla spread out and display themselves, and, in the end, recover their bright and glowing colour. Under other circumstances, where the death of the larva thus ensues—the

[16] In the ovaries of *Biorhiza aptera*, which gives rise to *Teras terminalis*, I have found 1,326 ova—in one instance as many as 1,570—the whole length of the abdomen being less than one-eighth of an inch, that of *Teras terminalis* only half as much.

[17] *Ann. des Sci. Nat.*, vol. l., 1846.

efforts of nature proving powerless to remedy the pathological turgescence—a veritable atrophy ensues, the stamens wither and the resiant gall languishes and dies."

The mode by which, in the case of the oak-apple, the essential changes are carried into effect and the development of the gall completed, is worthy our attentive study. I have already mentioned that, in ovipositing, the parent cynips so disposes of her eggs that, for the most part, they lie with their broad ends downwards towards the fixed surface of the cut axis. As spring advances, this surface becomes moistened by the rising sap; and, influenced by the process, the first range of pendent eggs is brought into direct and immediate relations with it.

About this time the embryo-larva hatches from the egg; that is, it perforates, by means of a dental apparatus already developed, the larger end of the shell, so as to come into contact with the moistened but still dormant tissue of the axis.

In this state of things new and important phenomena are exhibited. Under the stimulus of mechanical irritation, or, it may be, of chemical changes set up in the juices of the plant by a specific exudation from the embryonic larvæ, many of the cells about the woody axis become endowed with active powers of reproduction. Cell-multiplication thereupon ensues, and the formative tissue, insinuating itself between the anchored ova, gradually surrounds the escaping embryos, until each, embedded in the growing mass, is left sole occupant of its separate chamber.

In this process, the lowest eggs, or those with longest petioles, are, of course, the first to be overtaken, and, as a necessary consequence, their liberated larvæ occupy a place nearest the base of the growing gall. Others in turn are successively reached, until, finally, every embryo being embedded, the cellular mass fuses together, and presents to us the characteristic features of the oak-apple gall.

Here, then, we have a series of facts, both positive and negative, which point to the action of the embryo, and not to the deposit of a special virus by the parent cynips, as the direct and essential agent in the production of the gall. This agent, as will be seen, carries with it all the elements of a *vera causa*. It is, so far as my observations go, invariably present whenever an oak gall appears; and, though the exact mode of its operation is, for the present, undetermined, it may not unfairly be regarded as sufficient for the effect produced. This is not so with respect to the deposit of a specific virus. Many of the gall-producing insects and acaridæ have neither terebra nor poison gland; and yet, in presence of living ova, hypertrophy ensues, and a veritable gall appears.

Granting, for the sake of illustration, the existence and potency of such virus, ought we not, in this case, to expect that, even in the absence of egg or living larvæ, the normal energies of the fluid would be exerted, and, in the end, a gall—destitute though it might be of proper occupants—of necessity result? In my long experience, however, no facts confirmatory of this view have been discovered; nor is it probable that, under any conditions, such barren galls exist. Are we not then justified in discarding the hypothesis of a specific virus deposited by the parent cynips, and in attributing to the activities of the living embryos, combined, of course, with the normal forces of the plant, the genesis and metamorphosis of the gall?

Dr. Ransom, in his address as President of the Section of Medicine at the recent meeting of the British Medical Association at Nottingham,[18] defined a gall as a local hyperplasia due to the reaction of living cells to irritation. There were no grounds, he said, for adhering to the view, once held, that the parent insect deposits a virus with the egg. No galls were known to be due to any single act or impulse of any kind. The character of the reaction varied mainly with the irritant, but also, in a much less degree, with the tissues irritated. The irritant might inhere in the embryo *in ovo*, or in the free-hatched larva, or in both, or perhaps in an adult gall-mite, or in a mycelium, or in other more imperfectly understood organisms, animal or vegetal, if living; and although he granted the possibility that it was sometimes mechanical or physical, yet he thought it more probably always in some organic liquid, chemical substance, not very diffusible, produced in small quantities either continuously or at short intervals during a part of the life and development of the parasite, and having a different composition in each species.

Resting, as I think we may now do, on this solution of the problem, let us pass from the further consideration of the gall, to that of the gall-producing insect and its parasites[19]; and here we are met by one of the most curious episodes of entomological science, with a condition of things which, if not proved to be true, might well be regarded as a mere day-dream of the imagination. Nothing, in the prosecution of studies such as these, is more calculated to arrest attention or excite astonishment than the strict balance which is preserved in Nature among the various orders of animated beings. This is especially so with regard to the Insecta. Countless millions of insect-forms continually make their appearance upon the stage of existence, millions equally innumerable as constantly perish—"Comme si" (as Leon Dufour has said), "Comme si, dans le but des harmonies de la Nature, une loi de destruction devait contre balancer une loi de production"—as if the end of the harmonies of creation was, that a law of destruction should counterbalance a law of production—as if Nature, prodigal of her resources, created only to destroy; bent, as one might believe, from one aspect of her dealings, upon securing the greatest

[18] See *Brit. Med. Journal*, July 30, 1892.

[19] I desire here to acknowledge my obligations to E. A. Fitch, Esq., F.L.S., Maldon, Essex (one of the joint editors of the "Entomologist"), for the trouble he has from time to time kindly taken in identifying and naming for me the various insects resulting from oak and other galls.

happiness to the greatest number; while, from another, she would seem but to have set her heart upon devising and providing means by which to torture and destroy the sentient creatures she has made.

In this way, season by season, the greater part of the larvæ of insects, multitudinous often as the plague-flies of Egypt, are decimated by other larvæ, inquiline or parasitic —

"The unbidden crew of graceless guests."—*Virgil.*

which live upon their juices, or so rob them of their food, that they die of atrophy and inanition.

To realise this more fully, let us follow some phases of the life-history of *Cynips kollari*, the maker of the marble nut-gall.

And here, at the outset of our enquiry, we are met by the fact that no male member of this species has yet been discovered, the entire genus (in its present more restricted sense)[20] is, in fact, believed by Hartig and other competent observers [21] to be parthenogenetic or agamic. Whether this is so or not remains, perhaps, an open question. The evidence in its support is wholly negative; and though hundreds of thousands of galls of *Cynips kollari* have been examined by ourselves and others with no result but that of yielding females, the male element may nevertheless exist, and sooner or later be found, as in the case of the oak-apple and other now well-established instances, in a gall, differing in character, in date of maturity, and in position, from that in which the female appears.

Following, then, as we may now do, one of these presumably agamic females, we find her in due season puncturing the young buds of the oak, and depositing in each a single egg.

This action, apparently so simple, becomes, as we have seen, the initial factor in a series of organic perturbations, which result in the production of a gall of a persistent and determinate character. Were it not for our experience to the contrary, we might well believe that a home so strongly fortified as this is, and so lavishly provisioned, would, under all conditions of attack, hold out against invaders. This, however, is far from being so. The cynips larva, though hidden away in the very centre of the gall, is yet amenable to the incursions, the attacks, of numerous cruel enemies, for whom its property and its life become the necessary conditions of existence.

Of these, two well-marked groups especially invite attention. These are known as *Inquilines* and *Parasites*—the one phytophagous in habit, living on the vegetable tissues of the gall; the other entomophagous, or insect-eating, preying on the juices of the living host, which in this case feeds but to nourish the wolf within.

Early in the development of the gall and the gall-insect, these parasitic and inquiline enemies are alert and active. Guided by a marvellous instinct, by a perfection it may be of smell, or subtlety of

[20] This genus comprises more than 50 species.

[21] *See* Beyerinck, "Beobachtungen über die ersten entwicklungsphasen einiger Cynipidengallen," p. 139 (Amsterdam, 1882).

hearing, one of these entomophagous parasites (of which a type specimen may be seen in *Callimome regius*), discovers that a larva, condemned to become the living prey of her progeny, is at a sufficient distance from the surface of the gall to be reached by means of her fine abdominal terebra or ovipositor, with which she accordingly proceeds to puncture the gall, and on the body of the contained larva to deposit a single egg.[22]

In due course this hatches out its living embryo, the direct enemy of the legitimate possessor of the gall. This, feeding on the life-blood of its host, grows into a well-fed larva. The larva, in its turn, becomes a pupa, and the pupa an imago. Then, plying its mandibulate jaws, the mature insect proceeds to eat its way through the woody substance of the gall; and, clad in livery of golden sheen, with bright and iridescent wings, enters upon a new stage of existence; and, strong in the instincts of its race, begins, as its parents did of old, its insidious career of brigandage and death.

Nor are the dangers of the legitimate possessor of the gall limited by the operations of these sanguinary parasites. No sooner have the galls begun to grow, and the normal larvæ to feed upon the nutritive stores with which they are provisioned, than another group of enemies —near relatives of the Cynipidæ—are introduced, unbidden, into their dwelling-place, consume their food-substance, and shorten their lives. These are the phytophagous inquilines, which, pauper-like, avail themselves of others' labours, and live at their expense. Taught by an innate faculty that the home of the cradled cynips is furnished with provisions exactly suited to the early requirements of her own offspring, the vegetable-feeding *Synergus* usurps the well-stored sanctuary, and introduces there, not a single egg, as in the case of the *Callimome*, but a dozen or more, whence issues, in due course, a tribe of greedy larvæ that—to quote M. Leon Dufour—"vont realizer le '*sic vos non vobis*' de Virgile."

Thus the normal tenant of the gall, if not fortunate enough to escape, finds itself in this most dire dilemma—either to be eaten alive by its direct parasite, the *Callimome*; or to die of starvation from the action of its enforced messmate, the *Synergus*.

Nor are the ends of Nature yet fulfilled. The Inquilines, battening on their stolen viands, suffer, like the rightful owner, from parasitic enemies, which, consuming the invaders, become, in rough and barbarous way, the avengers of the dispossessed and suffering aborigines.

But here, again, as if to punish wrong, and work retributive justice, these parasites themselves are preyed upon by other parasites; losing thus in turn their own lives, as they before had sacrificed the lives of others. This is due, in great part, to the action of a new set of spiculiferous parasites, lower in the scale of creation than the

[22] *See* M. Leon Dufour, *Ann. des Sci. Nat.*, 3rd series, Zool., vol. I.

Chalcididæ, and which include in their number some of the most minute and slightly-formed of all the Hymenoptera. These are the Proctotrupidæ, several of which, belonging to the genus *Ceraphron*, are connected with the oak-apple, where I have found them in all stages of their development. Unlike the *Callimome*, these *Ceraphrons* do not lay a single egg on the *outside* of their host, but, on the contrary, deposit a number *within* its body. Here the contained eggs are shortly hatched, and the resulting grubs begin to devour the fatty substance of the attacked larva, until, in its dry and inflated skin, they find a comfortable, cocoon-like home in which to pass their further metamorphoses.

Perhaps, in their case, as in that of their predecessors the Callimomes, other families of entomophagous parasites attack and thin the ranks of the Proctotrupidæ, and in so doing maintain, though, as it would seem, under conditions of more than Bulgarian atrocity, the necessary balance of organic life.

I only ask, in presence of such facts as these, that amiable, but, as I venture to think, mistaken "Zoophilists," when they declaim against the cruelties of scientific investigators, and seek by penal legislation to check the progress of human knowledge, should bear in mind the actual course of Nature's dealings, and believe that in this, as in other things, Man, like the Creator,

> "From seeming evil still educes good,
> And better thence again, and better still
> In infinite progression."

G. B. Rothera.

VI.

Desert or Steppe Conditions in Britain: a Study in Newer Tertiary Geology.

THOSE who, without taking an active part in geological research or controversy, attempt to follow the latest results of the science, must often be struck by the curious way in which old theories fade and disappear, without being directly attacked. Such a fate seems to have overtaken the Diluvial Theory, notwithstanding certain recent attempts to revive it; we would now draw attention to the crippled state of the allied theory of the former existence of a "Pluvial Period." That a period of somewhat heavier rainfall may have existed during some part of newer Tertiary times, we are in no way concerned to deny; but as a question of evidence it is noteworthy that all the facts formerly relied on can now be shown to bear a quite different interpretation, and that the new facts accumulated during recent years tend to demonstrate the former occurrence, in place of the supposed Pluvial Period, of one or more periods of excessive drought.

Geological text-books still teach that the wide sheets of gravel found in the river valleys of the southern half of England, where only sands and clays are now deposited, point to a former excessive rainfall. Some of them even still suggest that the rivers once filled the wide valleys from bluff to bluff. The writers apparently have never tried to calculate how much water would be needed to fill these sloping valleys, and are unaware that a pluvial period with a rainfall of double or treble the present amount would be quite insufficient, and that at least one hundred times the present fall would in many cases be needed. The existence of these sheets of gravel, and the constant occurrence of deeply excavated valleys in regions where no streams now exist, constituted the evidence on which was founded the well-known Pluvial Theory.

Let us, however, try to put on one side all preconceived views and examine afresh the evidence. We will take first the fossils. After comparing a number of collections from various old river-deposits, and working out each one separately, it is possible to arrive at a clearer idea of the climatic conditions that prevailed than could be obtained by any mere examination of museum specimens or of lists of species. Among the more striking results of the study is the extreme rarity of fish remains, the comparative scarcity of truly aquatic mollusca, and the infrequent occurrence of perennial aquatic plants. The amphibious mollusca are common enough, especially such forms as can survive in the mud beneath a dried-up crust. With these amphibious forms are associated several continental species now rare or entirely extinct in Britain. We find, for instance, that one of the most abundant shells is usually the *Succinea oblonga*, now so scarce in Britain. We have also *Hydrobia marginata*, *Corbicula fluminalis*, *Unio littoralis*, and several species of *Helix*, all these forms having now disappeared from our islands. It might be thought at first sight that the assemblage pointed to a warmer climate; but a careful analysis of the list does not support this view, and the only character which the species possess in common is that all of them now live in sunnier and drier regions than ours, though not necessarily in warmer ones. The Pleistocene mammals found in Britain also point in the same direction, though not so decidedly, for many of them belong to extinct species whose former habitat is unknown to us. We notice, however, two or three species, such as the Saiga antelope and certain of the small rodents, which belong distinctly to the desert fauna of Central Asia. When the corresponding strata are traced eastward into Central Europe the evidence becomes much stronger, for Professor Nehring has discovered in Germany a mammalian fauna corresponding closely with that now inhabiting the Central Asian steppes.

If the loess of Central Europe be examined, it is found rarely to contain aquatic mollusca, but to yield in myriads such species as love sand-dunes and dust. The loess, as Baron von Richthofen has shown, is a desert-deposit such as now drifts before the winds in the dry regions of Central Asia. Thick dust-deposits like the loess do not extend so far to the west as Britain, where the climate must always have been comparatively moist owing to the proximity of the ocean; but in England there are inland deposits of blown sand, where sand does not now drift owing to the growth of vegetation, and these also probably point to drought. In the composition of our surface-soils we have also, I believe, evidence of the former wafting to and fro of fine material, which could not be obtained from the weathering of the underlying rock. The soil on our chalk Downs, for instance, is always full of small rounded grains of quartz, which cannot be derived from the underlying strata, for the upper and middle Chalk in this country do not yield anything but carbonate of lime, flint, and a little fine clay. The loess period seems to have affected Britain, though not so strongly as it did Central Europe.

The erosion of the valleys, undoubtedly exceptionally rapid during certain parts of the Pleistocene Period, and the formation of enormous sheets of valley gravel, remain, therefore, the sole evidence of the former existence of a Pluvial Period. Let us re-examine this evidence from a new point of view, and see what would be the necessary consequences of the existence of a cold dry epoch like that indicated by the fossils. In the first place, with a clear sky the winter must have been much colder, and all pervious strata would become frozen and impervious to a considerable depth. Any rain falling before the thick frozen layer had melted could not sink in, but would immediately drain off the surface, carrying away with it a thin layer of rock shattered by the frost. Thus, the chalk Downs, which now yield perennial springs, even after a drought such as that of the past summer, would then yield no springs, for the rain could not penetrate. When the surface was frozen, the heaviest fall of rain would be entirely thrown off these steep slopes in a few hours, and the Downs would become channelled by ravines containing impersistent torrents, such as we now only associate with mountain regions where the rocks are impervious. As soon as the torrents reached flatter open country, the material brought down would be deposited in fan-shaped deltas, like that on which Chichester is built.

Implements made by man are not uncommonly to be met with beneath such gravel deposits, and these discoveries are often, but to me unaccountably, taken to prove the former existence of rivers at spots now high up the slopes of hills, or even near the highest points of nearly level plateaux. From the occurrence of stratified gravels above the implements in such situations, it is further argued that since Palæolithic times deep valleys have been cut out, and what were formerly the alluvial flats, have now become outliers of implement-bearing gravel capping isolated hills. Many writers even reason as though Palæolithic man were an amphibious animal, unable to live far from a river, and absolutely proving from the abundance of his weapons at certain spots, that a river must once have existed in the immediate neighbourhood. If such were the case, Palæolithic man must have been very different from existing savage races of the arctic and temperate regions. An overwhelming desire for an abundant supply of water is not a marked characteristic of these, and, as a rule, a small quantity for drinking purposes is all that they require. We have evidence that Palæolithic man hunted the big game that was then so plentiful; but we have no evidence in this country that he was much of a fisherman.

The conditions under which the implements are found, often scattered over deeply-buried ancient land-surfaces, and associated with hearths and other remains, which prove that on that spot was the site of an ancient settlement,[1] are to me more suggestive of sudden flood action than of ordinary rivers. Is it not possible that Palæolithic man may have lived on the existing hills and plateaux, and that the gravel beneath which his remains are buried is merely a local flood gravel deposited in any hollow over the frozen slopes, and not necessarily proving the existence of any river at such heights?

[1] See Worthington G. Smith, ref. no. 6.

The occurrence in chalk Downs, and in hills of similar porous strata, of deep valleys in which no streams now exist even after the heaviest rain, and the existence of enormous sheets of irregularly stratified or rubbly gravels in the lowlands adjoining, instead, therefore, of constituting the strongest evidence in favour of the existence of a Pluvial Period, seem more probably, like the fossils, to point to the occurrence of cold desert periods, when the rainfall, though small, could act much more energetically as a denuding agent. Under such conditions it is possible to understand the correlation of a fauna poor in truly aquatic species with deposits indicating violent floods. The absence of subterranean drainage would not only cause violent floods, even with a small rainfall, but would lead to the disappearance of the springs and consequently of all perennial streams except such as drained a large area. This, again, would tend to emphasise the desert character of the fauna.

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Clement Reid.

VII.

The Genesis of Mountain Ranges.

THE Origin of Mountain Ranges is one of the most interesting problems in Geology. It is only of late years that anything worthy of being called a *theory* of their origin has been formulated. This is not surprising, because their internal structure as well as their external form had to be first known. American Geologists have had, perhaps, the best opportunities of engaging in this enticing study, hence the interest it has evoked has been greater on the other side of the Atlantic than here.

Among those who have given form to our ideas on the subject Professor Joseph Le Conte has been one of the foremost. It is therefore an important event when, as President of the American Association for the Advancement of Science, he chooses for his theme a review of what is already known of mountain structure, and an analysis and criticism of the leading theories that have been advanced to account for the origin of mountain ranges.

Professor Le Conte commences by drawing a distinction between what he calls a *Formal Theory* and a *Causal or Physical Theory*. The first must precede the latter, and while the formal theory according to his view is well advanced, the physical is in a very " chaotic state."

The *Formal Theory* is summarised as follows:—(1) " Mountain ranges while in preparation for future birth, were marginal sea-bottoms receiving abundant sediment from an adjacent land-mass and slowly subsiding under the increasing weight. (2) They were at first formed, and continued for a time to grow, by lateral pressure crushing and folding the strata together horizontally and swelling them up vertically along a certain line of easiest yielding. (3) That this line of easiest yielding is determined by the hydrothermal softening of the earth's crust along the line of thickest sedimentation. (4) That this line, by softening, becomes also the line of greatest metamorphism, and by yielding, the line of greatest folding and greatest elevation; but (5) when the softening is very great, sometimes the harder lateral strata are jammed in under the crest, giving rise to fan-structure, in which case the most complex foldings may be near, but not at the crest. Finally (6), the mountains thus formed will be asymmetric, because the sedimentary cylinder-lenses from which they originated were asymmetric."

Several American examples illustrating these views were then given, and it was shown that eruptive phenomena, faults, mineral veins, earthquakes, and other minor phenomena associated with mountains, are well explained by them. To quote Professor Le Conte :—" Leaving out the monoclinal type, which seems to belong to a different category, all phenomena, major and minor, of structure and of occurrences, connected with mountains, are well explained by the theory of lateral pressure acting on lines of thick sediments accumulated on marginal sea-bottoms and softened by invasion of interior heat. This view is, therefore, satisfactory as far as it goes, and brings order out of the chaos of mountain phenomena. It has successfully directed geological investigation in the past, and will continue to do so in the future." [1]

This is a statement of the theory with which Professor Le Conte's name has been associated, and it is one that is distinctly intelligible. It, however, contains in itself a proof of the difficulty of drawing a marked line between the *Formal* and *Causal*. The "*hydrothermal* softening of the earth's crust along the line of thickest sedimentation" is a *physical* assumption to account for certain phenomena. It is also a further *physical* inference that "*lateral pressure* acting on lines of thick sediments, accumulated on marginal sea-bottoms, and softened by the invasion of interior heat," accounts for all mountain structure excepting the monoclinal type.

I am not either affirming or denying the truth of these statements—into which I shall examine further on, but simply wish to show that the confusion of the two modes of thought complained of by Le Conte in other investigators has not been altogether avoided by himself; this being the case, it is only reasonable to ask if such a hard and fast division be possible.

Professor Le Conte next deals with *Physical* or *Causal Theories*, which he seems to limit to an enquiry as to the cause of the lateral pressure with which he builds his mountain ranges.

First he describes what is known as the *Contraction Theory*, which refers all lateral pressure of the crust to the *interior contraction of the earth*: a theory which is commonly known as the "Shrunk Apple Theory." It is this : The nucleus of the earth, represented by the interior fruit of the apple, shrinks in bulk, while the crust remaining of the same size adjusts itself to the contracting globe by folding and wrinkling, like the skin of a shrunk apple. It is difficult to understand, if the physical theory of the hydrothermal softening of the crust can be included as part of a formal theory of mountain building, why the contraction theory providing lateral pressure should be put in another category. To my mind, the contraction theory is as formal as the theory of mountain building we are considering, especially as at first understood; for it was founded upon

[1] *Nature*, October 5, 1893, pp. 551-4.

an assumption afterwards proved incorrect in several important particulars. It is impossible to state any theory without involving cause and effect, and what is not observation in these theories is mostly physics.

After explaining the contraction theory, Professor Le Conte gives a summary of the objections to it, and a formidable list it is—too extensive, indeed, to enumerate in the space I limit myself to. Many of these difficulties have been dealt with in my "Origin of Mountain Ranges," especially the most salient of them, based on the existence of a level-of-no-strain at no great depth in the crust of the globe, above which the crust is in compression and below in tension. Le Conte evidently attaches great importance to this principle, and he may well do so, as it clearly follows that the bulk of the crust in compression is quite inadequate to account for the irregularities of the earth's surface.

Reade's Expansion Theories are next dealt with, and Professor Le Conte honours me with a generally fair formal statement of my views. He feels, however, that he does not thoroughly grasp my meaning in several important particulars, more especially in relation to the cumulative effects of recurrent expansion. It would savour of self-conceit to repeat my views here, indeed it is unnecessary, as the theories and investigations can be perused in their original extended form, by which a more just conception of their value, whatever that may eventually prove to be, can be formed.

After stating the objections to Reade's theories, which will be dealt with further on, *Dutton's Isostatic Theory* and *Reyer's Gliding Theory* are explained and dismissed—Le Conte finally, whether we may agree with him in whole, or part, or entirely dissent, concludes a very able address, written in the clear and charming style peculiar to himself, with the following sentence: "After this rapid discussion of alternative theories, in which we have found them all untenable, we return again to the contraction theory, not, indeed, with our old confidence, but with the conviction that it is even yet the best working hypothesis we have."

Before criticising the theories of this able investigator and writer, many of which are quite individual and original, I think it will be best to dwell first upon those general principles in which we are in agreement. They are these: (1) The leading principle, and one first stated in America by Professor James Hall, is that mountain ranges are built up out of immense thicknesses of sediment; (2) that there is a relation of cause and effect between sedimentation and mountain building which Le Conte attempts to explain in one way consistent with the Contraction Theory, and I in another in more direct relation to my Expansion Theory. This is a very important agreement, as there are still some, though in a minority, who are not prepared to admit either the fact or the relation. It is nearly seven years since I published my work, and the additions to our knowledge, too numerous to mention here, all point to the fact that Mountain Ranges are built out of thick sediments. To go no further than our own island, Sir Archibald Geikie has clearly shown that the range of mountains in the Highlands of Scotland, the remains of which the Geological Survey has been investigating for many years, were built up of sediments many thousands of feet thick. It is the same, so far as is known, with all the existing great mountain ranges of the world, such as the Alps and Appalachians, the sediments of which they are composed being in many cases estimated at from 8 to 10 miles thick; (3) the accumulation of sediment, together with a consequent sinking of the sea-bottom, leads to a rise of the isogeotherms which first affect the strata of the crust on which the sediments repose, and next the sediments themselves.

These are the three leading principles in which we agree, but there are also minor points in which our ideas run parallel or partially so, but it will hardly be necessary to state them here.

But while our main principles so far run abreast, I attribute the elevation and folding of the chains in their initial stages, not to the contraction of the earth, but to the internal expansion caused by this heating of the sediments and the crust of the earth upon which they repose. These effects are intensified and continued until the building of the range is complete by other related and cumulative causes. It thus becomes a case in my view of *direct* cause and effect. Professor Le Conte apparently thinks that this expansion will have no effect at all in the way of elevation or folding, relying upon a criticism of Davison, which I shall presently deal with in the form stated in Le Conte's own words. The whole cause of both the folding and elevation is assumed to be in Le Conte's *Formal Theory* lateral pressure, and in his *Physical Theory* this lateral pressure is assumed to be due to the contraction of the earth. The hydrothermal softening of the earth's crust under the line of thickest sediments, in his view, determines the concentration of this lateral pressure in the localities in which it takes place.

Granting all the postulates involved, we here undoubtedly have a machinery which looks effective for mountain-building; but it is one thing to make a formal statement which is sufficient, and another to make its assumptions square with physical facts.

The theory was undoubtedly in better case when geologists were content to look upon the earth as in two simple conditions, namely, possessing an *unshrinkable crust* of unknown thickness—that is, of any thickness required for theoretical purposes—entirely in compression, and a *nucleus* whose function is to shrink. Such theories, even if afterwards proved to be wrong, are of value in focussing thought; they are often stepping-stones to the truth. When, however, we come to apply quantitative analysis, with the help of the physical knowledge of the day, to the Contraction Theory the problem assumes a different complexion. We find that the shrinking nucleus is but a shrinking shell, in thickness some one-twentieth part of the earth's radius, while the underlying real nucleus is a heated globe of some 7,500 miles in diameter, which has not parted with its original heat or shrunk at all. The crust in compression is also found to be only a few miles thick, the compression being most active at the surface, gradually diminishing to nothing at the level-of-no-strain. These conditions of strain and stress in a cooling globe being pretty generally accepted by physicists of the day, Le Conte seeks to eke out the amount of contraction required by the theory in other ways. They are, however, so far, but formal assumptions having very little foundation in physical facts, and may fairly be dismissed until some better proof of their existence is forthcoming.

Let us, however, grant the lateral movement required by the formal theory, and consider how such machinery would be likely to work.

It is difficult to conceive in what way the lateral pressure of an enormous shell, 8,000 miles in diameter, could be concentrated in one or two places by any softening of strata under incipient mountain ranges, for it would involve a torsion, in varying degrees, of the whole crust, and a shearing over the whole area of the nucleus on which it rests; but this difficulty has been already dealt with by Dutton, so we will not dwell upon it. When, however, we come to consider that the application of the force is *external* to the lenticular plate of sediments to be acted upon, our difficulties increase. A softening of the crust along a certain axis might determine an elevation and folding in its immediate vicinity, but it would hardly shade off into those regular folds over a great area of country which are nearly always present where former sedimentation and upheaval has taken place. This would involve a refined gradation of softening that is not likely to occur in nature. I have also in my work dwelt on the fact that the pressure which has produced mountain ranges must have been internal and *centripetal*, and have illustrated this idea by reference to the domed form of anticlinals, varying from almost a segment of a sphere through an ellipsoid to an inverted canoe-form with their accompanying similarly-shaped but inverted synclinals.

It is, perhaps, only my individual opinion, but it seems to me improbable that the earth's crust could, by hydrothermal softening, be led into the symmetrical structures which characterise its surface configuration.

If, however, we look upon the pressure as *internal* to the folded area, the difficulty vanishes. A heating of the strata and crust would produce a state of compression acting throughout the mass from atom to atom, and greatest where the heat is greatest. This, it appears to me, is what is wanted to produce a folded mountain chain, and it will, as I have attempted to show in detail in my work and various papers, originate those great features of folds, foothills, and granitic and gneissic axes distinguishing all great mountain chains.

But, say my critics, your theory does not give lateral movement sufficient, we want much more; to which I have replied and sustained the affirmation by figures, that it gives more lateral movement than any of the other contending theories. I have also shown, and this view has been endorsed by no less an authority than Stefani,[2] that the amount of lateral movement required for mountain building is very much less than what is given by measuring the folds of mountain ranges and comparing their length with the base line of the chain. The assumption that the difference represents the amount of movement that has taken place is fallacious, because the reproduced arches never existed, and the strata have been lengthened by rolling out.

The subject of mountain building is so intricate and complex, and has so many aspects, that it would take much more space than is at my disposal to do justice to it; but before concluding these observations it will be necessary for me to reply to the objection considered by Le Conte to be fatal to my theory. He says, "But the fatal objection is that brought forward by Davison. It is this: sedimentation cannot, of course, increase the sum of heat in the earth. Therefore the increased heat of the sediments, by rise of isogeotherms, must be taken from somewhere else. Is it taken from below? Then the radius below must contract as much as the sediments expand, and therefore there will be no elevation. Is it taken from the containing sides? Then the sides must lose as much as the sediments gain, and therefore must contract and make room for the lateral expansion, and therefore there would be no folding and no elevation. I do not see any escape from this objection."

This, in my view, is a *formal* objection, and *formal* only. The criticism is underlaid by the profound misconception that the sediments *abstract* heat from the earth. So far from this being the case, *they conserve it and retain heat that would otherwise be wasted into space.* They act as a blanket or top-coat would do to the human body, as pointed out by Herschell and Babbage. The radial contraction of the earth is less under the blanketing sediments than under the other portions of the crust. Looked at in even this partial manner, there is practically no difference between a radial expansion of a part of the globe under the sediments—the only expansion apparently contemplated by Le Conte as effective—and a radial contraction of the surrounding crust of the earth. It is a question of *relative* movement and of *differential*, not absolute, heating. Increase of the sum of heat in the earth is entirely beside the mark. The answer is therefore simple and conclusive. The sediments neither abstract heat from below nor from the sides, they conserve and utilise what would otherwise be wasted. The flow of heat from below could not be quickened by the laying down of sediments, unless they were at a lower temperature than the surface of the earth's crust on which they were deposited.

[2] "Le Pieghe delle Alpi Apuane," p. 110, Firenze, 1889.

To be effective, the difference of temperature would have to be great —something quite impossible.

But it is not on radial expansion or contraction that the theory of the origin of mountain ranges bearing my name hinges. Reasoners on such complex problems should have the conditions fully and accurately before them.

A well thought-out theory cannot be gaily dismissed in a few short formal sentences.

The horizontal extension of aggregate groups of sediments compared with their maximum vertical depth is enormous; it may be 200 times as great. Consider what would be the result of heating a slab from below measuring 200 feet at the sides and 1 foot thick in the thickest part. If it were bound at the sides in the way the sediments are bound on the solid crust of the earth, the effect would be to throw it into domicular folds distributed in relation to the differential heating and thickness of the slab at various points. Even if it were not so held, the differential heating—the temperature being most intense at the thickest parts—would still throw it into folds through the intense stresses and strains set up. This is what happens in my view by the heating of the sediments which afterwards become mountain ranges. It initiates the movement, liberating other forces which continue the building of the chain. I cannot pretend to go into the details here, as they are of considerable complexity, and must refer those who wish to pursue the subject to my work and the several papers I have at various times during the last seven years given to the scientific world.

I must, however, before concluding, say a word on cumulative recurrent expansion, as Professor Le Conte here finds my theory obscure. This seems the more strange to me, as I actually commenced my work by giving concrete examples of the effect of recurrent expansion due to alternate heating and cooling. The folds in this way induced in sheets of lead are given as good examples and illustrated by photographs. Anyone who cares to look for such folds can see them in any lead gutter, lead-lined bath, or lead-lined sink exposed to varying temperatures. It is one of the plumber's arts to provide as far as he can for this well-known effect of frequent minute expansions; but I have shown that such effects are not confined to so ductile a material as lead. Terra-cotta copings are frequently affected in a somewhat similar manner. They go on expanding for years by infinitesimal degrees, mostly in the heat of summer, until I have seen what is technically called a "ramp"—a portion of the coping curving down to a lower level—lifted from one to two inches from its bed. Not only so, but in some cases a time comes when the bending at this point becomes a fault at one of the joints (normal fault), the expansion still proceeding. I have had a very interesting letter and photograph from Mr. John D. Paul, F.G.S., Leicester, recording an independent observation of his own on a coping at Leicester, showing a similar result, which he attributed to alternate heating and cooling by ordinary changes of temperature. The reason why this sort of expansion shows so much in terra-cotta copings, is because the joints are made in cement, and the whole becomes a long bar, alternately exposed to the vertical rays of the sun and to the cold.

Finally, Professor Le Conte seems to think that if expansion produces mountain elevation, contraction should bring the strata back again to the same level. This is a very common criticism with my objectors, and is due to a misconception born of looking at things in the gross—and, if I may venture to say it—in too formal a manner. The reason why the strata on contraction do not go back to their original form and level I have explained over and over again. It is this, no force on earth can pull strata back that have once been folded. The material by every expansion, however small, is pushed forward, and accumulates mostly in folds, so that the mountain range is actually piled up, by transfer of material from one locality to another, just as much as if it had been done by means of navvies and wheelbarrows.

To give another illustration, landslips are of frequent occurrence, and have been seen, or their effects seen, by many. One of those effects is to push up a fold, or pile up any loose materials, such as a shore deposit, before it. One might as well expect this material to go back again, and spread itself out over its old bed if the landslip were removed, as to expect mountain strata to do it, whether piled up by horizontal or cubical expansion, or pushed up laterally in any other way.

If thinkers would not concentrate their minds upon *vertical* expansion to the exclusion of *horizontal and cubical*, they would, I think, find no difficulty in understanding one of the leading ideas of my theory. In consequence of the extended areas of the sediments compared to their vertical thicknesses, horizontal expansion is infinitely greater in its effect than vertical.

When, however, all is said, there is no gainsaying the fact that the Origin of Mountain Ranges is a subject involving intricate reasoning, and cannot be disposed of by formal statements, however clear; for their clearness frequently arises from the exclusion of conditions which ought to be considered.

My object in making these explanations and criticisms is entirely to further the cause of a science which I have deeply at heart, and which, for its progress, is dependent on the labour of many. No one can rejoice more than I do at the valuable contribution towards the elucidation of so difficult a subject as the Genesis of Mountain Ranges with which the veteran Le Conte has favoured us.

T. MELLARD READE.

VIII.

Indexes to Botanical and Zoological Nomenclature.

THE compilation of indexes by means of which a student can find an accurate reference to the first description of a generic or specific name, entails a devotion to the subject known to but very few. To deliberately determine to spend some fifteen or twenty years of a lifetime in order that other students of science may be spared the time occupied in hunting up descriptions in more or less obscure publications, is a species of insanity that one could only wish were more common. The appearance of the first volume of Mr. Daydon Jackson's index to the genera and species of flowering plants, shows that it is possible to accomplish what appeared at first a hopelessly gigantic task, if one has the requisite energy, is provided with the funds, and is able to devote the greater part of his time to the undertaking.

In 1840–41 botanists were presented by Steudel with the "Nomenclator Botanicus," a list of plant names then supposed to be of specific value, together with their synonyms. In 1855 G. A. Pritzel published his "Iconum Botanicarum Index Locupletissimus," containing a reference to the first description of all the then known flowering plants and ferns, and he issued a second edition of this work in 1861; but botanical literature had grown to such an extraordinary extent by twenty years later that Darwin found to a worker like himself, living far from museums and libraries, that it was a most difficult task to get information as to new species and to the place of publication. He accordingly discussed the matter with Sir Joseph Hooker, who put the idea into form, and made arrangements for the manuscripts to be stored in the Herbarium at Kew. Darwin and his friend George Bentham agreed to supply the funds necessary to ensure the continuous working of the scheme, and Mr. Benjamin Daydon Jackson was chosen to carry it out. Mr. Jackson accordingly, in 1881, started the compilation, arranged for a special staff of clerks to assist him, has carried the work triumphantly through, and presented botanists with Volume I. (from A—Dend.) last month.[1] The successful termination of so grand a work

[1] "Index Kewensis plantarum phanerogamarum nomina et synonyma omnium generum et specierum a Linnaeo usque ad annum MDCCCLXXXV. complectens nomine recepto auctore patria unicuique plantae subjectis sumptibus beati Caroli Roberti Darwin ductu et consilio Josephi J. Hooker confecit B. Daydon Jackson." Fasciculus I. [Aa—Dendrobium]. 4to. Oxonii e prelo Clarendoniano. MDCCCXCIII.

must be a matter of supreme satisfaction to its compiler, and in congratulating Mr. Jackson, one congratulates also the botanical world on the immense labour saved to them in having the "Index" for reference.

While, however, giving Mr. Jackson the lion's share of the credit for this work, he himself would certainly be the last man to ignore the help of others. To the enthusiastic little band of clerks (despite the fact that they were paid for their work) a large amount of thanks are also due; while of his other helpers Sir Joseph Hooker has read the whole of the proofs and annotated and corrected the MS. as only one of his large experience could do. From the Kew authorities and from the staff of the British Museum (who have been supplied with proofs), Mr. Jackson has received invaluable assistance, and the same may be said of officials of the numerous public Herbaria, and many private collectors.

It is, perhaps, not too much to say that it is only in London that so gigantic a task could be successfully terminated in so comparatively short a time. The libraries at Kew, the collections at the British Museum, Bloomsbury (where are stored the books of Sir Joseph Banks), and the still more complete library of the Natural History Museum, present a series of botanical books equalled by no other city in the world; and these have all been fully drawn upon. Further, there are the collections of the Linnean and the Royal Societies, besides the rich stores of volumes in the hands of private individuals, all of which have been placed unreservedly at Mr. Jackson's disposal.

As a commencement of work, the generic names from Bentham and Hooker's "Genera Plantarum," were arranged in alphabetical order, and the names in Steudel's "Nomenclator," intercalated; each genus was placed in a separate wrapper, and housed in boxes with a fall-down front, so as to allow of easy reference and insertion of any new names that were indexed by the workers. These boxes of MS. ultimately reached the number of 168, and were arranged in pigeon holes.

The "Index" includes all names of flowering plants and ferns from 1735, the first edition of Linnæus's "Systema Naturæ," down to the end of 1885, and the estimated number of entries reaches 500,000.

The following are examples of Mr. Jackson's entries:—

Acnida rusocarpa, *Michx. Fl. Bor. Am.* ii. 234. t. 50.—Am. bor.

Beesha capitata, Munro, in Trans. Linn. Soc. xxvi. (1868) 145.—Madag.

Cyperus leptostachys, Vahl, ex Kunth, Enum. Pl. ii. 93 = distans.

Printing began in the autumn of 1891, and has now reached the letter K, but owing to the extreme care bestowed on the proofs, it has only been possible to issue the first part at present. The book will be completed in four parts, and it is anticipated that the whole will be issued to the public by the end of 1894. One thousand copies will be published at a price of eight guineas nett (two guineas each part). It is sincerely to be hoped that so monumental a work will not be allowed to stand still, but that it will be kept up-to-date in manuscript, both at Kew and at the British Museum. Indeed, nowadays, it is the obvious duty of a public institution to save the valuable time of its own staff by arranging for a special department where information of this kind can be obtained for the asking.

It is, perhaps, in a general review of this kind, somewhat carping to criticise so great a result, but one is impelled to notice the very serious oversight of the omission of dates in all but those entries relating to serial publications. It would have conduced to far greater perfection had it been realised that the date of a generic or specific name is of considerable importance; so considerable is it, indeed, that it is perfectly incredible that it should have been left out. But Mr. Jackson can remedy this if he has still the energy and the means to do it, by issuing with volume 4, at a slightly extra cost, a complete list of the abbreviations used in alphabetical order, together with the date and a brief extension explanatory of them. We trust he will see his way to do this. For a criticism on the scientific aspect of the work, we need but refer to a masterly article from the pen of Mr. James Britten in the *Journal of Botany* for October; with this criticism we are quite in accord. The introduction of the word "Kewensis" strikes us as unnecessary, and we would like to have read Jackson and Hooker on the cover, instead of Hooker and Jackson, but this without the slightest disrespect to Sir Joseph Hooker.

To turn to Zoology: up to the time of Louis Agassiz, zoologists had practically no book to refer to that would assist them in discovering the literary whereabouts of a generic name. Gmelin, in his edition of Linnæus's "Systema Naturæ," 1788–1790, had published a complete index to the many thousand species described in that work, but it was reserved for Louis Agassiz to publish a "Nomenclator Zoologicus," in which he gave a full reference to every generic name he could find that had been used up to his day (33,000 entries). Nothing further was available beyond the lists published in the yearly volumes of the *Zoological Record* (vol. vii. and onwards), until 1873, when August Marschall published his "Nomenclator Zoologicus," a book containing much new matter, but, unfortunately, in many respects inaccurate. On October 9, 1879, a letter appeared in *Nature* from Samuel H. Scudder, containing a notice that the writer had arranged and collated previous lists of generic names, and asking assistance towards its completion. In 1882 subsequently appeared Scudder's "Nomenclator Zoologicus, an alphabetical list of all generic names that have been employed by naturalists for recent and fossil animals from the earliest times to the close of the year 1879." This work was divided into two parts; part 1 containing a full reference to all generic names not in Agassiz, Marschall, or the *Zoological Record*; and part 2, "Universal Index," an index giving the name, author, and date of every generic name, so far as the compiler had collected, which had been used for an animal from the earliest times to about 1880. This book, which contained 80,000 references, though necessarily incomplete, was at once recognised as of extreme value, and one may say without exaggeration is absolutely indispensable to zoologists. Scudder has promised a supplemental list of generic names discovered to have been omitted, but this has not yet appeared.

None of these books, however, offered those advantages to the zoologist that Pritzel offered to the botanist. They dealt with generic names only, while Pritzel dealt with specific names. The example set by Mr. Jackson in botany was followed by Mr. Davies Sherborn, who, on 15 May, 1890, published a scheme in *Nature*, for an index to the genera and species of all known animals. With some slight modification of the scheme, work was commenced on July 1, 1890, and has steadily progressed ever since. Accommodation for the MS. was provided at the Natural History Museum by the Director and the Keepers of Zoology and Geology, and the MS. is available for reference in alphabetical order of genera to anyone who desires such information. The plan of compilation adopted by Mr. Sherborn differs somewhat from that of Mr. Jackson. Every species has a separate and distinct slip, and every reference is taken from the original source; a volume is systematically searched page by page, and every species extracted, so that once that volume has been indexed, the compiler never requires to see it again. The slips are all made in duplicate with transfer paper, one set being sorted up into the great alphabet of genera, and the other set tied up and put away under the author of the book.

By this method not only is one able to see the whole of one author's work collected together, but this second set of slips will remain clean and intact for the final sorting, if publication is ever reached. Synonymy has not been attempted in the strict sense of the word by Mr. Sherborn; that, in his opinion, is more the duty of a specialist, but every time a species-name has been placed in another genus a reference is given, so that when complete and arranged the searcher will be able to find the history of that species-name without trouble, through all its generic vicissitudes.

The following are examples of the entries :—

subterranea Anas, J. A. Scopoli, Annus i. 1769, p. 67.

vampyrus Pteropus (L.), C. Illiger, Prod. 1811, p. 118 [Vespertilio].

panthera Felis, J. C. D. Schreber, Säugth. iii., p. 384 (1777) and 586 (1777), pl. xcix. (1776).

Working alone, and with one small grant from the British Association, the compiler has already collected about 90,000 references, and as the books are taken as far as possible in order of date from the earlier to the later, a considerable number of the rarer and more obscure publications are already indexed and available. The references are taken from 1758, the date of the tenth edition of the

"Systema Naturæ" of Linnæus, that having been found the most convenient starting point and more in accord with the ideas of foreign as well as British naturalists. The system above described (the completion of one book at a time) is apparently an especially good one, for it permits the MS. to be always complete as far as it goes, and will enable any person, at any moment, to take up the work, should it unfortunately be found necessary.

A still more gigantic proposition has been advanced by the German Zoological Society, which proposes not only to form a catalogue of all described species, but to give a short diagnosis of each as well! The vastness of this compilation overwhelms one, but that indefatigable indexer, Professor Victor Carus thinks well of the scheme. This could not possibly be done by one man, and the notion is, that specialists in every branch of natural history might undertake some family or even genus alone. The final details of Professor Carus's scheme are unknown to us, a brief notice only having as yet appeared in a recent number of the *Zoologischer Anzeiger.*

IX.

The Wilds of South-East Africa.[1]

IT is not difficult to prophesy that this handsome volume will be a distinct and immediate success. The public have anxiously waited to hear what Mr. Selous had to say on the Mashunaland question, and they receive the information at the very nick of time. But it is to a far larger section than to the political public that this book will appeal, for the boy will welcome Mr. Selous' volume with its stirring adventures as a companion volume to his "Robinson Crusoe," while the man will read it with the pleasure derived from his knowledge that it is written by one who knows the lions, elephants, and other large game as perfectly as he knows his horse.

Mr. Selous left England in November, 1881, for the Cape, and travelling on horse by way of Kimberley reached Klerksdorp, sleeping sometimes in the open, sometimes in the houses of the Boers, according to the weather. Purchasing a waggon and a pair of oxen and taking with him several Matabili boys from Klerksdorp, the traveller started for the interior, passing through the Matabili country and reaching the junction of the Loangwa and Zambesi rivers on 22 June. Here a camp was made, and for the next six months Selous was hunting and travelling in a country where lions were always prowling about, though nowhere plentiful.

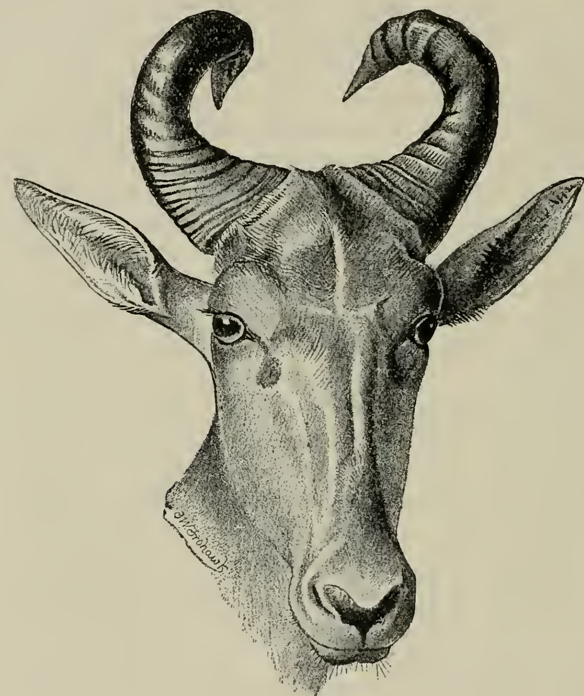
Returning to Klerksdorp to pack and despatch his collections to England, the traveller laid in a fresh stock of provisions and trading goods, and by May, 1883, had once more entered Mashunaland, and pitched his camp on the Manyami River. In July he made a start south for the Sabi River in search for the white rhinoceros (*R. simus*) and Lichtenstein's hartebeest (*Alcelaphus lichtensteini*), in the hope of adding some skeletons and skins to the collections of the British Museum. But he was unsuccessful, and in November he broke up his camp and turned to the south-west, reaching Matabililand in December, where after having a disturbance with Lo Bengula over

[1] TRAVEL AND ADVENTURE IN SOUTH-EAST AFRICA; being the narrative of the last eleven years spent by the author on the Zambesi and its tributaries; with an account of the colonisation of Mashunaland, and the progress of the gold industry in that country. By Frederick Courteney Selous. 8vo. Pp. xviii., 504, map, portrait, 22 plates and 35 illustrations in the text. London, 1893. Price 25s.

a hippopotamus, concerning which he was innocent, and for which he had to pay £60, he set out for the Transvaal.

Setting forth again in March, 1884, in company with W. M. Kerr, Selous decided not to hunt in Lo Bengula's country after the sorry treatment received at the king's hands, but to turn west after reaching Bulawayo, and spend his time in the northern parts of Khama's territory. Towards the end of June Mababi flat, N. of Lake Ngami, was reached, but as there was not much to be had in the way of game, the party returned to Horn's Vley, where elands, giraffes, and gemsbucks were to be found in abundance. Selous briefly describes a storm, which he saw at this place, during which the rain came down so heavily that, although the soil was a deep loose sand, the lightning showed a sheet of water, for the sand could not absorb the rain as fast as it fell. Resuming the eastward journey, the travellers followed the old waggon-spoor to Tati, and thence went back to Bulawayo. Having obtained permission from Lo Bengula, at the price of £60 (a salted-horse), for the right to hunt in Mashunaland, Selous decided to do so, especially as he had several commissions to execute for European museums in skins and skeletons of antelopes of a species more abundant in Mashunaland than elsewhere. He accordingly crossed the river Gwelo and passed through the country to Umfili and the Machabi Hills, where he fell in with a mighty herd of nearly 200 elephants, of which he and his man Laer killed six. Spending the best part of eighteen months in this district, during which time the camp was moved to the Manyami River, and excursions made in all directions, Selous was fortunate enough to secure five fine specimens of Lichtenstein's hartebeest, of which one pair are now preserved in the Natural History Museum, and a second pair are in Cape Town. In December, 1885, a return was made to Matabililand, and from thence a quick run home to England, only to return to Mashunaland early in 1887. It is interesting to read that the track which Mr. Selous' six waggons made in 1887, when passing over the ground on which Fort Salisbury now stands, was still visible in the softer soil near Fort Charter in 1890.

In April, 1888, Selous started from the Transvaal for the Zambesi, having received through Westbeech, the trader, an invitation from a missionary friend, who had once travelled with him, to visit Garanganzi country, near Lake Bangweolo. To a man of Selous' temperament, such an idea was delightful, particularly as it meant a journey through new country, and the probability of abundance of elephants, and hastily fitting out an expedition he set forth for Wankie, as the most favourable place to cross the Zambesi. Journeying north, the little party reached Minenga in the Mashukulumbwi country, and it was here that Selous met with one of his most stirring adventures—a night attack on his camp, which dispersed all his men, and left him alone in Africa with nothing save what he stood up in, and a rifle with four cartridges. Taking the Southern Cross for his guide, Selous began his lonely journey southward, having his rifle stolen by the way, and narrowly escaping being shot. He fell in with the remains of his party near Wankie, and then found that on the fatal night twelve men had been killed and six wounded, the survivors bolting into the bush, and making south during the night, across the country. From Wankie, Selous, undeterred by the perils of the last journey, went up the Barotsi Valley as far as Lialui, and returned by canoe along the Zambesi River to Kazungula, where he arrived in October, 1888. In December, although no rain had fallen, he pushed rapidly across the desert country, and reached Bamangwato



Head of Lichtenstein's Hartebeest.

in January, 1889. After paying a flying visit to England, Selous reached Quillimani, on the Coast, in July, and conducted a prospecting party to the Upper Mazoe, travelling by canoe up the Zambesi from Mazaro to Tete. Leaving the river, the travellers struck S.W., reaching Mount Hampden (close to where Salisbury now stands) about September, returning to Tete in October. Thence journeying down the Zambesi to Vicenti, he was back in Cape Town in December.

About this time Selous threw in his fortunes with the British South African Company, and in March, 1890, we find him at Palapye and Bulawayo interviewing Lo Bengula, whose ideas on the subject of roads through Mashunaland were being discussed with Mr. Cecil Rhodes at Kimberley, in eleven days after the interview. Returning to Palapye he formed a camp on the Macloutsie River, and examined the whole district for a strategic position, which resulted in the building of Fort Tuli in the middle of the year 1890. Towards the end of June the pioneer expedition to Mashunaland set out for Tuli, and a road was opened up through Victoria and Charter to Mount Hampden, and on September 11 the expedition, despite the hostility of Lo Bengula, was brought to an end by hoisting the British flag at Fort Salisbury. Concluding treaties with the chiefs of the southern and eastern districts occupied Selous three months, and on his return to Salisbury he plotted out a five-inch to the mile map of Mashunaland, which, completed in 1892, is now in the hands of the Royal Geographical Society. In January, 1891, we find the traveller signing a treaty of alliance and concession between Motoko, a chief of north-east Mashunaland, and the Company, and afterwards passing south to the Umtali camp, in order to cut roads from thence to the Lower Revui, and from the Odzi River to Salisbury. By the beginning of July, Selous had the entire road from Umtali to Salisbury "in good order for heavy waggons, all the bogs being corduroyed and the streams bridged." Journeying alone to Tuli to see if the waggons coming up country were in a position to bring sufficient stores for Fort Salisbury, Selous returned to Mashunaland, and was employed by the Company in laying out and making roads until May, 1892, when, there being no more work for him to do, he terminated his engagement, spent two or three months shooting and collecting specimens, and finally made his way to Beira, on the coast, and returned to England on 17 December, 1892. After a brief rest in this country, Mr. Selous returned to Mashunaland, on the news arriving of the trouble with Lo Bengula, and it is a matter of supreme satisfaction to know that so experienced and courageous a man is guarding our interests and native subjects in the wilds of Africa.

From the above somewhat lengthy account of the traveller's wanderings, the reader will have gathered that this book is one of peculiar interest, quite apart from the remarkable adventures with which it is crowded. Page after page teems with stirring exploits with lions, elephants, hyænas, and other animals, besides the risks undergone in dealing with the treacherous Matabili and other tribes. Mashunaland and Matabililand no longer convey a mere geographical expression, but are living countries, and the descriptions of their inhabitants, whether man or beast, related as they are by one who has crossed and re-crossed the country in every direction, are of surpassing interest.

The book is well got up, the illustrations are interesting, and the map of the country is of considerable value, though it suffers from the fact that the spelling of the names does not agree in all cases with those in the narrative. It is distinctly a book to buy and put on one's bookshelves, and not one to borrow from a circulating library.

By the kindness of the publisher we are enabled to reproduce an illustration of Lichtenstein's hartebeest; and one of special interest in the book is that of a Mashukulumbwi warrior, whose singular head-dress conveys the fact that this people must necessarily live in an open country.

SOME NEW BOOKS.

An Examination of Weismannism. By George John Romanes, M.A., LL.D., F.R.S. Pp. 221. London: Longmans, Green & Co., 1893. Price 6s.

This book presents many of the characters of book-making. It has been written piece by piece—one section of it being intended for the second part of "Darwin and after Darwin," unfortunately delayed by Dr. Romanes' illness; another part consists of additions and modifications suggested by recent publications of Dr. Weismann; one appendix is a reprint of some controversy in the *Contemporary Review*, and an excellent portrait of Dr. Weismann has been put at the beginning that readers may know the features of the gentleman who is being criticised. There is in the book, therefore, the precise difficulty which Dr. Romanes complains of with regard to the writings of Professor Weismann. Inconsistencies, additions, and emendations make it very difficult to understand the exact views of Dr. Romanes. Thus in the preface the non-inheritance of acquired characters (the discussion of which is postponed to another volume) is said to be the "fundamental postulate upon which Weismann has reared his elaborate system of theories": on page 49 the fundamental postulate is stated to be the continuity and stability of germ-plasm. On page 77, while admitting that influence of a first sire upon children to a second might be reconcilable with absolute continuity of the germ-plasm by supposing the germ-plasm in the spermatozoa to penetrate an unripe ovum, he holds that an analogous phenomenon in plants goes to exclude that hypothesis and most definitely to substantiate the hypothesis that the spermatic element must exercise some influence on the somatic-tissues of the female, which in turn act upon the ovum. In the second appendix he attacks Herbert Spencer for precisely this second view, and strongly supports the first.

These, however, are mere accidental blemishes, and Dr. Romanes' general attitude can be observed. This general attitude apparently was determined by the question of acquired characters. As is well-known, when observations on the Hydrozoa led Weismann to the conception of a continuity of germ-substance from generation to generation, he raised the question as to how acquired characters could affect the germ-plasm at all. The controversy that arose turned upon the interpretation of facts, and most will agree that the controversy has contracted since it began. At first, the opponents of Weismann said that there was an overwhelming amount of evidence in favour of the general inheritance of acquired characters. Against that view Weismann and others urged that the germ-plasm was stable: unaffected by somatic changes. Now, at least, Dr. Romanes and others freely write that acquired characters are much less veritable than are congenital characters, and a large number of cases, formerly insisted upon, have been abandoned. On the other hand, Professor

Weismann has expanded those hints in previous writings, that Dr. Romanes called illogical admissions, and in his most recent book, by allowing that unequal nutrition affecting the germ-plasm may cause variation of the germ-plasm, he "admits" the action of the soma upon the germ-plasm to an extent which will probably be found to cover the residuum of facts now that they have been sifted by controversy. For this source of variation, in addition to amphimixis, and to the inherited results of the original action of the environment upon primitive organisms, brings Weismann's theory of evolution so nearly into line with what Dr. Romanes agrees to, that at first he proposed to cancel a large part of this book—but he has left it, showing how very prettily he would have fought had there been anything to fight against! But he still insists, against Weismann, that Weismann's theory requires the absolute stability of the germ-plasm. However, his examination of this question cannot be complete without the promised sections upon acquired characters, and everyone will hope that Dr. Romanes' health will allow him to publish the new volume soon.

In other matters, however, there is quite enough left for controversy.

Dr. Romanes very acutely divides Weismannism into the theory of heredity which deals chiefly with the germ-plasm, and the theory of evolution which deals chiefly with variation.

He objects to the vast elaboration of the germ-plasm with its molecules, biophors, determinants, ids, idants, and so forth, chiefly because he regards it as a work of "artistic imagination" rather than of "scientific generalisation." So far as concerns "determinants"—the particles of germ-plasm which correspond to groups of cells that vary similarly and simultaneously—he admits the great value of the conception, and he accepts the "ids" as a logical consequence of "determinants"; but he sees no reason whatever for identifying idants with chromosomes. In this, Dr. Romanes makes the same error as he did in the matter of the continuity of the germ-plasm. In both cases, Weismann started with the observed fact; the visible changes in the Hydrozoa led him to the continuity, and the actual divisions and marshallings of idants in sexual cells led him to the particulate theory of germ-plasm. And it is because his theories start from observed facts of this kind that they appeal so strongly to "laboratory naturalists."

He gives a very interesting comparison between Galton's "Stirp," and Weismann's germ-plasm, pointing out how the recent extensions (Romanes would call them "revolutions") in the latter theory bring it more into harmony with the former. Both allow that the soma arises from the germinal cells, and that in each ontogeny only a part of the germinal material is used up—the greater part being handed on. According to Galton, occasionally, but rarely, there are contributions from the soma to the stirp, and so acquired characters may be inherited occasionally: in Weismann there are never such centripetal contributions, and so acquired characters, as such, are never inherited. In the mechanism of ontogeny, Weismann insists on a peaceful disintegration of the architecture of the germ-plasm; Galton on a constant struggle between competing carriers of heredity. Weismann, in his dealing with "xenia"—the influence in plants of fertilisation, not on the fertilised cell, but on the tissues of the female, allows the possibility of the direct action of germ-plasm upon the somatic tissues, even though those tissues may belong to another individual. Dr. Romanes calls this a surrender of the perpetual isolation of the germinal substance to a sphere of its own, and evidently holds that it goes towards establishing a reciprocal action between soma and germ-plasma. It is difficult to follow this. Weismann has of course always held that germ-plasm is isolated only so far as it does not receive impressions from the body, to be reflected upon the body of the next or after generations. The whole of his theory of the structure of germ-plasm implies that its particles have a determining influence upon the soma, and there is no difference in kind between supposing that determinants can enter and determine embryonic cells, and supposing that they may enter and determine adult cells. It is not always given to a man to know his own father, and there is no reason to suppose that a determinant can nicely distinguish between protoplasm of a developing cell and protoplasm of another cell, even although that other cell belongs to another plant.

In the theory of evolution Dr. Romanes apparently resents that the recent elaboration of Weismann's views has met many objections to them. This is chiefly in the matter of variation. Dr. Romanes writes :—" Weismann has now expressly surrendered his postulate of the absolute stability of germ-plasm! We have already seen that, even in the first volume of his *Essays*, there were some passages which gave an uncertain sound with regard to this matter. But as they seemed attributable to mere carelessness on the part of their author, after quoting a sample of them, I showed it was necessary to ignore such inconsistent utterances—necessary, that is, for the purpose of examining the theory of germ-plasm as even so much as a logically coherent system of ideas."

How charming! but with all deference to Dr. Romanes, it is, at least, as fair to suggest that Weismann, by the inclusion of " inconsistent utterances," was, in the true spirit of science, guarding himself against " logical coherence " in a shaping theory. It may be given to schoolmen, writing of such matters as the dancing of angels upon a pin-point, to be logically complete—but until, root and branch, we have eaten up the tree of knowledge, any theory of evolution or heredity that is logically coherent and complete will be scientifically incoherent and incomplete. The particular extension in question is the idea that local variations of nutrition, of which all along Weismann, as everyone else, has recognised the capacity to produce variations in *the individual*, may also produce variations in the germ-plasma. As Dr. Romanes rightly points out, this extension removes the great stumbling block of seeing as the cause of variation only the combination produced by amphimixis of original Lamarckian modifications of the Protozoa.

A Dictionary of Birds. By Alfred Newton, assisted by Hans Gadow, with Contributions from R. Lydekker, C. S. Roy, and R. W. Shufeldt. Part ii. 8vo. London: A. & C. Black, 1893. Price 7s. 6d. nett.

Amongst a certain section of that vast brotherhood ycelpt " Ornithologists," a work like the present has been a long-felt want; and, when it was announced that Professor Newton had in hand a " Dictionary of Birds," we are afraid that we allowed our imagination a rather lofty flight. As might be expected in such a case, when the first part actually appeared, we were bound to confess to a certain feeling of disappointment, inasmuch as we had expected a work on a rather more pretentious scale: we felt that neither the Professor nor his colleagues were doing themselves justice. After having made ourselves tolerably familiar with the first part, however—the general scope and purpose of which, it will be remembered, was set forth in these pages a few months since—this feeling has, in many respects, given place to a sense of gratitude for small mercies, for undoubtedly it is the best book of its kind which has yet appeared, and we imagine that, not until the second edition appears will there be a better.

To attempt anything more than a very rapid survey of the part before us would be to far exceed the space at our disposal, but it shall be our endeavour to crowd as much information into that space as possible.

The first article of any importance is that on Geographical Distribution, and when we say that it covers some 52 pages, it will be seen that it is fairly exhaustive. We can bestow no further praise upon it than to say that it is in every way worthy of comparison with the excellent article on Extermination which has preceded it. Conning its pages, we feel that the whole of this most difficult subject has been so lucidly explained, that even those who take up this book for the first time will have no difficulty in mastering it.

The account of the Grebes interested us much. The figure of the Great Crested Grebe conjured up a host of old and delightful recollections of days spent, so to speak, in their company, in one of their few remaining strongholds, the Norfolk Broads. We believe that we are correct in stating that its feathers are no longer "much in request for muffs and the trimmings of ladies' dresses," inasmuch as the tide of fashion has turned in another direction.

That the gizzard of this species, and Grebes in general, is invariably filled with feathers from its breast, mixed with fish-bones, is a fact that has long been known, yet, curiously enough, it has escaped notice here. The serration along the back of the tarso-metatarsus has also passed unnoticed, though this, it is true, is a point of small importance.

The articles on Humming-Bird, Hornbill, and Kiwi are brimful of interest—especially the first-mentioned. The marvellous brilliancy of the plumage, the extraordinary variety in size and ornamentation, coupled with the Edenic life these birds are popularly supposed to lead, have tended to obscure them in a haze of poetic glamour shared, probably, by no other animal on earth. We imagine, therefore, that not a few will find it difficult to picture humming-birds "dwelling in a world of almost constant hail, sleet, and rain," or "flitting about the Fuchsias of Tierra del Fuego in a snow-storm." There is surely good reason to fear that the recording angel will soon have to inscribe the name of "*Chrysolampis mosquitus*" upon the calendar of departed species, or, in other words, it will go to swell the ranks of Birds *recently exterminated*, since "thousands of skins are annually sent to Europe to be used in the manufacture of ornaments (!), its rich ruby-and-topaz glow rendering it one of the most beautiful objects imaginable."

In speaking of the Manucode, Professor Newton says: "As with members of the Paradiseidæ generally, the nidification of the Manucodes had been shrouded in mystery, until . . . the nest and eggs of *M. comrii* were found in July, 1891 . . . ." This is correct as far as this species is concerned, but the egg of *Paradisea raggiana* was first described so long ago as 1883, by Mr. E. P. Ramsay, in the

Proc. Linn. Soc. N.S.W., vol. viii., and the egg of *P. apoda* was described from Aru in the *Zeitsch. f. ges. Ornith.* for the same year.

In the brief description of the Guinea Fowls, *Numida vulturina* has been included with those species which have "the crown bare of feathers and elevated into a bony 'helmet.'" This is, of course, an oversight, this particular species having no "helmet."

This part concludes with two extremely interesting articles on Migration and Mimicry. The former is especially fascinating, and brings together a mass of information, for which the general reader should be grateful.

Dr. Gadow's contributions can in no way be said to play a secondary part in this book. His task is by no means an enviable one, for to render concise and readable articles out of such uncompromising material as has fallen to his share to mould, is, to say the least, a difficult one; but, nevertheless, he has succeeded in presenting us with descriptions that are at once singularly clear and undoubtedly correct. We would, however, be allowed to point to the description of the humerus and the four accompanying figures as a slight exception. In the figures referred to—the left humerus of a raven—all the more important points of muscular attachment have been distinctly named, but, in the lowest figure, the crista superior is called crista "lateralis," while in the text "tuberculum superius" should read "tuberculum externum" so as to agree with the figures. So again, "tuberculum inferius" should read "tuberculum medium," for the same reason.

Now a word as to the illustrations. On every hand, the figures of Swainson have been most eulogistically praised. *Fas est dictu*—we have seen better. For instance, who would recognise the caricature on p. 401, which is supposed to represent the head of the Guinea Fowl? or the ? model for an umbrella handle on p. 406? which we are asked to believe is the head of *Scopus*. To our mind, the figures on pp. 388, and 508–10 are far-and-away more life-like and beautiful. Some of Swainson's figures undoubtedly are good, such as that of the head of the Hawfinch, but these are the exception, and not the rule. The elegant figures of the Heron, Jabiru, and Merlin are almost beyond praise, but these are not Swainson's.

Figures of bills and feet come prominently to the fore in this book; doubtless they have their value, but they are by no means so important as the prominence given them would lead the lay mind to believe. In the case of unfamiliar forms, such as the Guan, we venture to think they are of no use whatever; here, not even the entire head is given, but only the beak. The amateur must indeed have a vivid imagination if he would conjure up the remainder of the bird!

We wish it to be distinctly understood that these remarks are offered, not in any spirit of captious criticism, but, as suggestions which might be acted upon in a second edition, which, we feel sure, will be called for.

We will conclude as we began, by reiterating that this book undoubtedly fills a gap, and it is hardly likely to be supplanted by any similar work for a long time to come. W. P. P.

The Zoological Record for 1892. Edited by D. Sharp, F.R.S. 8vo. Pp. 926. London: Zoological Society, 1893.

It is no exaggeration to say that the *Zoological Record* is the most important zoological publication in existence. The absence of this yearly volume would bring about a state of affairs too terrible to contemplate even in imagination ; it is our business, therefore, to be exceedingly thankful to the editor and his staff of contributors for the great pains which they have taken in registering the immense amount of zoological work of the past year, and to the Zoological Society for bearing the expense of this necessarily costly publication. These grateful feelings on our part leave scarcely any wish to criticise ; but as a matter of precedent it is a very easy task to criticise the *Zoological Record* ; it has been laid down in all criticisms of past volumes which we have read, that the way to deal with this work is to search diligently for misprints; to ignore the vast amount of labour expended upon its production, and to dwell particularly upon the defects of newly-enrolled members of the staff of recorders. We, personally, happen to be of an extremely conservative disposition, and are, therefore, intense respecters of precedent ; in spite, however, of natural inclination, stimulated by such misprints as "*Aelosoma*" for "*Aeolosoma*," *fluviventris* for *flaviventris*, *Naidonina* for *Naidium*, etc., we feel very strongly that the only proper way to criticise the *Zoological Record* is to point out, if it can be done, that the record is incomplete. So far as we have tested the present volume this is not the case ; it appears, however, that the last volume was so to some extent, for there are a few references to papers published in 1891, which should, of course, have been included in that volume. But—particularly in this kind of work—better late than never ! One of the more useful features of the *Zoological Record* is the general section with which it commences ; this improvement was introduced by the late editor, and we are glad to notice that the present editor has not dispensed with a very important part of the volume.

F. E

OUR HOUSEHOLD INSECTS. An Account of the Insect-Pests found in Dwelling-houses. By Edward A. Butler, B.A., B.Sc. 8vo. Pp. 344, 7 plates and 113 figures. London : Longmans, Green & Co., 1893. Price 6s.

WE can strongly recommend this book, whose chapters originally appeared as articles in the pages of *Knowledge*. The various species of insects which have, more or less, established themselves as companions of man are described in systematic order. The structure, habits, and life-history of each insect are set forth in a manner at once accurate and popular ; so that the reader who is attracted to the book will learn what material for interesting study may be found in the bodies of the humble lodgers in his dwelling. The affinities of the household insects with their out-of-door relations are indicated, and a very fair notion of the comparative morphology of the leading insect-types may be obtained by the careful student. The insects described include the wood-boring, skin-eating, and meal-worm beetles, ants, wasps, clothes-moths, cockroach, cricket, earwig, flies, gnats, flea, bug, book-lice, silver-fish, and skin-lice. The figures of the insects and their anatomy are good, and the plates are reproductions of micro-photographs prepared for lantern slides.

While the book is primarily intended as a popular work, we do not think the author will be disappointed of the hope expressed in the preface that the serious student of entomology will find it useful. The only objection is likely to be raised by the householder, who will perhaps complain that while the author describes with enthusiasm the jaws of the creatures and the depredations they commit on furniture, food, and clothing, he has but little advice to give upon methods for their wholesale destruction.

ON HAIL. By the Hon. Rollo Russell. 8vo. Pp. xvi., 224, with 2 photographs of Hailstones. London: Edward Stanford, 1893. Price 6s. nett.

THIS book deals with "Descriptions of Hailstorms and Hailstones," "Observations of Temperature, Clouds, and Winds at great Altitudes," and "Electricity and Hail." The author then gives a digest of the various "Theories of Hail," and of "certain properties of Vapour, Water, and Ice, and conditions of the Air which may be connected with the formation of Hail," with a "Summary of characteristics of Hailstorms and Hailstones," and notes on "the Development of a Hailstorm." Appendixes on "General Weather Conditions," "Cold produced by Radiation," "Dust Particles and the form of Ice-crystals," "Types of Hailstones," etc., are also given, and the author states the conclusions arrived at from his researches.

The work can be recommended to the student, as presenting all that is known on the subject up to the present, and especially for the convenient digest of the theories of previous writers. It should also form a useful addition to the library of the general reader. It is well got up, printed in large type, and the two photographs of hailstones (actual size, one of which is 2 in. in diameter) which fell at Richmond, Yorkshire, in the storm of 8th July this year, are excellent, and show both internal and external structure.

MESSRS. CASSELL & CO. have issued part i. of a new Gazetteer of Great Britain and Ireland; it is clearly printed, it is accompanied by a neat map of the British Isles coloured to show the counties distinctly, and there is also the first section of a map of England on a scale of an inch to rather more than ten miles. The work is intended to be "A Complete Topographical Dictionary of the United Kingdom," and it professes to meet "a want hitherto unsupplied." Among the many matters on which information is promised in the preface, are notes on the Prehistoric Remains, Earthworks, &c., on the Physical Features, and (in the cases of parishes) some account is to be given of the nature of the soil.

Judging from the part now before us, we find that the articles, as a rule, are much shorter than those of Fullarton's Gazetteer of England and Wales, of which the first edition was published in 1843. It was no doubt difficult to get good accounts of the soils in each parish, consequently we find the subject treated very unequally, and not always clearly, nor accurately. The following are instances:—At Abbeystrowry, "Soil clay and artificial loam, overlying slate"; Abbotsbury, "Soil chiefly red clay" (no mention of iron ore and limestone); Aberavon, "Soil sand and gravel, overlying coal and minerals"; Abernethy, "Soil chiefly granite rock"; Aldborough, "Soil loamy, overlying clay" (no mention of shelly "crag"); Aldington, "Soil various, overlying rock"; Aldsworth, "Soil stonebrash, overlying lime and freestone." A concise and reliable account of the principal soils and economic products of the rocks in each parish would have been useful.

THE Cambridge University Press are about to publish a series of Natural Science Manuals, which will cover a wide field, some of the books being adapted for beginners, while others will deal with special topics and will be useful only to more advanced students. The series will be divided into two sections, a Biological and a Physical. The former will be published under the general editorship of Mr. Arthur E. Shipley, M.A., Fellow and Tutor of Christ's College, Cambridge, it will include a manual of Invertebrate Palæontology by Mr. H. Woods, Demonstrator of Palæobotany at Cambridge, which is now ready; a text-book on the Practical Physiology of Plants, by Mr. Francis Darwin of Christ's College, and Mr. E. Hamilton Acton of St. John's College, which is in the press; works on Physical Anthropology, by Professor Alexander Macalister; on the Vertebrate Skeleton, by Mr. S. H. Reynolds of Trinity College; on Fossil Plants, by Mr. A. C. Seward, Lecturer in Botany in the University, and an Introduction to the study of Botany by Mr. Francis Darwin, which are in preparation. Other volumes will shortly be announced.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Mr. Charles F. W. McClure, M.A., has succeeded Dr. Osborn as Professor of Biology at Princeton College, New Jersey.

The Rev. A. Irving, D.Sc., F.G.S., has resigned his appointment at Wellington College, Berks, and removed to Hockerill, Bishop's Stortford, Herts.

The late Professor Newberry's name is to live in a fund which the scientific societies of New York have resolved to raise. It will be called the John Strong Newberry fund, be not less than 25,000 dollars, and the income derived from it will be devoted to the encouragement of scientific work in geology, palæontology, botany, and zoology. Professor N. L. Britton is secretary to the subscription committee.

The Jersey Biological Station promises a quarterly publication together with an issue of microscopical preparations of the rare and less known marine organisms. The text will be descriptive of the slides issued, and will contain hints and notes on microscopical manipulation, together with original observations upon Marine Zoology. The subscription for the year, including fourteen slides, is 21s., and this issue is limited to seventy-five copies.

We regret to learn that, owing to the transfer of political power in the Illinois legislature, Dr. Josua Lindahl has been dismissed from the post of State Geologist. Dr. Lindahl's scientific attainments are well known; he served as zoologist on the "Porcupine" expedition, and the concluding volume of the Report of the Geological Survey of Illinois showed that in him that State had secured an energetic, industrious, and capable servant.

We much regret to learn that the post honoured by the names of Amos H. Worthen and Josua Lindahl, has been handed over to a Mr. William F. E. Gurley. This gentleman is, we understand, a collector of fossils, and his name has once appeared in so-called scientific literature as co-author of a paper with Mr. S. A. Miller, of Cincinnati, O. One more step remains for the State of Illinois—to make Mr. Miller State Palæontologist; and this step will doubtless be taken.

The fine collection of fossils made by the late Professor A. H. Worthen, which contains very many of the specimens figured and described in the Report of the Geological Survey of Illinois, has at last been bought by that State. The specimens are at present exhibited in the Illinois building at the World's Fair. Each has a register number, and, as a very complete sale-catalogue was published,[1] all future visitors to the State Museum will easily be able to identify them.

[1] Warsaw, Ill., 1889; sold in London by Wm. Wesley & Son, price 6d.

It seems odd to us in England that a well-paid State Geologist should be able to make a private collection of 2,871 fine specimens, to treble its value by selecting 752 of these as types for the descriptions by other State officers in the State publications, and as a consequence to finally force on the State the purchase of this collection for a very large sum of money. But in America such a proceeding is not thought in the least odd ; and that is the oddest thing of all.

The Indiana Academy of Science has, according to *Erythea*, determined on a biological survey of the State. The promoters intend to publish a complete bibliography of the botany, zoology, and palæontology of Indiana, to ascertain what has already been done, while the main purpose of the survey will be to make known the extent, distribution, biological relations and economic importance of the entire fauna and flora. L. M. Underwood is at the head of the botanical division, and the special work of this season in botany will be a study of distribution, particularly of the lower cryptogams and of the rarer forms of flowering plants and ferns. We trust the enthusiasm of the Indiana naturalists will endure throughout and carry to a successful issue this worthy project.

The *Transactions of the Norfolk and Norwich Naturalists' Society* for 1892–93, which has just appeared, form an excellent model for local societies. Of the 170 pages contained in this part, all except four or five relate to the natural history of Norfolk. The address of the President (Mr. H. B. Woodward) deals with the geology of the county. Then follow papers by Mr. Southwell on the Siberian Pectoral Sandpiper, Sowerby's Whale Shooting at Holkham, The Herring Fishery of 1892, and some Additions to the Norwich Museum. Mr. J. H. Gurney writes on the Immigration of the Lapland Bunting; Mr. H. B. Woodward on Caleb B. Rose, the Norfolk Geologist; Mr. Mayfield on Norfolk Slugs; Mr. A. W. Preston on Meteorology; Mr. Clement Reid on *Paradoxocarpus carinatus* from the Cromer Forest-bed ; and last, Mr. James Edwards contributes an account of the Coleoptera, forming Part XII. of the Fauna and Flora of Norfolk. Sir P. Eade's note upon Tortoises is the only contribution not of local interest; and even it relates to tortoises that have lived ten years in the county, and may therefore be considered almost to belong to it.

The first number of the *Proceedings of the Malacological Society of London* has just made its appearance, edited, under the direction of a publication committee, by Mr. B. B. Woodward. In addition to the proceedings of the inaugural and subsequent meetings of the first session, it contains eight of the papers read by members. One-half of these are systematic, while of the remainder two are anatomical, so that the ground covered is fairly representative of the aims of the Society, as set forth on the second page of the wrapper. The number, which runs to 30 pp., is illustrated by two full page plates and several illustrations in the text. A footnote to page 11 makes us aware that conchologists cannot, alas! more than any other section of humanity, implicitly trust their fellow man, some smart American seemingly having robbed Mr. E. A. Smith of one of his new species, a proceeding which the publication committee "greatly regret," and reflect upon.

The fifteenth annual meeting of the Greenock Natural History Society was held on September 28, and the secretary, Mr. G. W. Niven, presented a satisfactory report. Only one paper embodying local research seems to have been read during the last session, that by Mr. M. F. Dunlop, on some rare Rotifers. The meetings are chiefly occupied with papers of a general character, and sometimes relate to physical science.

The fifty-fifth annual meeting of the Manchester Geological Society was held on October 10. Notwithstanding unusual losses by death and other causes, the membership is still well maintained, and many important communications, especially of economic interest, have been discussed during the past session. In the early part of this year a circular was addressed to the members and the coal-owners and lessees of Lancashire and Cheshire, asking for their assistance and co-operation in the work which the Society is hoping to accomplish, of giving a fairly accurate enumeration and description of the fossils of the coal-bearing rocks of this district. Instructions were given to carefully note the exact horizon and locality from which each fossil was derived, so that an attempt might be possible to correlate the various coal seams in different parts of the county, thereby rendering valuable aid to future explorations for coal. The result of this appeal has not, as yet, been such as the Council had reason to expect, but it is hoped that the object has not been lost sight of, and that considerable additions to our knowledge of the fossil fauna and flora of the Coal-measures will yet be forthcoming from the hands of the members and others interested in the work. Several members have already responded by sending specimens, whose assistance will be acknowledged and their contributions incorporated in the next list of Fossil Plants, etc., which is being prepared by Mr. Robert Kidston, F.G.S., for publication in the Society's Transactions.

OBITUARY.

H. W. CROSSKEY, LL.D., F.G.S.

Born 1826. Died October 1, 1893.

BY the death of Dr. Crosskey, Geology loses one of the most pains-taking students of Glacial and post-Glacial deposits; while to the general public he was well-known as a leading Unitarian minister, and as one devoted to the educational advancement of the poorer classes. He was born at Lewes, and undertook his first pastorate at Derby. Thence he removed in 1852 to Glasgow, and in 1869 to Birmingham. While in Scotland he paid much attention to the post-Tertiary deposits of the Clyde Valley, and about the year 1855 he became associated with David Robertson, "The Naturalist of Cumbrae," and their joint labours on those fossiliferous deposits were published by the Geological Society of Glasgow. Later on they were joined by Dr. G. S. Brady in a special study of the post-Tertiary Entomostraca (or Ostracoda), and this resulted in a Monograph on the subject, which was published by the Palæontographical Society in 1874. Dr. Crosskey was the author of a series of Reports on the Erratic Blocks of this country, which were communicated during the past twenty years to the British Association.

THE death is also announced of Mr. Thomas Bain, the South African Geologist, who for a long period continued the work of his father, the late Mr. Andrew Geddes Bain, in discovering so many of the fossil reptiles of the Triassic Karoo Formation now in the British Museum.

CORRESPONDENCE.

The pre-Glacial British Fauna.

The solution of the problem which forms the subject of Mr. Bulman's article is a very important one, both from a biological and from a geological point of view, and he has very ably argued the points in favour of a survival of a pre-Glacial Fauna and Flora in the British Islands. Since the appearance of Professor Forbes's classic essay referred to by Mr. Bulman, many additional facts regarding the existence of a Lusitanian Flora in the South of Ireland have been brought to light, and although the possibility of their post-Glacial transmission by birds is not to be excluded, it is not likely that such was their origin, since they are now known to be associated with Lusitanian animals, of which one at least (*Geomalacus maculosus*) must have reached these shores by a land passage, as both it and its eggs are almost immediately killed by immersion in sea-water. That this slug should have been accidentally carried by birds from Portugal to the South of Ireland is surely not worthy of argument. The geographical distribution of slugs, indeed, and of non-operculate molluscs, especially the subterranean species, may be most profitably utilised in the task of solving the problem of the origin of the British Fauna.

Equally important, and in many respects even more so, are the mammals, and an enquiry like that initiated by Mr. Bulman might with advantage be started, tracing the origin of the Irish species first. Familiar English species, such as the Weasel, Mole, Voles, Common Shrew, and Hare, and many others, are absent from Ireland. It has been suggested that Ireland was separated from England before these mammals had time to cross over, but among them we have some of the most quickly-spreading animals, and it is more probable that they arrived in England only long after the species at present inhabiting Ireland, which therefore formed part of another and much earlier immigration from the continent. This immigration was probably pre-Glacial, but that the second was not entirely post-Glacial seems proved by the fact that remains of some of the mammals mentioned as being absent from Ireland occur in the Forest Bed.

R. F. Scharff.

22 Leeson Park, Dublin.

Archæopteryx.

In reference to Dr. Hurst's interesting, but somewhat intemperately-worded article on *Archæopteryx* last month, I should like to remark that although Professor von Zittel's copy of Owen's restored figure of the hand of this bird wants one finger in the original German edition of the "Handbuch der Palæontologie," the mistake is rectified in the subsequent French edition. It is thus obvious that the author's omission was an accident, and not intentional; possibly the block was "battered" at the edge of the page.

A. S. W.

TO CORRESPONDENTS.

All communications for the Editor *to be addressed to the* Editorial Offices, *now removed to* 5 John Street, Bedford Row, London, W.C.

All communications for the Publishers *to be addressed to* Macmillan & Co., 29 *Bedford Street, Strand, London, W.C.*

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NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

No. 22. Vol. III. DECEMBER, 1893.

NOTES AND COMMENTS.

A National Trust for Places of Beauty and Interest.

WE learn from the *Daily News* that a movement which may turn out to be of some importance was started at the rooms of the Commons Preservation Society on November 16. It has long been felt that the nation is in the way of losing some of its most valuable possessions through the want of some custodian to whom they may be readily transferred, and by whom they will be jealously guarded. Districts celebrated for their natural beauty are by degrees marred and disfigured; houses and ruins of unique interest are destroyed, because, passing from hand to hand, they some day come into the ownership of persons unable to appreciate them, or forced to realise any money value they may have. Within the last two years, such noteworthy spots as the top of Snowdon, the island in the middle of Grasmere Lake, and the Lodore Falls have come into the market, and might not improbably have been permanently secured for the public enjoyment had some body capable of acquiring and protecting them in the public interest been in existence. We have also noticed with grief the quarrying operations that are carried on along the Cheddar Cliffs —the grandest inland cliffs in this country—as if there were not abundance of the Mountain Limestone to be obtained elsewhere in the neighbourhood. Local authorities can hardly at present be expected to help the public to preserve the beauty of its great pleasure grounds; their area of action and the sources from which they draw their funds are, as a rule, too contracted, and they have many claims upon their not too ample resources. But apart from local authority there is absolutely no body which can hold and manage places of beauty and interest on behalf of the public; for the existing open space societies are not corporate bodies and exist rather to influence public opinion than to hold property. This want it is now proposed to meet. Canon Rawnsley, the Vicar of Keswick, well known as an energetic champion of the Lake District, has for some weeks been enlisting support for a body which it is proposed to style the "National Trust for Places of Historic Interest and Natural Beauty." This body is to be incorporated under the Joint Stock Companies Acts, with the licence of the Board of Trade, as a non-profit-earning society. Its primary function will be to accept from landowners gifts of places which they desire to place beyond risk of injury from their successors, and to keep such places intact and at the service of the nation. As funds increase it is thought that purchases of important places may be made, either at the expense of the general agents of the society, or by means of special contributions for the purpose. The new society is enabled to make a beginning by the generosity of a Welsh landowner, who is desirous of transferring to its care a beautiful sea-cliff on the West Coast. It numbers among its adherents such well-known names as the Duke of Westminster, Lord Dufferin, Lord Rosebery, Sir Frederick Leighton, Professor Huxley, the Master of Trinity, Cambridge, Mr. Shaw Lefevre, and Miss Octavia Hill, and will lose no time in "acquiring legal form, and entering upon its duties." In so doing it will certainly carry with it the good wishes of all who have a feeling for nature and historic association.

Winchester College.

All the world knows that Winchester College has recently been celebrating its Quingentenary. To commemorate this event by some permanent memorial has long been the wish of Wykehamists; but the multiplicity of proposals proved the truth of the saying, "quot homines, tot sententiæ." It has at last been decided to raise a fund for two purposes. First, the restoration of William of Wykeham's Chantry in Winchester Cathedral; secondly, the establishment of "a group of Memorial Buildings for the preservation of Wykehamical antiquities, and the encouragement of art, archæology, natural history, and other sciences."

The former of these objects lies outside our province. With regard to the latter, we extract from the circular of the Executive Committee the following excellent remarks:—

"The aims of the collection of archæology and art would be (i.) to illustrate and encourage the regular course of school study; (ii.) to furnish boys with interests outside that regular course. The first division would naturally consist mainly of collections illustrating classical art, or otherwise bearing on the study of the classics or the Bible. The second division would consist mainly of mediæval or modern specimens of art. Such collections would consist of representative series of reproductions of objects of art selected for their beauty or educational value, and of any good originals that could be obtained. The latter are needed to give reality to the collection, and to let boys acquire a touch-and-handle familiarity with specimens; the former to show the real beauty of antiquities, and to stimulate a desire to visit the famous collections."

"The science collections would probably be a development in more adequate quarters of the present collection of the Natural History Society, which is good though not large. Special stress would doubtless be laid on the collection of local minerals [fossils are doubtless meant], fauna, and flora. An attempt might also be made (as has been done at Harrow) to imitate the admirably instructive series of type forms exhibited in the Museum of Natural History at South Kensington; and it would be highly desirable to connect some moderate provision for elementary biological and botanical work with the natural history museum."

"It would be premature at present to venture on more than the most general sketch, but many other developments might be suggested which should be the work of subsequent years, for it must always be borne in mind that for the purposes of a school museum the process of growth is more valuable than any completed collection, and that the undertaking will be a failure unless it commands the sustained interest and support of masters and boys alike."

All who sympathise with the extension of scientific education will rejoice that Winchester is no longer to be behind our other great Public Schools in this respect; and if we number any Wykehamists among our readers, we have no doubt that they will hasten to send in their subscriptions to Mr. Percy Toynbee, 109 Gloucester Terrace, Hyde Park. As for the presentation of specimens, we will lead the way with a little bit of advice to the Committee:—Be as ready to refuse as to accept!

The American Association for the Advancement of Science.

We take the following comment from the last number of the *American Naturalist*, and commend it to the notice of those interested in the welfare of the British and kindred Associations:—"The question is often asked, Why do the American zoologists so universally neglect the American Association for the Advancement of Science? For many years scarcely an American publishing zoologist has been present at the meeting, while the few papers on zoological subjects are in striking contrast to the interest shown in the sister science of botany. The reasons for this state of affairs are not readily stated. Possibly most potent of all is the feeling that the Association is far from being a representative of American science, and that it has degenerated into an annual junketing party. It is certain that the interests of science have been often sacrificed to excursions which interrupt the sessions, and which should be postponed to their close. Then, too, criticisms are often heard that it is run as a close corporation, that nominating boards are packed in order that certain persons may be put in office, and that the expenses of the Association are far greater than they should be for the results achieved. There is, too, an inside history which cannot be detailed, which would explain a large portion of the indifference displayed. Before the Association can regain its influence, it must undergo a complete transformation in its management and methods of administration. It must also present features which will attract the better workers of the country."

Fortunately, the harmony of the British Association is not marred by any such "inside history" as that of which our American cousins complain ; but even here, and in other British learned Societies, those who happen to hold professional positions do not always pay that respect to distinguished amateurs which is their due, when chosen to officiate or to inaugurate discussions. The picnic-element can never be entirely eliminated from annual gatherings—it is not desirable that it should be. Science cannot progress satisfactorily without some practical sympathy on the part of the outside public ; and it is the duty of those concerned in purely scientific work at the great festival which nearly every civilised nation now holds, to maintain the correct balance between technical discourses and intellectual recreation.

THE CORRELATION OF GEOLOGICAL FORMATIONS IN EUROPE.

WITH the advance of science, more and more minute methods of investigation are adopted, methods which could not have been adopted before, while sources of error remained so numerous, but which, when once a firm foundation has been laid for them, speedily lead to important results. It is often found, too, that the foundations are laid by branches of a science that appear distinct, and that the eventual results are attained by the combination of differerent methods. Changes of this kind are now taking place in Palæontology and Stratigraphical Geology. It is no longer possible for the palæontologist to study his fossils within the four walls of a museum. He has learnt all that can be learnt there ; but now he sees certain minute differences between forms, which he cannot quite explain. They may be important or they may be only accidental. He can only decide by himself carefully collecting the specimens in the field. Prepared by long study of certain groups of animals, intimately knowing them, almost as a shepherd does his sheep, he now proceeds to examine the rocks with equal minuteness. He is no longer content with the broad generalisations of pioneer geologists, but, just as he has already split up the genera of the old naturalists into numberless more accurately defined new genera and sub-genera, so he must divide the strata into zones and sub-zones and almost infinitesimal horizons. Then at last he will be able to discover the relations that exist between the variations that he has observed in his fossils and their place in geological history, and he will be able to trace out with a certainty that none can gainsay the evolution of species and of genera, and the migrations of fauna.

Work of this kind has been done abroad for some time. The important biological results of Beecher, Clarke, and others, to which we have often alluded (Natural Science, vol. i. pp. 606, 628; vol. iii., pp. 15 and 163), almost entirely depend on very careful collecting, inch by inch, through considerable thicknesses of rock. Munier-Chalmas and Haug have done similar work in Europe of late years; but in England, if we except the admirable investigations of Lapworth on Graptolite zones, which could only have been carried out by a man thoroughly acquainted with the fossils, little of the kind has been accomplished. We therefore welcome heartily the very interesting paper by S. S. Buckman, on the Bajocian of the Sherborne district, just published in the *Quarterly Journal of the Geological Society* (vol. xlix., p. 479). Not only by his exact knowledge of, and previous work on, the Stratigraphy of the Dorset Oolites, but by his prolonged studies of their most important fossils, the Ammonites, he is excellently qualified for work of the kind we have just described. Hence we are not surprised to see that he goes very much further than previous geologists in his subdivision of the strata, while that his subdivisions are not illusory is proved by the possibility of tracing them in adjacent districts and even on the Continent. Thus, he is able to show that there is the same faunal succession in Dorset, Somerset, Gloucestershire, Normandy, Southern France, and Würtemberg. This important point, it must be remembered, could never have been proved under the old system, when three zones were the most that were recognised in the Inferior Oolite. Indeed, as the President of the Geological Society remarked at the reading of the paper, "It was scarcely too much to say that if rocks were to be studied in this minute way, the whole of stratigraphical palæontology would be revolutionised."

The point in Mr. Buckman's paper that will most strike the casual reader is his proposal of a new term, "hemera," as a subdivision of the technical term "age." Practically the word means the time during which a particular species was in existence. Thus "*fuscæ* hemera" means the period of time during which *Oppelia fusca* lived in the district in question. The species chosen to give names to hemeræ are, of course, those that only had a short existence, for the object is "to mark the smallest consecutive divisions which the sequence of different species enables us to separate in the maximum developments of strata." The term, therefore, is a purely chronological one, neither superseding nor a subdivision of "zone," so that the objections to it made by speakers at the meeting fall to the ground. Mr. Buckman, indeed, points out that species actually occurring together in a thin band of rock, may really belong to different hemeræ, a fact which can sometimes be proved by examining the same stratum in another place where its development is greater.

Of the other very interesting results brought out by this detailed method of work, such as the variations in the amount of deposition and the migrations of the mollusca correlated therewith, we can hardly speak here. It is certainly obvious that this kind of investigation is what we want more of; but it is also obvious that it can only be done by those who, like Mr. Buckman, are palæontological specialists, and who are, like him, intimately acquainted with the district that they are examining, and able to take advantage of every temporary exposure. This is where our numerous local geologists and collectors will find their opportunity.

The Correlation of Geological Formations in America.

The principles alluded to above are clearly not neglected by our fellow-workers on the other side of the Atlantic. The Geological Survey of the United States has issued a series of "Bulletins" during the past few years dealing with the Correlation of the Geological Formations of the United States. These bulletins (nos. 80, 1891, Devonian and Carboniferous; 81, 1891, Cambrian; 82, 1891, Cretaceous; 83, 1891, Eocene; 84, 1892, Neocene; 85, 1892, Newark System; 86, 1892, Archæan and Algonkin) are historical studies of the classifications and nomenclatures of the formations, made for the purpose of ascertaining how satisfactory correlations have been made, and upon what principles they have been based. The literature of the century has been reviewed, and various specific problems have arisen for the solution of which it has been necessary to determine the relations between standard formations already named and classified and those newly discovered. The discussion of each problem has been followed out in detail, the various attempts at correlation have been noted, and the methods employed and the final results attained have been traced to the principles involved in their determination.

We reproduce here the headings of researches dealt with in the Devonian and Carboniferous monograph, as giving a good idea of the amount of material rendered easily available to the geological student. They are as follows:—

(1) The general correlation of the Palæozoic formations of eastern North America with the corresponding formations of Europe.

(2) The determination of the parallelism between the upper Palæozoic formations of the Appalachian region and the rocks of the interior of the continent as far west as the Mississippi River.

(3) The correlation of the northern Appalachian region with the various subdivisions of the Coal-measures and formations immediately underlying them.

(4) The problems connected with the correlation of the Chemung and Catskill groups, and with the correlation of the Waverley and Marshall groups.

(5) The elaboration of the Mississippian series, or "sub-Carboniferous" formations of the Mississippi River basin.

(6) The Permian problems of Kansas and Nebraska.

(7) The correlation problems involved in classifying (*a*) the formations of the Acadian province, and (*b*) the formations of the Rocky Mountains and Western Plateau provinces.

In the discussion of these various problems, the definite stages in the development of the principles of correlation have been recognised.

At the opening of the century, the Wernerian system of classification was adopted. In this classification, the mineral character of the formations was regarded as fundamental.

The second stage took definite shape in the New York system, and while a general "parallelism of strata" was believed in, "gaps" and "intercalations" were assumed, to make the interpretations fit the facts. Fossils played a secondary part, only being considered of value when exact identity was recognised. This principle did not reach satisfactory results, because stratigraphic order and stratification itself offer no intrinsic evidence of the age of a formation, and stratigraphic structure was found not to be uniformly persistent even for a few miles' extent.

William Smith, early in the century, advanced the idea that strata could be identified by the fossils they contained, and we need not trouble the reader with any proofs of the value of this.

Hence in the third stage of the correlations methods, fossils assumed the chief *rôle*, and the minute and exhaustive study of organised beings in their stratigraphical and geological relations has proved to be, not merely the best, but the only reliable guide to correlation of geological formations.

The conclusions reached from these historical studies confirm the belief that the description and nomenclature of structural formations should be quite independent of their correlations, and that precision in correlation must be based upon mature and exhaustive palæontological study, that the time-scale must be made independently of the structure-scale, and that the time-scale of correlation is based fundamentally upon biological data.

The investigations also lead to the further conclusions that, as nomenclature finds its basis in some intrinsic characters of the things named, *uniformity of nomenclature* for formations is impracticable, since the intrinsic characters of formations are local, and have nothing to do with their geological position; and that *uniformity of classification* can be looked for only through an exhaustive biological study of fossils, and is inapplicable to geological structure, stratigraphy, or formation.

When it is mentioned that the Archæan paper occupies 550 closely printed octavo pages, it will be seen that the amount of labour expended on these Reports is prodigious. They are most valuable to the geological student on both sides of the Atlantic, and we regret that space does not allow us to give a detailed account of the several monographs.

Geological Survey of Queensland.

The annual progress of the Geological Survey in this vast territory is not to be estimated by the number of square miles mapped; it must be judged by the separate detailed reports on areas of special economic interest. The work, which is under the superintendence of Mr. Robert L. Jack, Government Geologist, is well organised. The headquarters of the Survey have been removed from Townsville to Brisbane, and there is placed the collection of minerals, rocks, and fossils. There the officers, when not engaged in field-duties, are employed in writing reports, in preparing geological maps, in laboratory work, and in affording information to miners and others. Plans of old mines, wherever possible, are preserved. In some cases it is difficult to find out anything about the earlier gold workings; no plans have been kept; and yet, in certain instances, the workings, not rich enough to pay formerly, may be again approached when cheaper processes for treating the ore come into use. A most important work will thus be done by keeping a permanent record of mining operations.

Among the Reports which we have received are the following:—The Kangaroo Hills Silver and Tin Mines, by Mr. Jack; Geological Observations in the Cooktown District, by Mr. W. H. Rands, with accounts of Coal, Gold, Antimony, and Tin; and Grass-tree Gold Field, near Mackay, by Mr. Jack. These Reports and the Maps can now be obtained in London at the Office of the *British Australasian*, 31 Fleet Street.

The Inheritance of Acquired Characters.

Owing to their coiling, the shells of Ammonoidea and Nautiloidea have furnished biologists with much evidence bearing on theories of evolution, and now Professor A. Hyatt has discovered yet another point, which, he claims, proves that acquired characters have been inherited (*American Naturalist*, vol. xxvii., p. 865). Coiled Nautiloidea have, as everyone admits, been gradually derived from straight forms, such as *Orthoceras*. The cross-section of an Orthoceran, or even of a slightly-curved Cyrtoceran, or loosely-coiled form, is circular or elliptical; but the section of a close-coiled form, like *Nautilus*, shows an impression of that part which comes in contact with the preceding whorl, so that there is a re-entrant curve. In old age, however, when the shells again uncoil, this impressed zone disappears, and the section becomes circular again, a fact which seems to show that the feature is directly due to pressure, and is, therefore, an acquired character. As such it is not found in the early, uncoiled stages of those Nautiloidea that are close-coiled in the adult; at least, it is not so found in any of the Silurian or Devonian species. But at last, in the Carboniferous, Professor Hyatt has found a species that seems to prove his point; for, in *Coloceras globatum*, which is in many respects a highly-specialised species, seven specimens examined have shown this impressed zone existing while the shell was still in the partly-curved or cyrtoceran stage. The same early appearance of the impressed zone is likewise seen in numerous Jurassic, Cretaceous, and Tertiary species, including the living *Nautilus pompilius*. This, then, seems due to the inheritance of a character in successively earlier stages of individual development, according to a well-known law; while the character so inherited is believed to be an acquired one.

Nuclear Division.

In the *Annals of Botany* for September (vol. vii., pp. 393–397), Professor J. B. Farmer publishes an interesting preliminary note on nuclear division in the pollen-mother-cells of *Lilium Martagon*. The processes of karyokinesis in animals and plants now are recognised to be so similar as to suggest identity in cause, and zoologists as well as botanists will read this note with interest. It tells of the discovery of multipolar spindles in cell-division. These spindles stand in obvious relation to granules not unlike centrosomes and are placed at their poles, and there is some reason to infer that these granules in the cytoplasm have been derived from the nucleolus. Of course, multipolar spindles have been observed before, both in plants and in animals, and Hertwig and others have asserted a nuclear origin for the centrosome. But the full publication of Mr. Farmer's results will be awaited with interest, as aberrant cases like this frequently call attention to important details obscure in normal types.

The Flora of Eastern Malaya.

The Flora of the Eastern Coast of the Malay Peninsula forms the subject of the latest number of the Linnean Society's *Transactions* (vol. iii., part 9). Mr. H. N. Ridley, during the past few years in which he has held the post of Director of the Gardens and Forest Department, Singapore, has made some collecting trips on the eastern side of the Peninsula, the Flora of which was practically unknown; the researches of the earlier botanists like Griffith, Wallich, and Scortechini, and of the later collectors, having been confined to the more accessible western side. Mr. Ridley collected chiefly in the native State of Pahang, but also visited the more northern States of Kelantan and Tringganu, and the island of Pulau Tiuman, off the coast of Johore. The number of species collected and observed exceeds 1,200, but this represents only a small proportion of the whole flora, as opportunities of collecting were often limited, while many of the trees and shrubs were not in flower or fruit at the time, and did not therefore afford material for identification. It is suggested that three times this number would not be an over-estimate of the species existing in the area traversed. Those obtained give a fair representation of the lowland and coast flora, and of that of the sub-alpine woods of the Tahan district and of the limestone rocks of Kota Glanggi.

There is a very marked difference between the floras of Singapore and Pahang. Many plants which are common in the southern part of the Peninsula, and especially the secondary jungle plants, become scarce or disappear in Pahang, while there is a large accession of Bornean types. The latter, Mr. Ridley is inclined to regard rather as the relics of an older flora common to the whole of this region, than as forming part of a distinct Bornean flora.

The littoral flora was best seen at the mouth of the Pahang River. Above high-water mark on the sandy sea-shore, was a single row of Casuarinas (*C. equisetifolia*), on the branches of which were many lichens and fungi and a fine orange *Loranthus*, which is described as a new species. The ground beneath was carpeted with many grasses and sedges, Ipomœas, etc. The most striking were the porcupine-grass (*Spinifex squarrosus*) and a beautiful prostrate, blue-flowered *Vitex*. The mangrove-swamps proved less extensive than in most parts of the Peninsula, as, owing to the sandy character of the country, the rivers do not bring down the requisite mud.

Passing up the river towards the capital, Pekan, one enters a great heath district, consisting of flat, open, sandy country, dotted over with thickets and clumps of bushes, alternating with grassy patches. Here were found many interesting grasses and sedges, and among the bushes or trees Eugenias, *Ilex*, several figs, and, growing upon them, many ferns and Loranths, and some small orchids. On the right bank of the river at Pekan, the country is more swampy and less sandy, and large tracts are covered with a new species of *Saccharum*, and great tufts of a palm (*Licuala spinosa*). There are many pools full of lotus, while, in drier places, a *Clerodendron* and a small *Crinum* abound, and on the river banks dense thickets reach to the water's edge, interspersed with pink- or golden-flowered Cassias, orange and red Bauhinias and clumps of white-flowered *Clinogyne*.

The Kota Glanggi limestone rocks in the woods of the island of Tawar afforded a distinct flora, and many good finds, the most important of which were a new genus of Musaceæ, one of Rubiaceæ, a *Trichopus* hitherto known only from Ceylon and Southern India, and many new orchids.

Among the numerous interesting plants found in the woods at the mouth of the Tembeling River, at its union with the Pahang River, was a *Brugmansia*, the first of that most remarkable of orders, Rafflesiaceæ, recorded from the Malay Peninsula. Passing up the river to the valley of the Tahan, a totally distinct flora was observed. Here, the rocky river-banks are clad with a dense forest of trees and shrubs, most conspicuous of which was a new Dipterocarp, bright with pink fruit or scenting the air with large pinkish, cream flowers, while its boughs were laden with *Cœlogynes*, *Dendrobiums*, and many other orchids in magnificent flower. A long, narrow-pointed leaf was very characteristic of the plants growing close to the water's edge, and belonging to very different orders. The author suggests that this may be a protective adaptation against the rapid rises to which the river is subject, from sudden falls in the mountain districts, thus exposing the plants to submersion by a rush of water. Broad foliage would be torn off or mutilated, but narrower leaves, offering less resistance, would be less liable to injury.

In the ravines, down which run the numerous smaller streams to join the river, were Begonias, Aroids, many Scitamineæ, and others. The soil of the woods is in many parts very sandy, and Mr. Ridley thinks that to this is due the paucity of *Termites*, as they are unable to make their subterranean nests in sand which would fall in, the stiffer clay being needed for the dome-shaped chambers and passages. Owing to their absence, the leaves and sticks on the ground decayed slowly and formed a richer soil, while in the clayey woods where white ants abound, the vegetable fragments are rapidly eaten as they fall and rendered useless for soil-fertilisation. In the upper woods of the Tahan River, the ground was permanently sodden with the heavy rains, and at night glowed brilliantly from the luminosity of the decaying leaves. The dense jungles of the Tahan River yield a good quantity of Rattans of many species, but in the more accessible country along the banks of the Tembeling and Pahang rivers, the best have been exterminated. Gutta-trees are plentiful in the Tahan districts, and Pahang gutta-percha fetches a good price.

There was very little cultivation in Pahang, although the soil in many parts is exceptionally good, much better than that to the south of the Peninsula. In the villages along the main river, maize, tapioca, sago, hill paddy, and fruit are chiefly cultivated. The Rajah of Tembeling had some very healthy young Arabian coffee trees in his garden, from the leaves of which he made a kind of tea, being quite ignorant of the use of the berries.

Mr. Ridley's list includes descriptions of three new genera, in Rubiaceæ, Asclepiadeæ, and Scitamineæ respectively, and there is also a goodly number of new species. Thus of eighteen Cyrtandreæ, nine are new, seven of which belong to the genus Didymocarpus, many of the Scitamineæ are new, and no less than twenty-one orchids, including three *Dendrobiums*, four species of *Sarcochilus*, and two of *Phalænopsis*. What seems to be the wild original form of the Patchouli was found a long way up the Tahan River, far away from cultivation; it is quite distinct from the other plants which have been supposed to be the wild parent.

The Seedlings of Conifers.

The last two numbers of *Le Botaniste* consist largely of an account of some anatomical researches on the seedlings of Conifers by the editor, P. A. Dangeard, who claims, among other things, to have put beyond doubt the existence of an intermediate tigellar region distinct from both stem and root, often reaching above the cotyledons, and "possessing a veritable autonomy." He also discusses the varying and often large number of cotyledons in Gymnosperms, which contrasts so markedly with the pair or single one found in the dicotyledons and monocotyledons respectively.

Thus the stone pine (*Pinus pinea*) has 11 to 13, the corsican pine (*P. Laricio*) 8, in the scotch fir the number varies from 6 to 9, in the larch there are 5, 6, or 7, in the white spruce (*Picea alba*) 5 to 7, and so on. One theory explains the large number of cotyledons by the development of new leaf-structures between the original members, while another supposes the subdivision of already existing cotyledons.

The former idea is recalled by the study of the germination of the cypresses. In *Cupressus Lindleyi*, for instance, the internode immediately above the three cotyledons is much shortened, although the following ones are well-developed. The first whorl of three leaves is thus brought on a level with the cotyledons, of which the seedling seems to have six, the three true cotyledons being distinguishable only by their smaller size. Similarly, *Cupressus Corneyana* has apparently four cotyledons. It is, therefore, conceivable that by a gradual approach of the origins of the seed-leaves and those of the succeeding whorl a permanent increase in the number of the former might be effected. Anatomy, however, does not favour this view, as in the cases examined, only the vascular bundles belonging to the cotyledons are inserted on those of the primary root, and M. Dangeard is of opinion that the increased number is due to the division of two large cotyledons, and he derives the numerous cotyledons of the Abietineæ from *Araucaria* or some closely-allied ancient genus. Examination of the seedling of the Chili pine or monkey-puzzle (*Araucaria imbricata*) will easily show how the transformation has been produced. Each of the two cotyledons has a small number of nerves which unite at the base of the leaf into a single bundle; these nerves have become independent and formed as many distinct cotyledonary bundles in the axis, while the limb of the two large cotyledons has split between each bundle, forming as many lobes. The mode of germination thus constructed recalls strikingly that characteristic of the Abietineæ, and of *Pinus* in particular. Variations occur in the genus *Araucaria* which favour this idea; the total number of nerves, instead of being distributed between two cotyledons, may be divided among three, or sometimes four; while, on the other hand, if we examine the course of the cotyledonary bundles and their relations with those of the root in a seedling of the stone pine, we observe, when the number of cotyledons is odd, a fact which recalls the original union of their bundles; some namely uniting with others and not being directly inserted on those of the root. The author remarks that this theory is at any rate not contradicted by what we know of fossil Conifers ; *Walchia*, a genus allied to the subgenus *Eutacta* of *Araucaria*, appears in the Upper Carboniferous, while the pines begin only at the Lias. On the other hand, it is generally agreed that the Abietineæ show close affinities with the Araucarieæ.

The Respiration of Plants.

In Pringsheim's *Jahrbücher*, (vol. xxv., p. 1) there is an account of some investigations by Anton Amm on the intramolecular respiration of plants. With regard to the relations between the amount of carbonic acid produced in this function and the degree of temperature to which the plants were exposed, it was found that the minimum temperature, as in the case of normal respiration, was below freezing point, since at 0° C. a significant amount of the gas was given off. As the temperature rose, intramolecular respiration also gradually increased, but this increase was not proportional to the rise of temperature. In both Wheat and Lupine seedlings the optimum was reached at 40° C., which coincides with the optimum for the normal process. On the other hand, while there is, doubtless, a maximum temperature for the latter function, in the case of the wheat plant and *Lupinus luteus* somewhere about 45° C., there can, properly speaking, be no such point in the intramolecular process, since in absence of oxygen, seedlings cannot stand temperatures between 40° and 45° C. without prejudice to their vitality, and the rapid fall in the respiration curve when the optimum temperature is passed is due to the commencement of death.

The author finds the relation between the amounts of carbonic acid formed in the normal and intramolecular processes to vary with the temperature. Thus in the case of the wheat the relation $\frac{I}{N}$ decreases in passing from 0° to 25° C., and then increases regularly up to 40° C. The values obtained with the Lupine, except for some fluctuations, show a similar fall and rise ; the minimum is, however, reached at 35° C., not 25° C.

The relation between the amounts of carbonic acid gas formed in the two processes varies in different stages of development of one and the same plant, the fraction $\frac{I}{N}$ increasing with increasing development. The present investigations have also supplied fresh confirmation of the fact that, by the withdrawal of oxygen, production of carbonic acid at once sinks in amount but remains constant for a long time at the lower level, and immediately rises again to the original amount when oxygen is again supplied.

Finally, the results show that the different organs of a plant, *e.g.*, flowers and leaves, give an almost identical relation between the normal and intramolecular respiration, while the organs of different species show quite a different relation.

The paper includes an account of the apparatus and methods used in these researches, as well as an historical review of the subject.

THE GROWTH OF PLANTS.

NATURE does not pose as a graceful designer in the rectangular inflorescence of *Siphonychia diffusa*, according to the description which Aug. F. Foerste gives of it in the *Bulletin of the Torrey Club*. "Here is a plant," he says, "with its flowers laid out in rectangular inflorescences—cymes with a flat top with quadrate outlines, or of a form which at once makes us wish to say parallelopipedon, as though the other dimensions were equally stiff and rectangular. In these rectangles, the flowers are laid off almost with the precision of corn rows in a field." This general effect is not lessened by the way in which the inflorescences are arranged on the stem. The latter is almost prostrate, and the cymes terminate the branches at such heights as to fall approximately all in the same plane, and being moreover disposed with their diagonals vertical or parallel to the stem, their sides all become parallel, "so that the final effect is that of a series of rectangular fields, set out in some western landscape, where all lines run north and south or east and west." The writer adds, "the plant must be seen to be appreciated."

The same author also calls attention to the renewed growth of trees in summer after having already once formed their terminal scaly winter buds. In the formation of the buds the plant is planning for the future; in many trees, the warmth of spring has hardly called the vital functions back into vigorous action before the growth for the year is completed, and a few weeks later a well-developed terminal scaly bud awaits the winter. Considerable maturing may be needed before all the characters necessary to withstand the winter's cold are acquired; but the fact is evident that "the more terminal leaves have remained in the crude state of scales when all the freshness of spring was inviting them on to full development to vigorous leaves."

The buds represent a year's growth, and before this year's is finished the tree begins to prepare for the next year's task. Hence there is a certain definiteness to the work, and we can foretell in a measure how much the plant will do from year to year. This definiteness must be related to conditions of climate found in certain areas of the plant's distribution. Northwards the relations between the number of leaves necessary for vigorous development, and the provisions for the same, are so well balanced, that it is rather rare to find woody plants renewing their growth after having once formed their terminal buds. These correlations, however, lose in value in going southward, and many trees, after having already formed such buds in the spring, start growing again in the summer, and again form terminal buds. Under exceptional conditions this may take place three or four times in the course of a year. In the black-jack oak (*Quercus nigra*), near Bainbridge, in Georgia, the author found cases of repeated renewal of growth very common. In the older trees such a growth has taken place twice this year on certain branches, while in young shoots coming up from the roots it has been quite general. In one case, where a fire had checked development for some time by destroying the present year's growth, adventitious shoots formed scaly buds three, or even four, times. It was interesting to notice how nearly equal was the number of internodes developed each time as a striking proof of the fact that this oak has so deeply acquired the habit of producing a certain number of internodes and then a terminal bud, that it repeats this growth and termination at times when circumstances favour a single longer-continued growth before winter buds need be prepared.

Another phase of the subject was presented in some new shoots from a hickory stump, a few of which had made their long growth during one uninterrupted period. In one shoot the internodes got closer together, and the leaves smaller towards the middle of its length—evidently preparatory to the formation of a terminal bud; but the shoot, so to speak, changed its mind, and succeeding leaves became more distant and larger until, later, the real terminal bud was formed considerably farther on. In another, the leaves were reduced to scales towards the middle of the shoot, but the internodes were too long to admit of the formation of a bud, while in several cases terminal scaly buds had been developed but renewed their growth.

The Movements of Plants.

Cobæa scandens is well-known in conservatories and gardens as a graceful climbing plant. It is a native of Mexico, but has been in cultivation for more than a century. In 1805 it was figured for the *Botanical Magazine*, where it is stated that the drawing was taken in July, 1784, at Mr. Woodford's, Vauxhall. It had been previously described and figured by Cavanilles in the first volume of his "Icones," and appears to have been first raised in Europe in the royal garden at Madrid. The movements of the flower-stalk and floral organs during the flowering period form the subject of a recent paper by Max Schultz in Cohn's *Beiträge zur Biologie der Pflanzen* (vol. vi., p. 305).

During the development of the flower-bud the stalks have a strong tendency to grow towards the light and in opposition to the force of gravity, or, to put it briefly, are positively heliotropic and negatively geotropic. The axis of the young bud falls in the direction of the length of the stalk. As growth proceeds, the end of the stalk bends horizontally, and at the same time the pressure of the petals causes the hitherto fast closed calyx to open at the tip. Finally, through the more rapid growth of its upper surface, the horizontal portion dips at a slight angle, giving the flower a nodding position in which it opens. The author shows that these movements are due to the action of gravity, the end of the stalk becoming horizontally geotropic, tending, that is, to lie in a direction at right angles to the force of gravity, while the rest still retains its original negative geotropic tendency.

The flowers are markedly proterandrous. The two upper stamens are the first to shed their pollen, the anthers standing before the entrance to the flower. They remain in this position for about twenty-four hours, and then bend back towards the interior of the flower, at the same time that the anthers of the three lower stamens open, and by curving of the ends of their filaments come to hold the same position at the entrance. After another day these have also served their turn and bend back useless towards the lower edge of the flower, while the style, which has hitherto lain concealed, grows out and curves into the position vacated by the stamens, the stigmas spreading meanwhile. A change has also occurred in the colour and scent of the blossom. While the two upper anthers are functional, the corolla is still greenish and has a pungent, disagreeable smell, but after the three lower anthers have opened, the colour is a deep dull purple, and the scent strong and sweet.

After the anthers have retired and the stigmas remained in position for another day, the upper end of the flower-stalk bends vertically downwards, and shortly after the lower portion comes to lie horizontally. Both these movements result from the action of gravity. Simultaneously with the latter movement, the upper portion undergoes further geotropic changes of position, by which the part immediately behind the flower comes to point vertically downwards, the piece between this and the original place of bending remaining horizontal. It is interesting to note that the horizontal part of the stalk has a dorsiventral structure, the cortical tissue of the ventral side being more strongly developed than that of the dorsal.

These movements of the flower and its stalk are, perhaps, a device for ensuring self-fertilisation where cross-fertilisation by insects has failed; they occur in any case, whether pollination has taken place or not.

Malformation of the Daisy.

A novel malformation in the flower-head of the common Daisy is the subject of a note by Dr. Masters in the last issue of the *Annals of Botany* (vol. vii., p. 381). The specimens were sent the summer before last as a "new British plant"; they were kept through the winter and on flowering this year have reproduced the peculiarities previously observed. The young flower-head was oblong, and the bracts of the involucre fewer in number and less wide-spreading than usual. The outer or ray-florets were of the usual colour, but only five in number; some were spreading, others erect or more or less twisted. By far the most striking peculiarity, however, lay in the disc, where, instead of a great number of separate corollas, was a single petaloid cup composed of several corollas, apparently flattened out and united by their margins. The free border of the tube showed lobes and other signs of a composite nature. Within the cup were the stamens in a single row, very numerous, quite free, and surrounding a club-shaped expansion of the axis which occupied the centre of the flower-head. This dilatation was solid and undivided below, but above gave off a number of deltoid processes, " which doubtless represent bracts or paleæ." In no case, says the writer, was there any trace of styles, ovary, or ovule, except in the ray-floret, which enclosed a two-lobed style as usual. One is tempted, however, to suggest from Dr. Masters's figures and description, that the central head of " bracts or paleæ " might represent the carpels, which have, like the corolla and stamens, become aggregated and then also leafy in character.

It is saying a great deal for the rarity of this phenomenon when Dr. Masters admits that he has never before observed such a case in the family of Compositæ. The nearest approach is a very common malformation of the foxglove, where the corollas of the flowers in the upper part of the raceme are blended into one terminal cup.

Lepidostrobus.

Professor F. O. Bower has been examining in the light of recent advances in Palæophytology the structure of the axis of the cone of *Lepidostrobus Brownii.* This fine fossil specimen, preserved in the British Museum, was the subject of a paper, by Robert Brown, in vol. xx. of the Linnean Society's *Transactions*, where the author, as Professor Bower points out in his communication to the *Annals of Botany* (vol. vii., no. 27), described details of great importance in classification which have been neglected by subsequent workers. In the paper now before us, attention is called to the close correspondence of the tissues to those of *Psilotum*, both as regards arrangement and structure; while in the stellate, connected central xylem, the fossil bears a certain resemblance not only to the Psilotaceæ but also to species of *Lycopodium* and *Selaginella*. As the author has recently demonstrated points of similarity between the sporangia of the fossil genus *Lepidodendron* and those of *Tmesipteris*, this correspondence in internal anatomy between *Lepidostrobus* and the Psilotaceæ becomes especially interesting.

In a comparison of the cortex of living Lycopods and Psilotaceæ, of the *Lepidostrobus* and stems of *Lepidodendron*, the author shows that while considerable variety of detail is manifested, it is possible to match the different types of structure found in the fossils with similar characters in closely-allied living forms. He also concludes, by a consideration of another set of examples, that the well-known trabecular development in *Selaginella*, traces of which also occur in *Lepidostrobus*, is a specialised and more definite example of that lacunar development which appears in such various forms and positions in cortical tissues of other Lycopodinous plants.

All these considerations, the author remarks, serve to draw the Lycopodinous plants of the present and past more closely together as a natural family, while it is interesting to note that the lines of similarity "do not focus themselves specially between any two genera, but are such as to suggest complex cross-relationship between the several representatives of this very natural series."

B. H. Halsted has been investigating (*Bull. Torrey Botanical Club*, October) a disease which has, in the last two or three years, become very common among hot-house Pelargoniums in the United States. The leaves lose their healthy green colour, and become speckled or blotched with yellow, while corky ridges are found on the stem and petioles, and the whole plant may become sick, stunted, and useless. The usual appearance on the blades is that of numerous specks, which seem to be supercharged with water, giving the part a clear amber look when held up to the light. A similar appearance in Carnation leaves, previously studied, was known to be due to micro-organisms, but, on investigation, no trace of bacteria or infection by inoculation could be found in the Pelargonium, and it was concluded that the plants were suffering from a dropsical affection due to excess of water and insufficient light. Professor Atkinson has described a similar trouble which he calls Œdema of the Tomato. The remedy would seem to be a cooler, drier soil and increased light for the aërial parts wherever possible.

An explanation of the want of accord in the results of estimation, by methods hitherto used, of theobromin and caffein in specimens of tea and coffee, preparations of cacao and cola nuts, is supplied by some recent work by A. Hilger and other chemists, on the cola nut and seeds of the cacao. In both of these there exists a nitrogenous glucoside, from which caffein or theobromin respectively, besides other compounds, are produced by action of a diastatic ferment, also present in the seeds. The authors succeeded in isolating both the glucoside and the ferment. To get trustworthy results in the estimation of theobromin or caffein, the glucoside must be completely split up before these bodies are isolated. In the cacao it is broken up into theobromin, dextrose and a non-nitrogenous body, cacaoroth, while with the cola nut an analogous reaction obtains, caffein and kolaroth being formed. For details, the reader is referred to the *Deutsche Vierteljahrsschrift für öffentliche Gesundheitspflege*, vol. xxv., pt. 3.

In the *Annals of Botany* Mr. E. H. Acton gives the result of an analysis of wheat grain grown near Canterbury nearly thirty years ago, and threshed in 1892, and compares it with some of last year's crop from the same field. As was to be expected, the old grains contained much less water than the new, the proportion being 9 and 14 per cent. respectively, while the insoluble food-material stored in the seed, such as proteids and starch, were found to have changed considerably in the direction of compounds soluble in water. Thus whereas in the new wheat scarcely one-seventh of the proteid matter was thus soluble, in the old nearly half dissolved, and while no sugar was found in the new, and only just over 1 per cent. of dextrins, the old contained 6·2 per cent. sugars and 6 per cent. dextrins; with, of course, less starch. These changes are probably the result of the action of ferments which were originally present, but have since perished, as the power of converting starch or proteid was found to be nil. This being the case, it is not surprising to find the author describing the old sample as "apparently dead," having shown no signs of germination under favourable conditions maintained for two months. In the absence of ferments to break up the insoluble albumen, starch, &c., the supply of food and energy required for germination would be unattainable.

Nature lovers in our own country are often pained by the vandalism of so-called naturalists, who, possessed with the rage for collecting, threaten the existence of our rarer plants and animals. Now, according to the *Orchid Review* (November), the Rajah of Sarawak is closing his dominion to collectors, owing to the depredations committed by orchid hunters and the like. It is scarcely to be wondered that a man objects to have the rare and beautiful objects of the fauna and flora of his country carted off wholesale to gratify the passing whim of a moneyed class in another hemisphere.

Mr. G. F. Scott Elliot is again in Africa on a botanical and generally scientific expedition. This time his destination is Uganda, which he hopes to reach *viâ* Mombasa and the Victoria Nyanza. Mr. Elliot, who is known to readers of Natural Science as well as to botanists in particular, has already touched the great continent at several points. A few years ago he made a trip through the Transvaal and Natal, and shortly after spent some time in Madagascar. His next visit was to the North, where he was anxious to work at the Morocco Flora, but, owing to political disturbances, was unable to get into the State, and so went on to Egypt, and collected up the Nile, as far as the Wady Halfa. Finally, about two years ago, he visited Sierra Leone as botanist to the Boundary Commission. The scientific results of these expeditions have been published in the Linnean Society's *Journal* and the *Annals* and *Journal of Botany*. Uganda is a promising field, and we wish Mr. Elliot a successful trip and a safe return with a rich harvest of results.

Mr. Ernest Gedge, whose admirable letters to the *Times* as Special Correspondent of that paper in Uganda are, doubtless, well known to all our readers, has just returned to the coast, and is now on his way to Matabeleland. Mr. Gedge's hasty return is to be regretted, as otherwise he would, probably, have carried out his original programme and visited the unknown but important region between Elgon and Karamoyo on his way back to the coast.

Mr. Herbert Ward has an article in *The English Illustrated Magazine* for November entitled "Martyrs to a New Crusade." He gives a brief sketch, with portraits, of those of Stanley's companions who passed away, either during the last expedition, or from privations endured while attached to it. Major Barttelot, Thomas Heazle Parke, James Sligo Jameson, Robert Nelson, and Captain Stairs, are pathetically sketched by their comrade, who also pays a tribute to the fidelity of the black followers.

Those interested in slugs will find in the December 21 number of *The Conchologist* the completion of Professor Cockerell's Check-List of those molluscs. It is difficult to estimate the value of these publications, where one can find forms described in widely-scattered publications all collected together for the first time. The author knows as well as his critics the weak points in his list, and offers it for destruction and re-construction, until reasonable finality is reached and permits of a revised edition.

The editor, Mr. W. E. Collinge, has made sundry comments upon the notes of the author, and these, though in many cases apt, are testy, and do not, moreover, justify the insertion of Mr. Collinge's name on the title as joint-author. One remark of Mr. Collinge's at first strikes one as very sound, viz., that modern science "demands a knowledge of internal as well as external morphology, and *rightly refuses to recognise inadequate descriptions, or descriptions of shells apart from the animal, or to acknowledge genera or species founded upon purely external features*." But, however much we may deplore the naming of a shell without study of the mollusc which produced it, we must not forget the difficulty in most cases in obtaining the animal, and it is a debatable question whether it is not better to name the shell when found, rather than wait for the soft parts to be forthcoming. In any case, we hope that Poli's idea of naming both the shell and the mollusc will not again come into favour.

The siphuncles, or, to speak more correctly, the siphuncular tubes of the straight forms of Palæozoic Cephalopoda present many variations in structure that greatly puzzle the morphologist. In some interesting "Remarks on Specific Characters in *Orthoceras*" (*Amer. Geol.*, vol. xii., pp. 232–236), Aug. F. Foerste adds an item to our knowledge by showing that in *Orthoceras erraticum* the siphuncle moved, during the growth of the shell, from one side to the other, while the siphuncular tube became less annular and more cylindrical. Similar movement of the siphuncle is known in coiled Ammonoidea, and may be accounted for by the coiling; but one fails to see its meaning in a straight and regularly conical shell.

ANOTHER curious instance of variation in Cephalopoda is recorded by A. Appellöf (*Bergens Museums Aarbog* for 1892, p. 14, just published). In all Octopoda one of the arms, usually the third on the right, undergoes a peculiar modification for reproductive purposes, which is known as "Hectocotylisation." In a specimen of *Eledone cirrhosa*, Dr. Appellöf has found the third arm on the left also similarly affected, without, however, any corresponding duplication of the genital opening.

THE clever sketches called "Zigzags at the Zoo," published in recent numbers of the *Strand Magazine*, are above the average. We do not remember to have seen anything more funny than the drowning of the two bluebottles by the rhinoceros, "Tom." Mr. J. A. Shepperd has exactly caught the flap of the ears, and his drawings are full of suggestion and life. The comparison of the racoons to a couple of well-known music-hall "artistes," if not in the highest degree scientific, has a sense of humour doubly dear to one whose wits have been much muddled by ponderous verbosity; while no one, after laughing over the illustration, will fail to remember that "*These animals bite.*"

AN elaborate memoir, entitled "A Contribution to the History of the Geology of the Borough of Leicester," by Mr. Montagu Browne, has been published by the Leicester Literary and Philosophical Society (*Trans.*, vol. iii., pp. 123–240). It contains a very full account of all that is known about the Keuper and Rhætic Beds, the Lower Lias, and the Glacial Drifts; with notes on the Water Supply. The Palæontological Tables enumerate the fossils found in the several formations in the district, including the "derived" specimens obtained from the Boulder Clay, and those found in Pleistocene valley-gravels and more recent deposits. The authorities for each record are noted by means of a somewhat complicated system of symbols.

AN excellent account of the Geology of Dublin and its neighbourhood, from the pen of Professor W. J. Sollas, appears in the *Proceedings of the Geologists' Association* (vol. xiii., pp. 91–122). It is well illustrated, and was intended as a guide to the members of the Association who visited Dublin and Wicklow last July.

We learn that Professor Huxley will contribute a final chapter to the "Memoirs of Professor Owen," which are promised early next year. This will be an estimate of Professor Owen's work, and the scientific public will await with considerable interest the words of one so peculiarly and eminently fitted to deal with this important subject.

Apropos of the article in our last number on Natural Science at the Chicago Exhibition, we may mention that the *American Geologist* intends to give very full accounts of all the geological exhibits at the Fair. The October number contains a description of the Geological Maps and Models, and also gives a summary of the proceedings at the World's Congress on Geology, which was held in Chicago during the week August 21 to 26.

It is announced that *The Conchologist*, a quarterly journal edited and published by Mr. W. E. Collinge, of the Mason College, Birmingham, will be known in future as *The Journal of Malacology*, the first part of vol. iii. being issued in January, 1894. For the present, it will deal almost exclusively with the slugs, and its aim will be to focus within one journal abstracts of the current literature relating to these molluscs, while affording a means of publication for original work.

Our reviewer, when he wrote the recent notice of the *Zoological Record*, must have been gifted with second sight; at all events, no more apt illustration of his ironical remarks anent the customary mode of reviewing that work could have been afforded than the notice which made its appearance but a day or two later in the columns of a weekly contemporary (*Athenæum*, November 11).

The whole notice is literally taken up in calling attention to those misprints and other oversights from which, owing to human frailty and the direct interposition of the printer's devil, a work of this character is never free. The only suggestion offered by this would-be smart critic is to repeat his last year's recommendation that the editor of the *Record* should procure a blue pencil and use it. Apparently this editorial requisite is equally wanted in other quarters, for we can hardly imagine a critique (?) such as our contemporary has admitted being allowed to appear in any other journal.

One is tempted to wonder how similar works, did they come up for review, would be handled. What, for instance, would be said about the valuable, and formerly well-edited, *Journal of the Royal Microscopical Society*? In the biological record there published we habitually find notices of papers on invertebrate embryology mixed up with those on vertebrate embryology, under the heading "A.—Vertebrata." Misprints, too, are by no means unknown there, and we have met with an instance where two papers on very different subjects, and pages apart, have politely exchanged references.

I.

High-Level Shelly-Sands and Gravels.

PROBABLY there is no subject of geological controversy that has provoked more animated discussion than the shelly-sands and gravels, found in various localities in the British Isles and Ireland, at levels ranging up to about 1,400 feet above sea-level. The question that excites geologists is the same that is reported to have troubled George the Third respecting the apples in the dumpling—how they got there. Having received a cordial invitation from the Editor of NATURAL SCIENCE to unburden myself on this topic, I gladly avail myself of the opportunity.

And here I am met with my first difficulty. I shall, in the course of this article, have to call these deposits High-Level *Glacial* Drift, which I suppose is begging the question, as their *diluvial* origin is stoutly and ingeniously contended for by no less an authority than Sir Henry Howorth. However, as I simply adopt the name that these sands and gravels are generally known by, I hope I may receive absolution.

Among working geologists of the present day only two explanations are offered as possible.

The older one, which passed unchallenged for over a quarter of a century, is that these shelly-sands and gravels were laid down when the relative levels of land and sea were different to what they are now, and postulates a subsidence of the land or rise of the sea, known as "the Great Submergence."

The newer hypothesis adopted by geologists who find various difficulties which they think are unexplained by the submergence theory, is that the high-level shelly drift is sea-bottom, not *in situ*, but carried up to its present position frozen in the sole of a glacier, or pushed up in front of it while advancing from Scotland over the bed of the Irish Sea.

It is difficult to give the precise details of how it is conceived this operation was carried on, as I have never yet been fortunate enough to meet with much more than a general statement, but one hypothesis is that the sands and gravels were released from their icy matrix by the sudden melting of the ice at the close of the Glacial Period, hence the rounding of the stones and the false-bedding of the deposits.

It is evident to anyone of ordinary common sense in scientific matters that such questions can only be answered—if they can be answered—by appeal to facts; unfortunately—or perhaps fortunately—facts present themselves in different colours to different minds, and what one looks upon as conclusive proof of the action of the sea, another thinks points decisively towards land-ice. Let us, then, shortly review what is known of these chameleon-like drifts, and try to present the information in a way that each reasoner may have an opportunity of forming his own conclusions—if, indeed, he feels equal to the task of taking so much trouble.

The first discovery of the high-level shelly drift in the British Isles was made by Joshua Trimmer, and recorded in the *Proceedings* of the Geological Society in 1831. It occurred on Moel Tryfaen, Carnarvonshire, at a level of between 1,300 and 1,400 feet.

There have been many discoveries in other localities since, but Moel Tryfaen remains classic ground, not alone on account of priority, but because of certain features which differentiate it from the other shelly-sands and gravels, of which I have given detailed descriptions and illustrations in a monograph lately published by the Liverpool Geological Society.[1] The next discovery was made by the veteran geologist, Mr. Joseph Prestwich, of a shelly patch near the Setter Dog, between Macclesfield and Buxton, at a level of about 1,200 feet above the sea. The Macclesfield Cemetery beds, at a level of about 600 feet—consisting mostly of shelly-sands some 70 feet thick—followed, and were described by the late Dr. Sainter and Mr. R. D. Darbishire.[2] Similar shelly-sands and gravels were discovered and described in Flintshire at Halkin Mountain, and up to 1,000 at Moel y Crio, by the late Daniel Mackintosh; and between Minera and Llangollen the same indefatigable observer, to whom Drift Geology owes so much, found an extension of the same shelly drift at levels ranging from 1,000 to 1,200 feet. Still more recently, these high-level shelly-sands and gravels have been traced further south, and occur at Gloppa, two miles from Oswestry, in great force up to the level of 1,200 feet above the sea, having a depth of 60 feet unbottomed. Mr. A. C. Nicholson, F.G.S., has given an excellent description of these beds[3] which, fortunately for geologists, he assiduously worked during the whole of the excavations made in carrying out the works of the new water-supply for Liverpool from the River Vyrnwy. Meanwhile, geologists in the Sister Isle were not idle, and the Rev. Maxwell H. Close recorded the presence of a remarkable deposit of shelly-gravels at Ballyedmonduff on the Three-rock Mountain, near Dublin, at an elevation of over 1,200 feet,[4] and of other exposures at various

[1] The Drift Beds of the Moel Tryfaen Area of the North Wales Coast; Session 1892–3.

[2] Notes on Marine Shells found in the Stratified Drift near Macclesfield, by R. D. Darbishire. Lit. & Phil. Soc. of Manchester, Session 1864–5.

[3] *Q. J. Geol. Soc.*, 1892. [4] *Geol. Mag.*, 1874.

levels of a similar nature, indicating a wide extension of these deposits penetrating into the northern parts of the Wicklow Mountains.

Other minor discoveries of shelly-sands, gravels, and clays have been made at various localities and at varying heights, but the purport of this paper is to deal with the prominently High-level Shelly Drifts, as bearing upon the two opposing theories of their origin.

Premising that I speak from personal observation of all the deposits the history of whose discovery I have briefly sketched out, I may say that, discounting local conditions, they have all certain characteristics in common. They are these—

1. The rocks and materials of which they are composed are largely foreign to the locality, but mixed to a greater or less extent with local materials.

2. Some of the rocks are far travelled and from different directions, but generally from a northerly direction.

3. The foreign materials are often above the level of their parent rock.

4. The pebbles, boulders, and small gravel are to a considerable but varying extent well rounded, a few are striated, some planed.

5. The sands have often all the aspects of marine sands containing a very large proportion of polished and rounded quartz grains.

In cases where local material preponderates, or materials which may have only travelled a few miles, as at Ballyedmonduff, highly-polished quartz grains are often disseminated through the mass, and can be obtained by washing and riddling.

6. As their name indicates, these sands and gravels contain sea-shells, mostly in a fragmentary condition, but often well preserved, especially the gasteropods. In fact, shells in any gravelly beach present the same aspect. The fragments are frequently worn at the edges, and microscopic grains of shell-fragments can be obtained by washing, and distinguished by the aid of the microscope.

7. The local rocks and gravelly materials are frequently angular, though sometimes well rounded—they are, as a rule, more angular than the travelled materials.

8. The gravels and sands are often well bedded, commonly current-bedded.

9. In some cases these characteristic marine beds are underlaid or overlaid, or both, with characteristic glacial till, composed almost wholly of local rocks and materials that have come down from a higher level. Moel Tryfaen is the only case in which I have seen this phenomenon, but I have no doubt there are others could we bare the ground. It can only happen in localities where there are high mountains near, commanding the site of the beds.

10. Finally, the whole aspect of the shelly-sands and gravels is that characteristic of aqueous deposits.

There are other high-level sands and gravels which, though no shells may have been detected in them, can be identified as of the same nature and origin.

Interbedded with these sands and gravels we find beds of clay and of dirty sand and gravel, but the typical sands and gravels are very clean and require no washing when mechanically separating their mineral constituents by means of sieves of small mesh.

It must not be thought that shelly-sands and gravels are confined to the high-level localities mentioned. On the contrary, they are to be found at all levels from the sea to the highest mentioned. They vary according to locality and the nature of the surrounding rocks, but they are essentially the same deposits, whether found at fourteen or fourteen hundred feet above the sea. For instance, there is a striking similarity between the shelly-sands and gravels of Howth, near Dublin, which rise from sea-level to about 350 feet above it, and the high-level deposits already described at Gloppa, near Oswestry, 1,200 feet above the sea. The foreign rocks they contain differ; at Gloppa they are mostly from the Lake District, Scotland, and Wales, possibly some of the flints may be from Antrim. At Howth the rocks are from the northern parts of Ireland and from the west. They have not, however, been examined to anything like the same extent as in England.

The low-level glacial deposits are differentiated from the high-level by the greater prevalence of Boulder Clays in the former, and these clays are as a rule distinguished by the still greater prevalence of foreign rocks and their more frequent and intense glaciation. These clays also contain shell fragments, and on washing yield just the same mineral grains and rounded and polished quartz grains. The Boulder Clays cover a very large area of the North-West of England. They are the purest at a distance from mountain masses; near them, local materials preponderate.

I have now sketched out the salient features and differences of the high-level and low-level drifts. It would be easy to multiply details, but my object and meaning might then get deeply buried under a mass of what might be called sedimentary information. For anyone who wishes to peruse the subject, there exist papers sufficient to supply continuous reading of the most solid kind for two or three years to come. It would be interesting to know in what state of mind a reader would emerge from such a task, probably his last state would be worse than his first.

By the earlier and perhaps simpler-minded geologists who paid attention to the subject, such as Trimmer, Ramsay, and Lyell, the position of these shells in the sands and gravels not being explicable by hypothesis of cartage or kitchen-middens, carriage by birds or pilgrims, nor even by waves of translation, were accounted for by submergence of the land by subsidence. They were looked upon, in fact, as marine beds *in situ*, their heterogeneous character being due to floating ice, which brought contributions from many localities.

Latter-day geologists coming from the study of glacial deposits in other countries have felt difficulties in accepting this explanation. They have found that an ice-sheet comparable to that of Greenland of the present day best explains the phenomena in the countries in which they have studied the glacial drift, and they seek to apply the same explanations to the Drift of the British Isles. The low-level drifts are more easily dealt with on this hypothesis than the high-level, but as they are essentially of the same nature, it seems necessary for consistency' sake to apply the same explanation to both. There are also certain phenomena which are thought to be inconsistent with the submergence theory, one of them being the position of boulders at a level higher than the parent rocks from which they were derived.

Again, it is said that the high-level drift is very partial and sporadic in distribution, that there is no marine drift in the interior mountain valleys, that there are no raised beaches, that the shells are always broken as if a heavy body had moved over them, that they occur mixed in species in a way that never happens in nature; that the two valves of bivalves are never found united—that they never occur in any bed as if the molluscs had lived on the spot, and finally, the species characteristic of warmer climates are mixed with those of colder climes, such as never occur together in the sea at the present day. Furthermore, the advocates of submergence are asked to produce stones from the Drift with the remains of barnacles or other organisms upon them.

It is also roundly asserted that rocks have never been found north of their origin, and by implication never will be. It is also suggested that, if the sea has flowed over the land to such a depth as the submergers require, deep-sea beds or beds of some sort containing sea-shells *in situ*, like what we find in the so-called Clyde Beds, should be common, whereas they do not exist.

The shading off of the glacial deposits southward is also looked upon as consistent with a land-ice, and inconsistent with a sea-borne origin.[5] These are what we may classify as objections founded upon observation. I think I have catalogued the greater number, but if any are omitted I have not the least doubt that I shall be reminded of them. In the meantime, we will consider another class of objections that are physical and theoretical. The subsidence of this solid land without evidence of volcanic action seems to some minds so improbable as to demand the clearest and fullest proofs before it can be admitted. The worst is, that when one condition is satisfied and paid

[5] What are called striated pavements, in which the striæ in the boulders are approximately parallel, are pointed to as evidence of the passage of an ice-sheet. These are, however, only found, so far as I know, at low-levels in the Boulder Clay. It is considered that floating ice could not have affected the boulders in this way. I do not propose to examine now the further question of bed-rock striation which is a much wider one than the origin of the Marine Drift.

for in mental coin, the price is raised and more demanded. Granted the subsidence, another difficulty looms ahead, it must have been differential, and it is pretty positively asserted that no evidence of any kind exists of subsidence south of the British Channel. Also, if there had been differential subsidence and elevation, faults of glacial date might be looked for in the solid crust, whereas there are none.

These objections as a whole are considered by some geologists serious enough to warrant the introduction of an agent to account for them, of which, unfortunately, we have very little knowledge. This agent is land-ice, and to Greenland we are asked to project our mental vision that we may understand the Glacial Period. I am now neither denying nor affirming the former existence, or probable former existence, of an icy covering to our isles such as is predicated. If, instead of the theoretical possibilities of such a tremendous agent as is postulated being dwelt upon, we were treated to actual facts drawn from the investigation of the Greenland ice-sheet, some of us would have a more comfortable assurance of being on solid ground forming a base for the reasoning process. Unfortunately, in the absence of recorded observations, we have to fall back on theoretical considerations alone. I trust it will not be considered arbitrary, when dealing with an agent possessing such potency for work of varied kinds as is claimed for an ice-sheet, to ask for the fullest proof of its powers. Formerly the proof of the action of land-ice was looked for in the local nature of the materials of the Boulder Clay, which represented the ground-moraine, together with the preponderance of planed and striated stones. Now everything in the Drift—foreign rocks, shells, local materials, rounded pebbles and boulders, sea-sands, are all claimed as the effect of land-ice, whether found on the top of a mountain or in the bottom of a valley.

There is a system of land-ice physics worked up with astonishing ingenuity and skill by which all these puzzling phenomena are theoretically, if we admit the premises, fully explained. If delicate shells are found intact in the Drift at an inland locality such as Gloppa, forty miles from the sea, it is suggested that they have been conveyed over hill and dale simply frozen in the sole of the glacier, forgetful of the fact that the generally comminuted condition of the Drift shells had previously been claimed as a proof of the passage of land-ice across a sea bed. Other materials, such as foreign rocks, have been conveyed in the heart of the glacier, and the whole—rocks, sea-sand, shells have been washed out, rounded and deposited in stratified masses on the sudden melting of the ice which is supposed to have occurred at the close of the Glacial Period. Unfortunately, no instances are quoted of any analogous kind of work performed by existing glaciers or ice-sheets. It is of this dearth of examples that geologists accustomed to the methods of investigation pursued by Hutton and Lyell have a legitimate right to complain. We should all be glad to assent to these various propositions if adequate proof were forthcoming, but unless some more solid food is vouchsafed us, I fear many will die unconvinced.

With so much theory and more hypotheses we might reasonably expect the theoretical physics of the ice-sheet to be quantitatively worked out. But have they been? Has anyone yet, in proof of the view that the shell-beds of Tryfaen have been pushed up from 1,200 to 1,400 feet above the level of the sea by a glacier advancing from the north over the Irish Sea bottom, calculated the thickness of the ice required at the "area of greatest precipitation" to overcome the glacier descending from Snowdonia. As concrete examples cannot be quoted of such work being performed by land-ice either in Greenland or elsewhere at the present moment, it becomes important for the support of the hypothesis that its theoretical possibility should be established. If this has been done the investigation has not come under my notice, though I have been studying glacial geology well-nigh a quarter of a century.

Unknown agencies are very unsatisfactory things to deal with scientifically. It is as easy to deny as to affirm, but as the affirmations seem to bottom upon the alleged impossibility of any other agent than an ice-sheet being able to produce all the complicated phenomena of the Drift, it will be the most reasonable procedure to examine also the validity and force of the various objections which have been urged against the older theory of submergence, and which I have in the preceding pages attempted to summarise.

We will deal with the objections to the submergence theory seriatim.

1.—*Boulders, shingle, and gravel are found above the level of the rocks from which they were derived.*

This by some is considered conclusive proof that the moving agent must have been land-ice. Here, again, we want satisfactory examples of an analogous kind drawn from existing glaciers. It seems to me much more probable that a glacier such as the Irish-Sea glacier is supposed to have been, would shear in its own substance, than that it would push sea-bottom before it uphill. The same reasoning applies to the supposititious case of the sea-bottom getting frozen into the sole of the glacier, for if this were to take place the obstruction to the movement of the ice over the sea-bottom would be increased. We may also ask where the shearing took place if not in the ice itself? It is difficult to conceive a sea-bottom frozen solid shearing under such stresses. Alternate thawing and freezing of the bottom may be suggested to meet the difficulty, but this would render the carriage of shells in the sole of the glacier without breaking more hard to conceive than ever. But may it not be justly asked whether resort to all these ingenious suppositions is not a sign of weakness?

Let us now consider what effect submergence would have. A study of any of our shores will yield ample proof that the sea works material shorewards, and throws it up sometimes out of its own reach. In a subsiding area, material might in this way be worked up a considerable distance above its original level. I believe the sea alone is capable of doing this, but assisted by shore-ice it would be more efficacious, and would account for some of the striations on the stones.

The material in which the erratic stones is embedded is unmistakably shore sand.

The sand of Tryfaen could not have come from anywhere but the sea itself. It also has been worked uphill, for there is no local material to yield it, and the wear and polish of the grains are distinguishingly marine features.

To anyone who has paid attention to this part of the subject, its marine character is unmistakable. The rocks that have been worked uphill are, as a rule, water-worn and rounded.

2.—*The High-level Shelly Drift is partial and sporadic.*

If the high-level drift were due to submergence, say the land-ice theorists, we should find it all over the hills, whereas it is confined to a few elevated places. This seems to me to be a singular argument. According to the postulate, the whole of the Drift above sea-level has been pushed up. Lancashire and Cheshire are pretty nigh covered with Drift. So much so, that almost every boring made into the solid rock through it involves more or less change in the mapping of the Triassic beds.

This is no disgrace to the surveyors who mapped them; they could not see through the Drift—even if Drift-geologists can? If, then, the ice-sheet only leaves sporadic deposits, why is not the Drift to the height of, say, 400 feet sporadic instead of continuous? Whether placed there by an Irish-Sea glacier or by the sea itself, both high-level and low-level Drift obey the laws of gravitation, consequently the most material is found at the lower levels.

In working out this objection, the supporters of the Irish-Sea glacier unconsciously minimise the quantity of high-level Shelly Drift—there is much more in existence than they have persuaded themselves to believe. There is no occasion for me to name the localities over again, as I have stated them at the commencement of this paper. Again, it can hardly be expected that all high-level deposits laid down by the sea should contain shells, and further it is not philosophical to assume that all high-level shelly drifts have been discovered. Their discovery has generally been in the nature of an accident. I fear the advocates of the Irish-Sea glacier are continually forgetting how difficult it is to prove a negative, and, unfortunately, their arguments are too frequently of the negative kind.

But, whether laid down by an Irish-Sea glacier or by the sea, the high-level drifts have been subjected to very active denudation since, as the rainfall is much greater in these elevated localities than on the plains, and the time that has elapsed since they were deposited, in my estimation, is certainly not less than 60,000 years. I know that some good geologists have persuaded themselves that not more than 10,000 years have elapsed since the melting of the ice, but, though respecting their opinions, I cannot agree with their premises.

3.—*The shells are always broken as if a heavy body had moved over them.*

This formula is not strictly true, especially in the light of modern discoveries. There is no doubt, however, that the generally fragmentary condition of the shells has been a source of difficulty to observers in the past. The demands of the opponents of submergence are, however, rather exacting. They ask to be shown bivalve shells with both valves united. I have personally never found them in this condition, excepting in the Clyde Beds, but I am assured by Mr. S. A. Stewart, of the Belfast Museum, that he has taken them out of the glacial beds in the neighbourhood of that city with the valves together.[6] In *Nature*[7] is the *précis* "of a most careful and important investigation into the shell-bearing clays of Clava in Nairn," in which a "shelly blue clay with stones in the lower part," 16 feet in thickness, overlain by 63 feet of Boulder Clay and sands, and reposing upon 36 feet of coarse gravel and sand and brown clay and stones, is described. "The highest part of the shelly clay is 503½ feet above sea-level, and the deposit appears continuous for a distance of 190 yards." "The shells are well preserved, neither rubbed nor striated, and the deposit is a true marine silt which, if not *in situ*, must have been transported in mass." The majority of the committee of investigation "consider the evidence sufficiently strong to prove a submergence of at least 500 feet." Here, then, appears to be the very thing the land glacialists are asking the submergers to produce; but, unfortunately, their demands are again varied, as some are not satisfied, and assume that the bed has been shifted *en masse*.

Considering that the valves of bivalves are only held together by ligaments that readily decay, it is not surprising if we do not find them in apposition in deposits which show evidence of strong current action, as most of the marine glacial deposits do, especially the high-level shelly gravels; perfect single valves are common enough and delicate univalves such as *Trophon*, *Nassa*, and the fine spires of *Turritella*.

The high-level shells are beach shells, that is, they consist of shells such as are thrown up and can be found together on many beaches. We do not, of course, get the exact *facies*, but in this connection I may say that I have found on the Crosby beach, with one exception, the whole of the shells named by Forbes from Trimmer's

[6] See Mr. Stewart's "Mollusca of the Boulder Clay of the North-East of Ireland." Proc. Belfast Naturalists' Field Club, 1879-80. "*Leda pygmæa*—Nock, Woodburne, Ballyrudder." "Almost invariably found in a perfect state."

[7] Reports of British Association papers, Section C., *Nature*, Sept. 28, 1893, p. 532.

original find on Moel Tryfaen. On the same shore, near to where I live, my sons and I have found no less than 70 species, though, probably, not more than 20 live in the immediate locality. On gravelly beaches the fragmentary shells far outnumber those that are unbroken. The glacial shelly fragments are also generally much rounded and water-worn. If we add to the ordinary effect of wave action that of shore-ice, which would prevent, except under very peculiar circumstances, the preservation of any deposit undisturbed, the mystery with which these glacial shell-beds has been surrounded is largely dissipated. Of course, it would, in some respects, be more satisfactory to meet with the shells as perfect as in a museum, but in that case I make no doubt that their glacial origin would not be admitted. The explanation of the fragmentary condition of the shells also explains why barnacles are not found on the stones. The mixture of northern and southern species takes place at the present day at Cape Cod.

4.—*The Rocks of the Drift are never found north of their origin.*

This is another sweeping statement which wants more proof than that vouchsafed us. Very little attention has been paid by observers to the point, though, doubtless, the great majority of the stones are from positions north of where they are found. The tracking of erratics to their origin is a very difficult and laborious process in most cases, as I know from personal experience. It is only where the masses of rock yielding them are in considerable force, and their character marked like Shap and Eskdale granite, that this can be satisfactorily done. It can be done, in other cases, by a system of tracking and exclusion. To differentiate Dalbeattie from Criffel granite when found in the Drift, as has been attempted, is well-nigh impossible. Eskdale granite is to be found in the Drift between Ravenglass and St. Bees, north of its origin. The rocks yielding erratics in the greatest abundance must have been the sites of glaciers, and these were principally in the Lake District and in Wales. How far north of their origin Welsh rocks are to be found cannot be known, because the sea occupies the space to the north. Charnwood Forest rocks are found in the Boulder Clay of Nottingham, and therefore must have travelled 15 miles northward (*Q.J.G.S.*, vol. xlii., p. 480). Other examples could be quoted of varying value. A great deal of gypsum is found in the Drift by the Estuary of the Dee, and this, there is every probability, came from further south, in Cheshire. There is, however, an absence of characteristic rocks south of Lancashire and Cheshire by which to trace the flow. In the high-level shelly drift the rocks, though from all levels, mostly come from localities where they may have been derived from high levels.

In my opinion, the distribution of the erratics is quite explicable by the prevalence of north-westerly winds, and by the tides. It is not probable, with a tidal current running through the "Severn

Straits" from the bight of Liverpool Bay, that many, if any, erratics would cross it westwardly from Wales.

Eskdale and Scotch granite are found along a base line from Macclesfield to Carnarvonshire, and for the two granites to be so distributed they must have crossed each other in their courses. The much talked-of Riebeckite—which is identified with that of Ailsa Craig, Professor Sollas informs me—has been found in the Drift south of Dublin; it also occurs west of Liverpool, as well as at intermediate localities in Wales. For an Irish-Sea glacier to perform this prodigy of distribution seems incredible. A glacier distributes its load in stream-lines, and the stones from one side do not cross to the other, and *vice versâ*.

5.—*If the land has been submerged, deep-sea beds containing shells in the position in which they have lived and died should be common, whereas there are none.*

This is practically number 3 objection in another form—that is, the fragmentary condition of the shells is considered to be a proof that a glacier has passed over and crushed them. The difficulty of meeting this objection is that we do not know what deep-sea beds under glacial conditions would be like. There is a physical element that is entirely overlooked, and that is the action of the tides. I have shown[8] that the whole body of water is moved down to the greatest depths by the tides, and not merely the surface.

Most of the glacial beds that I have seen bear the marks of this disturbance, in the form of current bedding, especially the sands. The sands are intercalated in the clays, and it is difficult to separate the one from the other. The Boulder Clays, according to my interpretation, bear the marks of current action also in the smallness and rounded nature of the shell fragments, the roundness of the grains of sand and their high polish, and the manner in which sand-beds occur therein. The proportion of sand to clay is generally very considerable, the small gravel is also often highly polished and rounded. Numerous examinations of low-level Boulder Clays by washing, sifting, and separation of the grains, impresses me very strongly with the enormous wear which every constituent particle of the Boulder Clay has undergone, something totally different to what is seen in sand washed out of a living glacier which is uniformly angular. It is, therefore, quite probable that such deep-sea beds as are asked for do not occur in tidal seas, or if they do, only in exceptional positions. It is true the dredge brings up live shells from the sea-bottom now, but it is a surface gleaning, and the shell fragments are usually much more numerous than the living examples. Molluscs dying on the bottom of a tidal sea would not lie undisturbed, they would be rolled about with each tide, so that it is quite possible that undisturbed beds may not be accumulated.

[8] Tidal action a geological cause. *Proc. L'pool. Geol. Soc.* Session 1873–4; also *Phil. Mag.*, 1888, vol. xxv., pp. 338–343.

These considerations may not be deemed conclusive, but neither is the objection. We want more knowledge to speak positively either way. Also we have not examined the *whole* of the Drift yet, so that it contains many unknown possibilities.

6.—*A Subsidence of the land to the required depth is an improbability.*

I confess the attitude of mind which this statement discloses is one I have a difficulty in comprehending. There is no known fact in physical geology on surer foundations than that the land in all known continents and islands has undergone considerable fluctuation of level. Some land glacialists admit it as regards elevation and even invoke former continental elevation as *the cause* of the Glacial Period. They are unwilling to admit subsidence, but the clearest evidence up to 5,000 feet is to be obtained in North America and to 1,000 feet in Greenland,[9] and examples could be quoted from well nigh the whole world. I am not speaking now of aught but post-Tertiary subsidence. Our own coasts in the presence of buried river channels far below low-water bear the marks of subsidence. If we are prepared to dispute their evidence it will be necessary to recommence the study of geology on a new basis.[10] There is no evidence of any kind to indicate that this sort of elevation and subsidence takes place by or is accompanied by faulting, but plenty to show that it is by the bending of the earth's crust and differential vertical movement shading off to nothing.

Conclusions.

Having now to the best of my ability fairly stated the case against submergence as an explanation of the marine glacial beds, it will be well to consider the general nature of the objections. A very little examination will show that the criticisms are largely based upon negative evidence, which may become invalid at any moment by further discovery.

The allegation of the extremely partial nature of the high-level shelly gravels is, I have shown, not borne out by facts, and every additional discovery of shelly drift weakens the argument. That no perfect shells, or shells with the two valves in apposition, were to be found in the Boulder Clay is an assertion that is already disproved, and more examples in the same direction may be forthcoming.

That the erratics, including in this term all foreign rocks whether occurring as pebbles or boulders, are not found north of their origin, is another objection which has not been proved. The absence of deep-sea beds such as the land glacialists demand, is a further statement of the same sort that it is impossible to prove. There remain, then,

[9] De Rance, *Glacialists' Magazine*, August, 1893, p. 6.

[10] Since this was written, Dr. Geo. Dawson (*Proc. Geol. Soc.*, Nov. 8) has described the finding of Mammoth remains in the Pribilov Islands and Alaska, indicating a former land-connection of the North American Continent with Asia, and therefore a subsidence either during or since the Glacial Period.

only two arguments to answer, namely, the occurrence of boulders at a higher level than the parent rocks, and the supposed improbability of a subsidence of the land to the extent required by the submergence theory. The former I have shown is explicable by ordinary sea action assisted by shore-ice, and the latter is no improbability but a difficulty born of unfamiliarity with ordinary geological phenomena.

It is now time to turn our attention to the difficulties presented by the alternative hypothesis—that of the Irish-Sea glacier—and here, indeed, we are at a loss where to begin, they are so many and various. A huge machinery of ice-sheets is called in to explain the carrying up of boulders above their origin, though no examples of the power of ice-sheets to do this are given. The crushing of the shells is referred to the same agent, though many examples are known of glaciers passing over soft beds without disturbing them. At the same time, delicate shells found on elevated lands far inland have to be accounted for, and it is suggested that they have been safely conveyed frozen in the ice-sole of the Irish-Sea glacier which carried them over hill and dale to be washed out and deposited on the final melting of the ice. The rounding of the gravel and boulders and their deposit on the hill-tops, it is said, took place in a somewhat similar manner; but, as usual, no existing examples of such action are referred to, nor is it explained why the deposits on hill-tops should be more water-worn than those on the plain. To account for the distribution of the erratics, either several glacial and interglacial periods are required, of which there is no record, or a most extraordinary set of currents of ice and undercurrents are postulated; everything, in fact, except vortex movement. Yet, with all this complicated machinery, none have been able, in theory, to satisfactorily get the *stones on one side of the Irish-Sea glacier carried to the other side.* Finally, the physics of the ice-sheet are not subjected to a quantitative test.

When we call up before our mental vision the simple and well-known facts of nature which suffice to explain the marine drifts on the theory of submergence, it seems unnecessary to resort to the ingenious and artificial system of physics elaborated to explain the phenomena by land-ice.

When we have more knowledge of the glaciers of the Arctic Regions, and facts, in place of ingenious suppositions, to base our reasoning upon, we may possibly have to revise all our glacial conceptions. In the meantime, the submergence theory of the origin of the high-level shelly gravels and sands seems to me by far the simpler of the two theories, and the most consistent with the facts and phenomena which the labours of a succession of enthusiastic geologists have made us acquainted with.

T. Mellard Reade.

II.

Some Facts of Telegony.

TELEGONY is a term recently proposed by Professor Weismann to denote the influence which a first sire is believed by many breeders to have on the offspring of the same mother by a second sire. In his second article on "The All-sufficiency of Natural Selection," which recently appeared in the *Contemporary Review* (1), and in his lately-published book "The Germ-Plasm," Professor Weismann states his belief that the evidence for this phenomenon is not sufficiently convincing to establish it as a fact. This, I venture to think, may possibly arise from an insufficient examination of the evidence. Telegony is, no doubt, as Mr. Romanes concludes, a rare occurrence; and hence it is quite possible for breeders of large experience never to have met with a case, as has happened, according to Professor Weismann, with Settegast, Nathusius, and Kühn, in Germany. Mr. Tegetmeier also, our greatest authority on poultry, recently stated in the *Field* that, with fowls, the influence of the last sire is prepotent. Nevertheless, the well-known physiological variability of animals, which is as marked as their differences of form and colour, must surely be taken into account; and I hereupon proceed to give some cases which seem difficult of explanation on any other than the Telegonic theory. And first, with regard to horses, I should not mention Lord Morton's celebrated mare, as the case has been so often quoted, were it not for the fact that Professor Weismann quotes Settegast to the effect that in the drawings by Agasse of the striped colts borne by the mare to the black Arab after the quagga-hybrid, which are preserved in the Museum of the Royal College of Surgeons, no resemblance to the quagga is perceptible beyond the stripes.

Darwin (2), who mentions that these colts had also stiff, quagga-like manes, does not, according to Professor Weismann, seem to have known of these drawings; but Dr. Alexander Harvey (3), whose very interesting pamphlet "On a Curious Effect of Cross Breeding" is referred to by Darwin, quotes therein McGillivray (*Aberdeen Journal*, March 28, 1869), who mentions the case of a mare belonging to Sir Gore Ouseley, which, after bearing a hybrid to a *Zebra*, produced to horses two striped foals, whose portraits and skins are said to be in the Museum above mentioned. Since Dr. Harvey also mentions Lord Morton's mare, it would seem that Settegast, and not he, has confused two distinct cases here. At any rate, it would be well that these pictures should be re-examined, and their history clearly stated.

In this same pamphlet McGillivray is the authority for two other curious cases, both occurring in pure horses. In several foals, in the Royal stud at Hampton Court, the progeny of the horse Actæon, there were unquestionable marks of the horse Colonel, to whom the dams of these foals had been put the previous year. Also a colt, the property of the Earl of Suffield, the son of the horse Laurel, so resembled another horse, Camel, "that it was whispered, nay, even asserted, at Newmarket, that he must have been got by Camel." It was found, however, that all that connected them was that the colt's dam had been put to Camel the previous year.

I do not think that the case of the onager's foal at the Zoological Gardens, recorded by Mr. Tegetmeier in the *Field*, in December last, is so unmistakable an evidence of Telegony as Dr. Romanes (4) appears to think. The onager was put to an Abyssinian wild ass, produced a hybrid, and then bore to a male onager a chestnut foal with a white blaze on the forehead; but as this foal thus resembled neither parent, and in fact exhibited a horse's rather than an ass's marking, the case is surely one of Analogous Variation.

With respect to the case of Carneri's cattle, alluded to on page 385 of "The Germ-Plasm," it is noticeable that Carneri, who, since he kept a herd, had presumably some experience of cattle-breeding, evidently considered the germ-infection theory more reasonable than that of reversion to a previous cross. Professor Wallace (5), in his book on the farm live-stock of Great Britain, cautions breeders against putting a mongrel bull to good cows, as well on this account as for general reasons against keeping bad stock; and Mr. Bourne assures me that this is a matter of practical consideration with cattle breeders.

In sheep, besides the interesting instance given by Darwin of some Merino ewes, which after being put to a Merino ram with neck-lappets, bore lambs with this abnormal character to other sires, there is a very remarkable case given by Dr. Harvey, on the authority of Mr. W. M'Combie, of Tilliefour, Aberdeenshire:—

Six very superior pure-bred black-faced horned ewes, the property of Mr. Harry Shaw, in the parish of Lochiel-Cushnie, in Aberdeenshire, were put, in the autumn of 1844, some to a Leicester (white-faced and polled), and others to a Southdown (dun-faced and polled) ram, and produced cross-bred lambs. In the autumn of 1845 the same ewes were put to a very fine pure black-faced horned ram (*i.e.*, of their own breed). The lambs were *all* polled and brownish in the face, much to Mr. Shaw's astonishment. In autumn, 1846 the ewes were again put to another very superior ram of their own breed. Again the lambs were mongrels, not showing so much of the alien breeds as those of the year before; but two were polled and one dun-faced, with very small horns, while the other three were white-faced, with small round horns only. Mr. Shaw at length parted from those fine ewes, without obtaining from them one pure-bred lamb.

In the case quoted by Darwin of a sow of Western's black-and-white breed, which, after bearing to a chestnut wild boar a litter of cross-bred pigs, produced long after his death, to a boar of her own breed, pigs showing chestnut markings—though this might be put down to reversion, it is noteworthy that Harvey, who mentions this case, adds that on a *subsequent* impregnation, still by a boar of her own breed, she yet produced pigs, some of which were *slightly* marked with chestnut.

Youatt also says (6): "The boar to whom the sow has her first litter of pigs has a considerable influence on future litters, especially if of a very pure breed. In one instance a black sow was put to a white boar, and afterwards continuously to a black boar for three litters, yet in all these three litters there were white or black-and-white pigs."

In the "Book of the Rabbit" (7), Mr. Edward McKay, I believe a well-known breeder, is quoted as saying, "The influence of a first sire sometimes extends for generations, *i.e.*, influence of previous sires over offspring by other sires out of the same dam. I have known curious cases, which I cannot help thinking are the result of the above; for instance, a silver-grey doe was put to a Himalayan buck, afterwards to a Dutch sire, and afterwards to a buck of her own class, and in every succeeding litter, for several generations, were youngsters of all the above kinds. This may seem improbable to some, but, in my mind, no doubt exists."

In the case of dogs, Herr Lang, of Stuttgart (quoted by Professor Weismann), and Dr. Romanes have only got negative results, and in response to an appeal by Mr. Tegetmeier for facts on this subject, Mr. W. Godwin (8), of Market Drayton, in the *Field*, of Oct. 14, 1893, remarks that in numberless cases he has failed to notice any influence of one sire on succeeding offspring by another. However, he gives some instances of this, for which he can vouch, both with dogs and fowls. Those relating to dogs are as follows:—

A spaniel bitch littered to a terrier; all were destroyed, and in the next litter, to a spaniel, one was more like a terrier than a spaniel, the rest being true spaniels. An Irish terrier bitch, mated with a fox terrier, and bore her next litter to a well-bred Irish terrier; most of the last were like, apparently, pure Irish, two or three were red and white, one was all white, except a brindle red mark on the head and another on the stern, and strongly like a fox-terrier, except in size. A cocker-spaniel bitch, when being taken to a curly black spaniel, escaped and paired with a pointer, but was re-captured and paired to the spaniel in a very short time. Yet the resulting litter were half-bred pointers, save one, which was undoubtedly got by the spaniel; but this in form and habits showed traces of pointer blood.

With birds, the conditions differ widely from those present in the case of mammals, since in the former class there is no possibility of the infection of the maternal blood by a cross-bred fœtus; yet there is evidence of the telegonic phenomenon in them also. Mr. Lewis Wright (9) is so convinced of its occurrence that he says, "We would never on any account allow a male bird of any strange breed to enter, even for a day in winter, a yard of hens which we greatly valued." He gives some instances, from which I select the following:—

A Mr. Payne, in England, had two Spanish pullets running with both a Spanish and Cochin cock. After they began to lay the Cochin was removed, and *six weeks* after the eggs were saved and set, but the chickens were feather-legged, in all other points resembling the Spanish.

In America, a breeder of game finding a neighbour's feather-legged bantam cock came over his fence, penned in his fowls securely, and saved no eggs for a month after, but several chicks still had feathered legs, though with no other sign of the cross. Dr. Harvey, who favoured the theory of maternal infection by the fœtus, states, as proof that telegony, as would be expected on this view, does not occur in birds, that he has been assured that crossing with a bantam during one season does not affect the progeny of a common hen during the next.

But Mr. Godwin, quoted above, also gives some instances of the working of this principle with fowls. A Dorking hen (5-toed, with bare white legs), after running with a Dorking cock, was put to a dark Brahma (4-toed, with feathered yellow legs.) The eggs on the two following days produced pure Dorkings; the egg on the third or fourth day a Dorking with three or four feathers on one leg; the next egg a feather-legged Dorking with four toes only on one foot, both legs being white; the next egg, and all after, yellow-legged birds, with a lot of feathers, as if the influence of the sire were progressive. This case came to his knowledge some thirty years back.

In Mr. S. Fielding's yard, at Trentham, Mr. Godwin saw some young fowls of game character, yet with a decidedly mongrel look. They were the offspring of a Silver Hamburg pullet, which had been running with a game cock, and three weeks had been allowed to elapse after her removal from him before any eggs were set; yet this was the result.

It is obvious that some of the cases above given can hardly be set down to reversion, feather-legged fowls and polled sheep not being ancestral types; nor is previous crossing a likely explanation. Before concluding, I may mention a remarkable fact stated in Professor Newton's new "Dictionary of Birds" (10); according to this, Nathusius came to the conclusion that by microscopical examination, which reveals differences between eggs of birds of various species, the egg of a bird paired with an alien male may be distinguished from one laid by the same bird with a mate of her own species. And Mr. A. G. Butler (11) affirms that a canary in his possession laid eggs coloured and marked like those of a chaffinch, when paired with a male of that species. Experiments are much needed on these points; and since we fortunately have in domestication birds nearly allied, yet perfectly distinct in species, such as the Muscovy and common ducks, Chinese and common geese, Guinea fowl and common fowl, and collared turtle-dove and pigeon, it is to be hoped that someone will be sufficiently energetic to experiment with some or all of these; hence, he may thus help on the solution of more than one debated question interesting to scientific men in general, for the same series of experiments might be made to throw light on Hybridism as well as on the subject of this paper.

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Frank Finn.

III.

Further Notes upon Arachnid and Insect Development.

IN an article in NATURAL SCIENCE (June, 1893) I gave a summary of some recent memoirs upon the classification of arachnids Mr. Goodrich has since (NATURAL SCIENCE, September, 1893) kindly pointed out that the views of Mr. Pocock on the relationships of the arachnid orders, therein set forth, agree in the main with those put forward twelve years ago by Professor Ray Lankester (1), in his memoir advocating the arachnid affinities of the King-Crab.

Ever since the appearance of that well-known paper by Professor Lankester, *Limulus* has been an animal of exceptional interest to zoologists. The recent publication by Mr. Kingsley (2) of a detailed description of the embryology of the King-Crab is therefore worthy of special notice. His results, on the whole, strongly support the arachnid affinities of *Limulus*; its development is shown to have but little correspondence with that of Crustaceans.

The structure of the ovary resembles that of scorpions, spiders, and mites. The ova are developed from the columnar epithelium of the ovarian tubes, and pass into a space between the epithelium and the tunica, which, at first, are in contact with each other. The segmentation of the egg proceeds by delamination, similar to that observed in various spiders, harvestmen, and mites. The mesoderm arises by proliferation from the thickened region of the blastoderm, and cœlomic cavities are formed in the somites by the splitting of the mesoderm into two sheets. The body-cavity of the adult is, however, due to secondary spaces in the mesodermal tissues. Each cœlomic cavity becomes divided into a dorsal and a ventral part; the former is believed to give rise to the generative glands; the latter is lost in most of the somites, but in the fifth it develops into the coxal gland (nephridium). This corresponds with the fate of the cœlome and the origin of nephridia in *Peripatus* and in arthropods generally;[1] it specially recalls the origin of the coxal glands in arachnids.[2] The formation of the alimentary tract agrees with what occurs in arachnids, and contrasts with the corresponding process in crustaceans, for, while the stomodæum is long, the proctodæum is short, and late in its appearance. Mr. Kingsley states that his results are, in the main, confirmed by the researches of Mr. Kishinouye (3) upon the development of *Limulus longispina*.

[1] NAT. SCIENCE, vol. i., p. 282. [2] *Ibid.*, pp. 524, 525.

In giving an account of the development of the gill-books of *Limulus*, Mr. Kingsley refers to Mr. Laurie's views on the relationship of these organs to the lung-book of arachnids,[3] and ingeniously shows that the reversal of the faces of the appendage suggested by Mr. Laurie is not necessary. From the embryonic appendage with invagination behind it (Fig. 1), can be derived the gills of *Limulus* (Fig. 2) or the lung of an arachnid (Fig. 3), the folds in the latter case, though on the forward side of the invagination, really corresponding with the hinder surface of the primitive appendage.

No details of the nervous system are given by Mr. Kingsley, but its development and adult structure have been lately described

Fig. I.

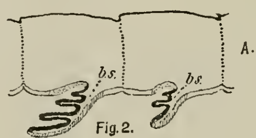


Fig. 2.

Fig 3.

FIGS. 1–3.—Diagrams of (1) Primitive Appendages, which develop into (2) Gills of *Limulus*, or (3) Lungs of Arachnid. A—Anterior aspect of animal; *b.s.*—Blood-spaces, which are seen to correspond in all three figures. [After Kingsley.]

by Dr. Patten (4), who shows the correspondence of the embryonic brain of *Limulus* with that of scorpions, spiders, and insects. He describes a pair of cephalic lobes with three divisions, from which are developed the large "cerebral hemispheres" of the adult. To the ganglia which innervate the cheliceræ, the term "mid-brain" is applied; the five next pairs are called the "hind-brain," and the next pair the "accessory brain." These terms indicate the homologies which Dr. Patten sees between the brain of *Limulus* and that of the Vertebrata, claiming that in the nervous system of the King-Crab fresh support is to be found for his startling theory of the arachnid ancestry of the back-boned animals. The front wall of the cephalic lobes passes downwards into the cavities of a pair of semicircular lobes, identified by Dr. Patten with the infundibulum of the vertebrate brain. From these arise the roots of the nerve of the median eyes, which are compared to the pineal eye of vertebrates. Correspondences are also found between the olfactory organs of *Limulus* and of vertebrates, specially lampreys. Dr. Patten, indeed, states that the nervous

[3] NAT. SCI., vol. i., p. 524.

system of the adult King-Crab is remarkably distinct in appearance from that of any other arthropod, and "shows a profound structural similarity to that of the vertebrates." In order to see this, one must "tear off the deceptive arthropod mask that disguises *Limulus*." The true value of Dr. Patten's homologies can only be estimated by students of the vertebrate brain. But more evidence will be required to convince most naturalists that the many profound structural distinctions between arthropods and vertebrates do not point to a wide divergence of origin. In all arthropods the œsophagus pierces the nervous system. Dr. Patten suggests that in the primitive arthropod ancestor of vertebrates the œsophagus "broke through the narrow band of nerve-tissue in front of it." But, unfortunately, in *Limulus*, which he regards as the nearest living representative of this ancestor, the nerve-tissue in front of the œsophagus is not a narrow band, but forms the great cephalic lobes; and so, if this explanation is to be accepted, the correspondences between these lobes in *Limulus* and the vertebrates must be homoplastic, and not homologous.

Dr. Patten gives interesting details of the structure of the sense-organs of *Limulus*. Sensory spines on the joints of the legs are covered with tubular openings, each containing a chitinous tubule in connection with a single elongate nerve-cell. These have a tasting function. In the organ of smell are found clusters of large cells surrounding a multipolar ganglion-cell, connected with a chitinous tube ending near the outer surface.

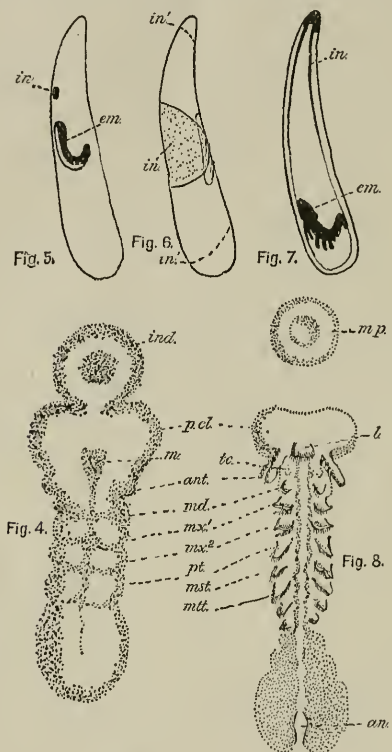
At the end of his paper, Mr. Kingsley reviews the question of the systematic position of *Limulus*. In a tabulated statement, he instances but six points in which its development and adult structure agree with those of the crustaceans, while the points of agreement with the arachnids number twenty-eight. He joins *Limulus* with the Eurypterids as a sub-class Gigantostraca forming, with the sub-class Arachnida, the class Acerata. With such strong confirmation of its arachnid affinities, most naturalists will prefer to call *Limulus* simply an arachnid. The association of the Eurypterids with the same class is justified by the researches recently summarised for us by Mr. Laurie.[4] But the Trilobites, for some time past considered as related to these forms, must probably be restored to the Crustacea since the discovery of their antennæ.[5]

Mr. Kingsley proceeds to the discussion of the classification of the Arthropoda generally. The Acerata and Crustacea form a sub-phylum Branchiata. The sub-phylum Insecta includes classes Hexapoda and Chilopoda while the Diplopoda form a separate sub-phylum. This view, that the centipedes are more nearly related to the true insects than to the millipedes, and that the Myriapoda must therefore be considered an unnatural group, has been put forward before, but has lately been elaborated also by Mr. Pocock (5) who

[4] Nat. Sci., vol. iii., p. 124. [5] Nat. Sci., vol. iii., p. 162.

lays special stress upon the position of the genital opening. The Diplopoda and Pauropoda (in which this opening occurs in the anterior region of the body) are classed by him as Progoneata, in opposition to the Chilopoda, Symphyla,[6] and Hexapoda, in which the generative duct opens at the hinder end of the animal, and which are accordingly called Opisthogoneata.

Mr. Kingsley reviews the main points of structure of the Diplopods,



Figs. 5–7.—Embryos of *Xiphidium*. *em.* embryo. *in.* indusium. *in'.* furthest extension of indusium. Fig. 4.—Young embryo of *Xiphidium*. Fig. 8.—Embryo of *Anurida*. *ind.* indusium. *mp.* micropyle. *p.cl.* procephalic lobes. *m.* mouth. *l.* labrum. *ant.* antennæ. *tc.* tritocerebral appendage. *md.* mandibles. mx^1. mx^2. maxillæ 1 and 2. *pt.*, *mst.*, *mtt.* thoracic segments. *an.* anus. [After Wheeler.]

Chilopods, and true Insects, and finds further support for the association of the two latter groups in opposition to the former, in the correspondence of the pairs of jaws in centipedes and insects, and the coalescence of the somites in pairs in the millipedes. The systematic position of the Pauropoda, Pycnogonida, Trilobites, Tardigrada, and Malacopoda (*Peripatus*) is considered uncertain by

[6] According to some observers, however, the genital opening in the Symphyla (Scolopendrella) is situated in the fourth body-segment. Nevertheless, these animals must be nearly related both to the Chilopoda and the Thysanura.

Mr. Kingsley. Indeed, he considers it doubtful if the latter animals should be regarded as arthropods, pointing out that their serial nephridia, muscular pharynx, generally unstriated muscle-fibre, eyes, and pre-oral antennæ point to their relationship with the Chætopod worms. However, their reduced cœlome, heart and pericardium, tracheæ, and jaws will probably induce most naturalists to retain them as primitive arthropods.

While Mr. Kingsley has thus increased our knowledge of the development of *Limulus*, another American investigator, Dr. W. M. Wheeler, has (6) made a valuable contribution to the embryology of insects. His researches have been principally carried out upon a species of *Xiphidium*, an orthopterous genus belonging to the family of the long-horned grasshoppers (Phasgonuridæ).[7] The females of these insects lay their eggs in the galls produced on willows by midges (*Cecidomyia*). The eggs of a grasshopper of a nearly related genus (*Orchelimum*), a cricket (*Gryllus*), a "praying-insect" (*Stagmomantis*), and a marine collembolan (*Anurida*) were also studied.

The process of gastrulation is described, and confirms Cholodkowsky's researches on the cockroach and Graber's on *Stenobothrus* in showing that this process in the Orthoptera corresponds more closely than had been supposed with what occurs in the higher groups of insects. A structure of great interest observed by Dr. Wheeler in the development of the long-horned grasshoppers, is the indusium. This organ, which seems not to have been before noticed in the embryo of any winged insect, arises as a circular thickening of the blastoderm in front of the head (Fig. 4). While the embryo moves through the yolk, from the ventral to the dorsal aspect of the egg (Fig. 5) the indusium increases in size until it has assumed a saddle-shaped form, covering the greater part of the egg's surface (Fig. 6). A layer, corresponding to the amnion of the embryo, is formed within the serosa, over the indusium. Before hatching, the embryo in its growth comes again to the original ventral aspect of the egg (Fig. 7) absorbing the yolk, while the indusium becomes reduced to a cap of cells at the apex of the egg. Dr. Wheeler considers the indusium in these grasshoppers to represent the micropyle observed in embryos of the Poduridæ, and he gives a figure of the embryo of *Anurida* (Fig. 8) to confirm this view. He would also compare the structure with the dorsal organ of Crustacea, the primitive cumulus of spider-embryos, and the embryonic sucking-disk of certain leeches.

The antennæ of the insect embryos examined are stated by Dr. Wheeler to arise behind the mouth, and are so figured (Figs. 4, 8) thus confirming Cholodkowsky's observations upon the cockroach.[8] They are innervated from the deuto-cerebrum or first post-oral nerve-mass. The segment of the trito-cerebrum (the ganglion next behind) bears a pair of evanescent appendages in the embryo of *Anurida* (Figs. 8, *tc*.). With regard to the hindermost appendages of the

[7] Locustidæ, auct.

[8] Nat. Sci., vol. i., p. 281.

body, Dr. Wheeler brings forward evidence to support the accepted view that they are utilised in connection with the reproductive opening.

Dr. Wheeler's observations upon the origin of the germ-cells confirm, on the whole, the results of Dr. Heymons with *Phyllodromia*,[9] except that doubt is thrown upon their very early differentiation as described by the latter observer. They have now been noticed in all but two of the abdominal segments of embryo insects, recalling very strongly the condition in worms. Dr. Wheeler's researches on the origin of the genital ducts confirm the view that they are modified nephridia. A peculiar mass of cells beneath the œsophagus, developed from the mesoderm of the trito-cerebral somite, is thought by Dr. Wheeler to be a vestigial nephridium, and, accepting the homology of this segment with the antennal segment of the Decapod Crustacea, to represent the green gland of a crayfish. This homology of the segments will bring the antennæ of insects into line with the crustacean antennule, and make the mandibles in Crustacea, Arachnida, and Insecta correspond. Dr. Patten, however, insists (4) that no segments have been suppressed in either insects or arachnids, and that the antennæ of the former correspond with the cheliceræ of the latter; but it seems impossible that all the recent describers of vestigial appendages can be mistaken.

With these contributions to the embryology of insects, it is well to record some recent memoirs upon their development after hatching. Professor Miall (7) has discovered the larva and pupa of a genus of Crane-fly (*Dicranota*) whose metamorphosis had been previously unknown. The grubs of the "Daddy-long-legs" (*Tipula*), belonging to the same family, are only too well-known as the "leather-jackets," which often devastate the roots of corn and grass crops. The larva of *Dicranota*, however, is a flesh-feeder; it lives in gravel and mud at the bottom of streams, and preys upon the well-known red worm, *Tubifex*. The head of the larva is small, and can be retracted within the thorax. The third to seventh abdominal segments bear pairs of fleshy legs with circles of hooks at the end, recalling the claspers of moth-caterpillars; the ninth (hindmost) segment carries three pairs of appendages, of which the posterior are long, and contain tracheæ. When the head is retracted, the brain, with the subœsophageal and prothoracic ganglia, is found in the mesothoracic segment. The alimentary canal shows adaptation to the carnivorous habit, being straight, and wanting the diverticula found in the grubs of *Tipula*. As in tipulid larvæ generally, there is but one pair of spiracles, situated on the back of the hindmost segment, which is also provided with tracheal gills, so that the grub can breathe by thrusting its tail either into air or water. A well-developed tracheal system provides for the storage of oxygen. The male generative organs develop very early, and ripe spermatozoa were observed in a larva not full-grown.

The pupa lives in damp earth, and breathes through a pair of

[9] NAT. SCI., vol. i., pp. 54, 55.

"respiratory trumpets" on the prothorax. The air is admitted to these through a series of small oval apertures, fine enough to keep out water or dirt, if they are not closed (as may, perhaps, be the case) by a transparent membrane. The abdominal segments bear dorsal plates with spines, by means of which the pupa makes its way out of the ground, before the fly emerges; ventral pro-legs, corresponding to those found in the larva, assist in this function.

The structure of the stigmata of the grub of the Common Cockchafer, as described by Herr Boas (8), recall the arrangement of the respiratory trumpet of the *Dicranota* pupa. The ordinary opening of an insect stigma is found to be closed in this larva, and the insect takes in air through minute pores in the "sieve-plate" surrounding the normal aperture. In both cases we see the adaptation to an underground life.

The highly interesting pupa of a minute moth (*Micropteryx*) has been lately described by Dr. Chapman (9). This pupa lives in a very strong cocoon, several inches underground. It is remarkable among lepidopterous pupæ in possessing a well-developed labrum, and mandibles which Dr. Chapman (allowing for the relative sizes of the insects) compares to those of a stag-beetle! As is well-known, these organs are usually reduced to the merest vestiges in moths and their pupæ. In the present case, the obvious use of these powerful jaws is to effect the escape of the pupa from its buried cocoon, before the emergence of the moth. The arrangement recalls what occurs in the Caddis-flies, and is another strong argument in favour of the primitive position of the Micropterygidæ among Lepidoptera, the wing-neuration having already led to the acknowledgment of their affinities with the Trichoptera. Yet another fact which tells the same tale is that the pupa seems entirely incomplete (the limbs are not soldered to the body) and the moth escapes through a simple dorsal slit—an arrangement hitherto unknown among moths according to Dr. Chapman.

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GEO. H. CARPENTER.

IV.

Note on the Evolution of the Scales of Fishes.

MUCH has been written by those who have studied the minute structure of fish-scales, concerning their primitive nature and mode of origin. It is generally admitted that the earliest hard parts appearing in the skin of fishes, were merely isolated calcified points or papillæ, such as those in which the tubercles of sharks and skates arise; and that all the later types of scales and plates are only the result of the fusion and elaboration of these fundamental structures. Palæontology, too, specially favours this idea; and an elaborate memoir just published by Dr. Rohon (3) shows more clearly than ever how large a majority of the fragments of dermal armour of chordate animals in the Upper Silurian rocks are almost identical in structure with the comparatively simple skin tubercles of the sharks and skates.

There is, however, another aspect of the subject which seems to have been less studied, and to which it is now possible to make one more small contribution. We refer to the question of the original character and subsequent phases of evolution of the typical overlapping scales of the trunk of ordinary fishes. Professor Ryder has inferred (4), from theoretical considerations, that these scales must have been originally disposed round the body in a series of rings corresponding to the successive plates of muscle; and he attempts to show, by an argument from mechanics, that they must have been at first rhombic in shape, in consequence of the direction of the strains arising from the working of the muscles during motion. It is obvious that the only proof of the theory can be obtained from Palæontology; and we propose in this brief note to summarise a few new or little-known facts bearing upon the subject.

Perhaps the simplest form of squamation met with in any fish-like organism is that of the Lower Palæozoic *Cephalaspis* and its allies, in which (as Professor Lankester first remarked) each segment of muscle seems to be surrounded by a distinct ring of six or seven scales; this ring being overlapped by the one immediately in front and overlapping that next behind (2, 5). Towards the extremity of the tail in *Cephalaspis*, however, where the motion is much greater than in the rest of the trunk, each of these rings is subdivided into a set of comparatively small rhomboidal scales—an arrangement decidedly suggestive of the truth of Professor Ryder's theory of the effect of mechanical strains.

Among undoubted fishes it is not easy to discover any facts particularly apposite to the question under consideration in the groups of Dipnoi and Fringe-finned "Ganoids"; but it seems possible to turn with more satisfaction to the higher "Ganoids" of the sub-class Teleostomi. Here it is quite evident, in a general way, that as the fishes become higher in organisation—become specialised—the scales are more deeply overlapping, thinner, and almost or regularly cycloidal. We can now even quote instances in which the anterior part of the trunk is covered by typical quadrangular ganoid scales, united by a peg-and-socket articulation; while the base of the tail of the same fish is enveloped by deeply-overlapping thin rounded scales, which would be typically "cycloid" were it not for the presence of a few remnants of superficial enamel.

Such an instance is a deep-bodied fish related to the European Liassic genus *Dapedius*, obtained from the Hawkesbury Formation of New South Wales and shortly to be described under the name of *Aetheolepis*. Typical examples of its scales are shown in the accompanying figures. Those of the anterior series, as shown by the

Scales of *Aetheolepis* from Hawkesbury Formation, New South Wales.

drawing to the left, are thick and rhombic with well-developed peg-and-socket articulation; while on the caudal region all the scales are very thin and much overlapping, and they gradually degenerate backwards in the manner here indicated. The specimens are not sufficiently well-preserved to admit of the making of microscopic sections; but it is probable that the hindermost scales are destitute of bony tissue, and the traces of enamel are merely a few isolated tubercles. *Aetheolepis*, in fact, so far as its scales are concerned, belongs in its anterior half to the "order Ganoidei" of Agassiz, while posteriorly it almost exactly enters the "order Cycloidei" of the same author.

There is a tendency to the same kind of scale-arrangement also in a fish from the English Lias, apparently of the genus *Endactis;* and from both instances it may probably be inferred that the cycloidal scales have resulted from the modification of the thick quadrangular scales by the mechanical conditions in the comparatively mobile caudal region where they are found.

It is well known that even in ordinary ganoid fishes with rhombic scales, their peg-and-socket union is almost always wanting on the tail, as if to ensure greater freedom of motion. So far as the present writer is aware, however, no case has hitherto been noticed in which firmly-united quadrangular scales are fixed at their upper and lower borders by more than the peg and socket. It is, therefore, of much interest to record that the scales of the Pycnodont genus *Mesturus* (from the Bavarian Lithographic Stone and the English Oxford Clay) are joined together above and below by a deeply dentated, interlocking suture, exactly as observed in certain scales of crocodiles. *Mesturus*, indeed, must have had an almost inflexible scale-armour.

If further proof were required that rhombic scales in the Palæozoic fishes are more primitive than deeply overlapping cycloidal scales, we need only refer to the case of the Palæoniscidæ. In this group the fishes with cycloidal scales always retain the thick rhombic ones on the upper lobe of the tail, however much the aspect of the trunk-squamation itself may have changed (1). It is therefore curious to find that among the Devonian fringe-finned ganoids, those with the most primitive fins (Holoptychiidæ) have the scales round and deeply overlapping, while many of those with more specialised fins (Osteolepidæ) have the squamation rhombic. At the same time, it must be remembered that the rhombic-scaled *Megalichthys* of the Coal-measures gradually passes into the round-scaled *Rhizodopsis* of the same age, the inner rib of each scale then degenerating into a small tubercle; and some of the round scales of the Devonian fishes show this inner tubercle, which is likely to have arisen in the same manner.

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A. Smith Woodward.

V.

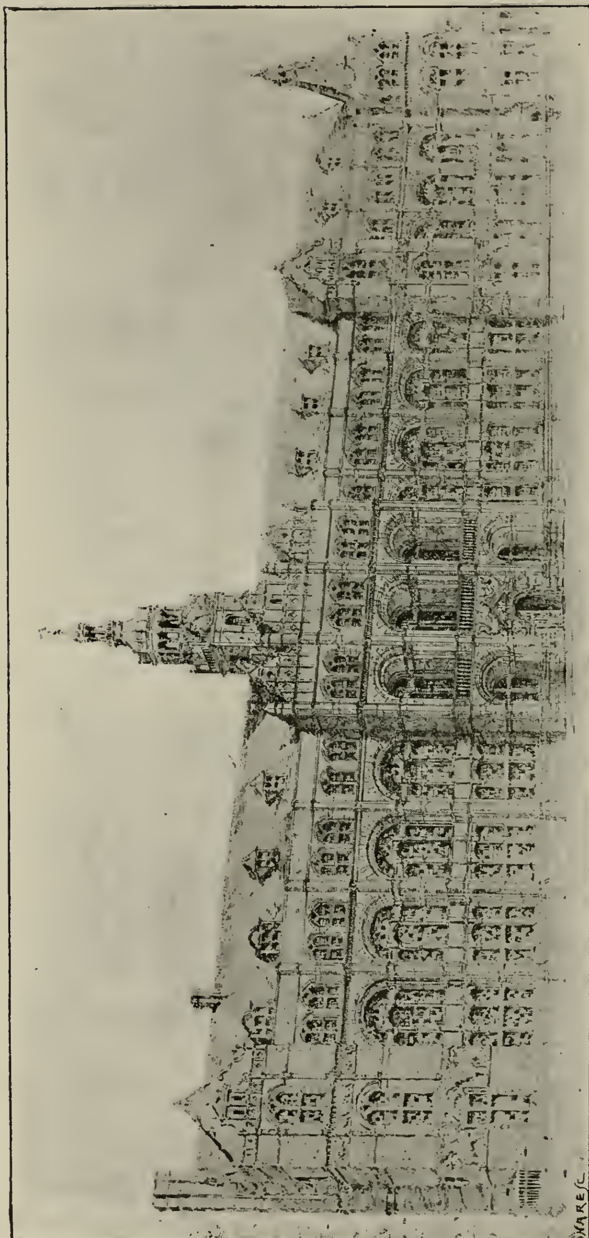
The Sedgwick Museum, Cambridge.

WHEN the question of a memorial to Professor Sedgwick came to be considered, it was generally agreed that nothing could be more appropriate than a Museum to contain the collections which had been so largely made during the fifty-five years of his Professorship, and which even then (1873) were too extensive for the building which they still occupy. For this purpose a sum of money, which now amounts to £23,000, was quickly subscribed.

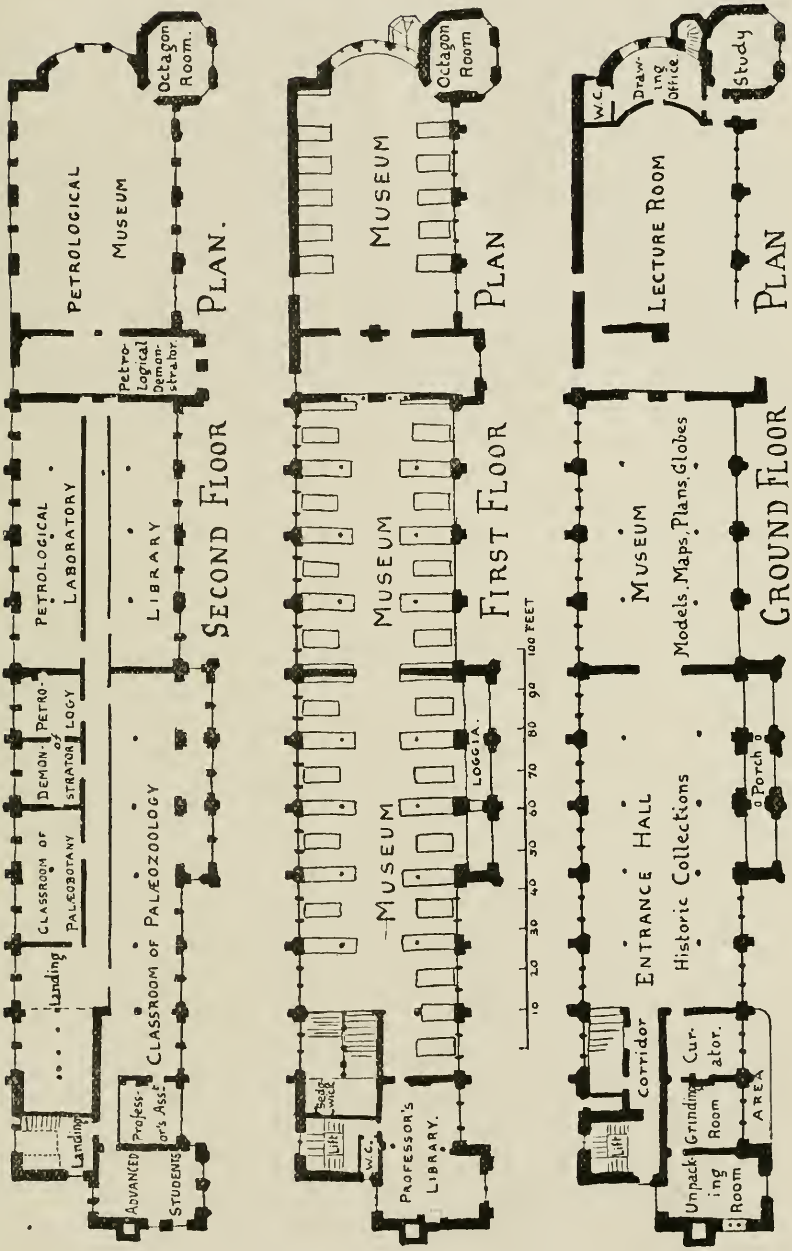
Several syndicates have been appointed by the University from time to time to carry out this object. But no real advance was made until 1891, when the site was fixed upon as that part of the Old Botanic Gardens which faces Downing and Pembroke Streets, and extends from Corn Exchange Street to near the Chemical Laboratory. This position will be most convenient both for teachers and students, since all the other departments of Science have their museums and laboratories close at hand. Another Syndicate was appointed on February 4, 1892, to prepare plans and obtain estimates for the building. The services of the eminent architect, Mr. T. G. Jackson, A.R.A., were obtained; and his plans, together with the report of the Syndicate, were presented to the University on November 22, 1892, and after some slight modifications were accepted by the Senate on May 4, 1893.

The perspective elevation and floor-plans are shown in the accompanying illustrations. The materials to be used in the construction are red brick and Clipsham stone (Lincolnshire Oolite). The length of the building will be 304 feet, and the breadth 44 feet (outside measurement); the main wall will be set back 10 feet from the street, and the projecting portions 4 feet. There will be three floors in addition to cellars and attics.

On the ground floor will be the entrance-hall, containing the original Woodwardian and other historic collections; to the right will be a room for models, maps, sections, etc., and beyond this a lecture-room ; to the left will be the Curator's room and other offices. At the north-west corner will be the principal staircase. The first floor will be devoted entirely to the Museum of Stratigraphical Palæontology, with the exception of a room at the west end for the Professor's library. This museum will be divided by upright cases,



The Proposed Sedgwick Museum, Cambridge.



Plans of Proposed Sedgwick Museum, Cambridge.

placed between the windows, into compartments, in each of which a table case will be placed. The third floor will be occupied by laboratories for Palæontology and Petrology, by private rooms for lecturers and demonstrators, and by the Museum of Petrology.

The two lower floors will be lighted by long windows reaching up to the second floor-level; the top floor will be lighted by smaller and more numerous windows.

The laboratories are placed on the second floor in order to secure the best light. This has made it necessary to place two rows of pillars in the lower floors for support; in the main museum they will pass through the middle of the upright cases, and will consequently be quite inconspicuous. At the head of the first flight of stairs there will be a statue of Professor Sedgwick. The eastern part of the site assigned to the Museum is at present occupied by the medical school, and as this cannot yet be accommodated elsewhere, it is proposed to build first a part of the Museum on the ground which is free.

Henry Woods.

VI.

Recent Explorations of the Maltese and Sicilian Caverns.

SOME interesting progress has recently been made in investigating the bone-caves of Malta and Sicily. It has long been a matter of common belief that these islands are the remnants of one of the old land barriers connecting Europe with Northern Africa during at least part of the Pliocene and Pleistocene periods; and a detailed study of the animal remains met with in the fissures and caverns is thus one of the foremost importance. Not only is it possible to recognise the mingling of northern and southern animals, and the apparent effect of isolation upon them before their complete extinction as the feeding-area became more and more reduced by subsidence; but it also seems likely that some idea of the nature of recent physical changes in the region in question can be obtained from a comparison of the sequence of deposits in the various localities examined. The work in Malta has been carried on by Mr. John H. Cooke, with the aid of a grant from the Royal Society of London (7); the new researches in Sicily are those of Dr. Hans Pohlig, based upon a large collection of bones in the Palermo Museum from the Cavern of Pontale, at Carini (10).

The bone-caves of Malta were discovered so long ago as the middle of the seventeenth century (1), and they have long been well-known through the explorations and researches of the late Rear-Admiral Spratt (11), Professor Leith Adams (2–5), Dr. Hugh Falconer (8), and Dr. George Busk (6). They have yielded some dwarf elephants, described under the names of *Elephas melitensis*, *E. falconeri*, and *E. mnaidriensis*, and a dwarf hippopotamus (*H. pentlandi*). They have also furnished evidence of a so-called gigantic dormouse (*Myoxus melitensis*), some large land-tortoises (3, 4), and various birds (9). Mr. Cooke's researches, therefore, are for the most part only an independent verification of results already obtained; but, at the same time, he has succeeded in making one or two striking additions to our knowledge of the extinct fauna in question. The principal specimens obtained have been placed in the British Museum, and a detailed report on these fossils by Mr. A. S. Woodward is appended to Mr. Cooke's account of his results.

Attention has been chiefly confined to one cavern in the Har

Dalam Gorge, near Marsa Scirocco Bay, where Spratt and Adams accomplished so much. It is situated 500 yards from the shore, and its mouth is now 40 ft. above the bed of the small stream which, in rainy weather, flows through the gorge. There is evidence everywhere of former torrential rains, where all is now comparatively parched; and Mr. Cooke considers that no other phenomenon could have filled the caves and fissures as he finds them. In most parts the Har Dalam Cave was filled to a height of within 2 ft. of the roof, and one of the principal sections of the contents showed the following succession of layers:—(i.) Unstratified surface *débris*, 6 in.; (ii.) Red clayey loam, 3 ft., with *Hippopotamus*, *Cervus*, and pottery; (iii.) Unfossiliferous black earth, 4 in.; (iv.) Dark red plastic clay, 1 ft. 6 in., with *Hippopotamus*; (v.) Reddish clay, 1 ft., with *Hippopotamus* and *Cervus*; (vi.) Unfossiliferous yellow plastic clay, 2 ft. In one place a human bone was found at about the base of layer iii.; and layer v. also yielded the first evidence of extinct carnivorous animals discovered in Malta—a portion of mandible with teeth of a bear, and one tooth of a canine quadruped as large as a wolf.

The discovery of these carnivora in the Maltese caverns is of great interest, because gnawed bones have already been noticed; but it is probable that still others remain to be found, notably the lion and hyæna. The jaw of the bear, moreover, cannot be specifically determined—cannot be definitely assigned either to any extinct animal, to the brown bear, the grisly, or to those of Northern Africa; can only be distinguished with certainty from our cave bear. As for the canine, a single tooth is insufficient to prove whether or not it belongs to a domestic dog.

Still more interesting is the discovery of a great number of bones of a small deer, mostly identified with the diminutive race of the common stag met with in Northern Africa and known as the Barbary Deer. The Fallow Deer may also have been present, but there are no characteristic fragments. One superficial layer in the Har Dalam cave consisted almost entirely of these remains in stalagmite, belonging to animals in all stages of growth, perhaps even from the unborn fœtus onwards. The adults vary much in size, but the largest complete antler measures only about 2 feet in length.

It is worthy of notice, in reference to Dr. Pohlig's recent memoir on the Cave of Carini, that the same form of dwarf deer is also now recognised in Sicily. Dr. Pohlig, however, gives a new sub-specific name to the animal, terming it *Cervus* (*elaphus*) *siciliæ*; and his nomenclature for the associated species is not altogether such as will commend itself to many zoologists.

The chief advance made by Dr. Pohlig, indeed, consists in his adding to the known Pleistocene fauna of the Sicilian area this small deer and the dwarf elephant commonly known as *Elephas melitensis*. Of the latter, even finer specimens have been discovered at Carini than those obtained from Malta; and Dr. Pohlig is now able to describe the whole skull for the first time. As the result, he is more than ever convinced of the correctness of his opinion, expressed some years ago, that all the dwarf Maltese elephants described by Falconer, Busk, and Adams are merely a stunted race of the typically European *Elephas antiquus.* He even goes further, and concludes that the latter elephant wandered in Pleistocene times as far south as India, being represented in the Narbada Valley by the so-called *E. namadicus.*

Besides the bones of animals in the new Sicilian cave, which are now described as including remains of such familiar European species as *Bos primigenius* and *Bison priscus*, there are also some traces of man, in the form of rude pottery and stone implements. There is, however, no very clear evidence as yet to indicate man's relationship to the extinct fauna ; and we refrain from quoting Dr. Pohlig's table of the succession of episodes supposed to be proved by the series of deposits he has examined. All these new facts will some day be of great service when the time for broad generalisations as to the recent changes of land and sea in the Mediterranean area is at hand; at present it seems futile to base speculations on isolated phenomena.

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SOME NEW BOOKS.

AUSTRALASIA. Vol. i., Australia and New Zealand. Stanford's Compendium of Geography and Travel (new issue). By Alfred Russel Wallace, LL.D., D.C.L. 8vo. Pp. xvi., 505, maps, and illustrations. London: Stanford, 1893. Price 15 shillings.

"IN the present volume an attempt has been made to give a compact description of our great Australian Colonies, which should be useful to intending visitors or emigrants; and which will also be interesting to the general reader who may wish to become acquainted with the natural features and social condition of the Britain of the Southern Hemisphere." With the above words the author prefaces his volume, noting also that he has treated of the history and geology, the physical characteristics, customs, languages, and probable origin of the aboriginal inhabitants. Further information is given as to the history of Australian exploration and the special characteristics and productions of the several colonies as well as their more important industries. Prefixed to the general work is a list of works used in preparing the volume, which, though arranged without any regard for alphabetical order or date, will prove useful to the reader if he wishes to pursue his subject further, for the book is essentially one of reference, although readable enough in itself.

Dr. Wallace opens with the general subject, dealing with the definition and nomenclature of Australasia, showing how the area covered by the word has gradually been restricted by geographers until, at present, it is held to comprise only Australia and New Zealand, together with the large islands as far north as New Guinea and the New Hebrides. He, however, holds it to include the islands of the Malay Archipelago as well, and prefers to retain the word Australasia, rather than Oceania, because the former indicates the true relation of the group to the great land-mass of which it forms a southern prolongation.

The geographical and physical features of Australasia are unique, and are thus sketched by Dr. Wallace. In the west, the Malay archipelago, comprising the largest islands in the world (Australia excepted), is unsurpassed for the luxuriance of its vegetation, as well as for the variety and beauty of its forms of animal life. Further to the east are the islands of the Pacific, remarkable for their numbers and their beauty; while to the south is Australia, as unique in its physical features as it is in its singular forms of vegetable and animal life. Still further south lies New Zealand, almost the antipodes of Britain, but possessing a milder climate, and a more varied physiography. Comprised thus between the northern tropic and the 40th degree of south latitude, this land-mass possesses as tropical a climate as Africa, while, owing to its being so completely oceanic, and to its wide exterior, it presents diversities of physical features and of organic life hardly to be found in any of the other divisions of the globe, Asia, perhaps, excepted.

The most striking contrasts of geological structure are exhibited by the coral islands of the Pacific, the active volcanoes of the Malay Archipelago, and the ancient rocks of New Zealand and Tasmania. The most opposite aspects of vegetation are presented by the luxuriant forests of Borneo and New Guinea and the waterless plains of Central Australia. In the Sunda Islands we have an abundance of all the higher and larger forms of mammalia; while farther to the east, in Australia and the Pacific Islands, the absence of all the higher mammals is so marked as to distinguish these countries from every other part of the world. When the land-surface is so completely broken up into islands we cannot expect to find any of the more prominent geographical features which characterise large continents. There are no great lakes, rivers, or mountain ranges. The only land-area capable of supporting a great river is exceptionally arid, yet the Murray of Eastern Australia will rank with the largest European rivers, its basin having an area about equal to that of the Dnieper. Mountains are numerous, and are much higher in the islands than in Australia itself. In such remote localities as Sumatra, Borneo, the Sandwich Islands, and New Zealand, there are mountains which just fall short of 14,000 feet. In New Guinea they probably exceed this altitude, if, as reported, the central range situated close to the equator is snow-covered; while in Australia itself the most elevated point is little more than half as high.

In studying the natural history of Australasia an accurate knowledge of the depths of the waters intervening between the several islands is of considerable importance, and Dr. Wallace provides some details which show that the depth of the seas is greatest near the larger land-masses in this region as in most others. For instance, close to the New Hebrides the soundings record 16,900 feet; between Sydney and New Zealand, 15,600 feet; and a little to the south-east of New Guinea, 14,700 feet. There is a comparatively shallow sea round the coasts of Australia itself, which gradually deepens, till at some 300 to 500 miles on the east, south, and west, the depth of 15,000 feet is attained. The sea connecting Australia with New Guinea and the Moluccas is rather shallow, with intervening basins of great depth. In the Banda Sea the line records 12,000 feet, in the Celebes and Sooloo Seas over 15,000 feet, and in the China Sea, west of Luzon, is a depth of 12,600 feet. Further westward the sea shallows abruptly, so that Borneo, Java, and Sumatra are connected with each other, and with the Malay and Siamese peninsulas, by a submarine bank rarely exceeding 200 or 300 feet in depth.

There are some five or six races of mankind in the area: Malays, Papuans, Australians, Polynesians, Tasmanians (now extinct), and the Negritos. With regard to some of these Polynesians, Dr. Wallace remarks that those inhabiting Samoa and the Marquesas are in no respects inferior to the average European, either in their complexion, physical beauty, or nobility of expression; and he laments that these higher tribes are all disappearing under the fatal contact of our much-vaunted civilisation. To this we venture to add our own regrets on the inevitable extermination of the fine race of Maories in New Zealand.

In the Zoology and Botany, Australia is characterised by possessing a number of peculiar forms, as well as by the absence of many which are common in almost every other part of the globe. The mammalia are almost exclusively marsupial, the only other representatives of the group now living being the opossums of America. Of the birds, there are no familiar types, as vultures, pheasants, and woodpeckers, but such peculiar forms as honeysuckers, paradise birds, lyre birds, and cassowaries. The snakes and lizards, though numerous, are also singular.

In the plants the same peculiarities are to be seen. The Malayan flora is a special development of that which prevails from the Himalaya to the Malay Peninsula and South China. Further east, this flora intermingles with that of Australia and Polynesia. The Australian flora is highly peculiar, and very rich in species; while that of New Zealand is poor but very isolated. Special sketches of both faunas and floras are given in connection with each colony.

The geological relations of this portion of the world are undoubtedly with Asia. Dr. Wallace thinks that the exceedingly shallow seas connecting the islands of the northern area show that not only Java and Borneo, but even the Philippines formed a south-eastern extension of the Asiatic Continent in a comparatively recent period. The vegetable and animal life shows this still more clearly. But between this mass and that of New Guinea and Australia, we pass over deep seas and find ourselves among a set of animals for the most part totally unlike those of the Asiatic Continent, or any other part of the globe. The resemblances of these southern life-forms to the fauna of Europe during the Secondary period are very striking, and lead Dr. Wallace to say that it is generally believed that the countries they now inhabit have been almost completely isolated from other land-masses since the Oolitic period. The evidence derived from a study of New Guinea, the Moluccas, and the islands as far as Lombok in the north, and Tasmania in the south, goes to prove that Australia was formerly more extensive than it is at present. That this is clear as regards its eastern seaboard is proved by the Great Barrier Reef, whose coral walls still indicate the former limits of the coast-line in this direction. On the same coast, but further removed from the mainland, are some scattered islands, conspicuous among which is New Caledonia, and this is slowly sinking.

Australia must, therefore, says Dr. Wallace, be regarded as an ancient continent of the Secondary or early Tertiary period now gradually diminishing, and this phenomenon of subsidence is displayed in New Caledonia and in some other islands of the South Pacific Ocean. Dr. Wallace is careful to point out that it is now recognised that coral islands do not prove subsidence wherever they occur, but may, and frequently do, indicate areas of elevation at a recent period.

Passing now to the special chapters of the book, we find the present volume confined to two great divisions, (1) Australia, including Tasmania, and (2) the New Zealand group. Malaysia, Melanesia, Polynesia, and Micronesia will form the subject of Dr. Wallace's second volume.

Australia itself, as a whole, is first dealt with, subsequent chapters treating in turn of New South Wales, Victoria, South Australia, West Australia, Queensland, and Tasmania.

With regard to Australia as a whole, Dr. Wallace calls attention to the simple conformation of the land-mass, which rises generally from the coasts into elevated uplands in the interior. But the assumption that Australia forms a vast table-land, with elevated borders sloping towards the interior, must be taken with considerable qualification. The Australian highlands form no connected whole, but are everywhere intersected by depressions, and a subsidence of 2,000 feet would convert the whole continent into a group of islands ; though this, of course, is true of most other continents. A further peculiarity of these uplands is their distribution mainly along the coast, and skirting the interior where no extensive mountain-ranges have hitherto been discovered. The forbidding and desolate plains of the interior of Central and Western Australia are described, and attention is called to their peculiar vegetation (the "Mallee scrub" and the "Mulga scrub") and the extraordinary deficiency of water.

The river-system of the eastern half of the continent is compared with those of the Old and New worlds, and notes are given as to the excessive irregularities of droughts and floods. In the central and western portions streams appear in the wet seasons, but after a short time disappear in the sands; hence, while one explorer has found a total absence of water and herbaceous vegetation, another, arriving in the same region after one of the rare rainy periods, has been delighted with the running streams, luxuriant herbage, and abundance of animal life.

The climate is much less variable than might be supposed. It may be described as hot and dry, and on the whole healthy. The mean temperature at Melbourne is 58° Fahr.; at Sydney it is about 63° ; at Adelaide it is slightly higher, while at Perth it is about the same. The extremes of heat are reached when the hot winds blow from the interior, the highest point yet recorded being the bursting of Captain Sturt's thermometer at 127° in the shade. The same traveller found a mean temperature of 100° Fahr. for three months on one occasion. The rainfall is heaviest at Sydney (50 inches), diminishing as one goes inland, while in the south at Melbourne it is 25 inches, and in Western Australia about 30 inches.

The chapters on the Botany, Zoology, and Geology, as one might expect from Dr. Wallace, are full and interesting. The geological map presents a mass of detail, and is generally effective, but one would like to have seen a more modern and precise word than "Trap" used, and the "Crystalline or Metamorphic" rocks not put down as Primary or Palæozoic. We fancy also that it will be news for some geologists to find Oligocene classed as Lower Tertiary, and Volcanic rocks under the head "Igneous or Plutonic."

Each division of Australia is treated in similar detailed fashion, but want of space forbids us from giving a general sketch of each.

New Zealand occupies the last 70 pages of the book, and receives a careful description, similar to that given to Australia. Dr. Wallace, however, is not quite correct in saying that the secluded and romantic waters of lake Taupo are furrowed only by the canoes of the natives, for there is an English settlement there, and we believe the ardent sportsman may now be conveyed by the more expeditious, if less picturesque, steam launch. We also rather doubt the magnificent spectacle of six geysers playing at once, as shown in the somewhat wooden cut of a view on the river Waikato (p. 418). A little contempt, too, seems to be bestowed on the great extinct birds in the sentence— No birds or reptiles have been found except such as are allied to forms still living on the island. But Dr. Wallace refers more kindly to these important relics of the past on p. 446, and our readers have been kept well up-to-date as to recent discoveries and observations on moas in the pages of this Journal.

Taken as a whole, the book is full of interest, is generally well illustrated, both by woodcuts and maps, and will prove an exceedingly valuable book of reference. Some few slips are found here and there, and some omissions of information to be found in the most recent publications; but Dr. Wallace may well be excused, for in the compilation of books such as these much time is occupied, and no one regrets more than the authors the impossibility of putting in more facts.

THE ZAMBESI BASIN AND NYASSALAND. By Dan J. Rankin. 8vo. Pp. viii., 277, with 3 maps and 10 full-page illustrations. London: William Blackwood and Sons, 1893.

WE must confess to considerable disappointment with this book, for we had expected much and have got but little. The author is known to be a man of considerable literary ability and a first-rate Arabic scholar. He has spent much time in East Africa, and knows the district and its natives well, while his work in connection with the opening of the mouth of the Chindi to trade has had an important influence on the commercial prospects of the whole of the Zambesi basin. Nevertheless, there is very little of permanent value in the book. The flora of the district is barely mentioned; the only information about the fauna consists of a few accounts of hippopotamus hunting, and the inevitable, but amusing, crocodile tragedies, which play in Africa the part of the "grizzly" in the stories of our imaginative Western cousins. This is the more unfortunate as the author seems to have met with some very queer beasts, such as the intrusive scorpions, which were so numerous in a steamer's saloon that they had to be dusted off; then there are the worms, which seem to thrive in the iron plates of the same steamer, and a brittle hippopotamus, both sides of whose head were smashed away by a blow on the jaw from only a 12-bore bullet. The book includes more than its title would lead one to expect, as one chapter is devoted to Mombasa, which is certainly far beyond the limits of either the Zambesi Basin and Nyassaland; the remarks on Mombasa, moreover, are disappointing, and as there is no fresh information in the chapter, we must conclude that the author's researches into the history of the early settlements of the Arabs have not been rewarded by new discoveries. This is much to be regretted, considering the anthropological importance of the subject. The author also gives a sketch of the scheme and objects of the International Flotilla Company, which hopes to establish communication from the mouth of the Zambesi to Cairo by the Nile, the great lakes, and the Shiré.

The book is extremely well printed and got up, many of the stories are amusing, and we cannot resist quoting one which illustrates the cuteness of mission companies when they do take to business. The author and two other Europeans once, at the extreme peril of their lives, saved a mission station by running the gauntlet down the Shiré, capturing and repairing the mission steamer which had been seized by the natives, and then working it back to Katunga. Apparently the only thanks they received for this plucky performance was *a bill for their passage*.

ELEMENTARY PALÆONTOLOGY. By Henry Woods, B.A., F.G.S. Crown 8vo. Pp. viii., 222. Cambridge Natural Science Manuals. Biological Science. General Editor, A. E. Shipley, M.A. Cambridge: C. J. Clay & Sons, 1893. Price 6s.

OUR chief quarrel with this little book is with its title. If, instead of naming it "Elementary Palæontology," the author had called it a

Rule of Thumb Guide to Invertebrate Zoology for the use of Examination Candidates in Geology, we should have had nothing to say against it. All that those gentlemen want is to be able to spot their fossils with a minimum of trouble and a maximum [?] of certainty; they reck naught of morphological niceties so long as they can "pip the examiner." To this goal Mr. Woods will prove an excellent guide, and that his goal is no nobler one is probably deplored as much by himself as it is by us. As to our own ideal, the curious—if such there be—will find it in NATURAL SCIENCE, vol. ii., p. 307.

We would willingly leave the book here, but as it is the first of a series issued under the auspices of no less a body than Cambridge University, it is only due to the Editor and his co-labourers that we should treat it with some seriousness. We hope, in the first place, that the other writers of elementary manuals will not use quite so many long words, at all events, without explaining them. Here the terms "isotropic silica" (p. 3), "optically biaxial" (p. 4), "siphonostomatous" (p. 10), "phylogeny" (p. 11), would make the ordinary undergraduate throw the book out of the window before he had finished the introduction. To judge from his definition of the Foraminifera, Mr. Woods has never heard of the student who was brought to a stand by a similar one. When his teacher said, "I suppose you don't know the meaning of *arenaceous*," he proudly replied, "Oh! yes. I have heard of arenaceous *foods*."

There is, as we have implied, far too much Zoology for a book that calls itself a "Palæontology." Under no circumstances can a palæontologist as such, much less a geologist, want to know anything about the nervous system of the Bryozoa or the renal organs of the Mollusca. The compilers of Zoological text-books have a right to prosecute Mr. Woods for poaching on their preserves.

The absence of finished pictures of specimens is a commendable feature, for such only help the lazy reader. Other writers in the series will do well to follow Mr. Woods' example, but it is to be hoped they will steer clear of his occasional habit of using a plural nominative with a single verb, *e.g.*, "The Porifera includes the Sponges." It would also have been an improvement had the name of some typical species, not necessarily the type species, been added to the descriptions of the genera. In the present state of Brachiopod nomenclature, for instance, it will take the student a few hours to determine what the author means by *Strophomena* and *Leptæna* respectively.

It is a pleasure to find the Pteropods at last removed from the Cephalopods to the Gastropods; but the account of the Cephalopods is no great advance on that of previous text-books. It is all very well for a teacher to be conservative; but there does come a time when one must either "mend or end." "The species of *Ammonites* . . . . differ so much from one another that they are now regarded as representing many distinct genera. . . . . But in an elementary work like the present it will be convenient to retain the old genus *Ammonites*." Long may the good old genus *Ammonites* remain—*in our text-books*! Seriously, what would Mr. Woods or anyone else think, if a writer were to substitute the word *Encrinus* for *Ammonites* in the above quotation? Anyhow, it is worse than absurd, after disposing of the various Families, not merely Genera, of Ammonitinæ in this manner, to devote a whole page to descriptions of the uncoiled Cretaceous forms, which are not genera at all.

Ammonites is described as "possessing two pairs of gills, two pairs of auricles, and two pairs of kidneys," while other details of its anatomy are given. Of course, if this is the case, the author is perfectly justified in retaining it in an Order Tetrabranchiata, although in opposition to the views of recent writers. It is, however, to be hoped that the specimen from which these details have been determined will soon be adequately figured and exhibited before some learned society by preference, the Royal.

On the whole, the definitions and descriptions are fairly accurate and moderately clear, while the bibliography at the end should prove useful to English students ; but is it not stretching a point to describe the calyx of *Apiocrinus* as "pear-shaped," when its shape really resembles that of a piece used in the game of draughts? Still, if there are slips of this kind, and we must admit that we have counted not a few, it is only because the author has attempted an impossible task. It is far more difficult to compile an accurate and intelligible elementary manual than it is to write an elaborate original monograph. Certainly, no one who has not done the latter should ever be encouraged to attempt the former.

F. A. B.

Text-Book of Geology. By Sir Archibald Geikie, D.Sc., LL.D., F.R.S., etc. Third edition. Pp. 1,147. London: Macmillan & Co., 1893. Price 28s.

Eight years have elapsed since the second edition of this book was published, and the work accomplished during the interval has led to an increase of 155 pages in the volume. Nevertheless, its actual bulk is not perceptibly increased—the paper is excellent, and the illustrations, of which there are thirty-two more than in the last edition, are admirably printed. The author maintains the moderate uniformitarian doctrines which he has hitherto professed, remarking "that the few centuries, wherein man has been observing nature, form much too brief an interval by which to measure the intensity of geological action in all past time."

Throughout, the work has been revised where needful, and additional references are given to the latest sources of detailed information. On all matters relating to rocks and rock-structure, to the work of modern agents, and the appearances of the rocks in the field, the definitions and descriptions are most lucid. In the matter of palæontological succession and of the value of organic remains in correlating strata at a distance, the views of the author are not in harmony with those of advanced zonalists. If "strict contemporaneity cannot be asserted of any strata merely on the ground of similarity or identity of fossils," yet practical contemporaneity may in many cases be justly maintained, especially when a similar sequence of "zones" is found to characterise areas widely separated. Zonal subdivision and attempts at very minute correlation may, however, be carried to an extreme degree; and further research may justify the cautious attitude of the author, who, in comparing the rocks of distant countries, remarks, "all that we can safely affirm regarding them is, that those containing the same or a representative assemblage of marine organic remains belong to the same epoch in the history of biological progress in each area."

In the portions of the book relating to Stratigraphical Geology, large additions are made to the sections dealing with the pre-Cambrian and older Palæozoic rocks, and other sections are duly brought up to date by the insertion of new facts and further references. The book, however, is so well known, that we need say nothing more, except to congratulate the author on the completion of his task—one which, considering its comprehensive character, must indeed have proved an immense labour; but which, for this very reason, is a signal service to Science.

Tables for the Determination of the Rock-forming Minerals. Compiled by Professor F. Lœwinson-Lessing, translated from the Russian by J. W. Gregory, B.Sc., F.G.S., with a chapter on the Petrological Microscope by Professor Grenville A. J. Cole, M.R.I.A., F.G.S. London: Macmillan & Co., 1893. Price 4s. 6d. net.

Many investigators who have not had the chance of proper training in a petrographical laboratory, and have had to gain their acquaintance with the methods by books, must have felt their lot a very hard one. Even when they have become fairly well practised in the branch of the microscopic mineralogy of rocks, the determinations in many cases are long and tedious, as classified diagnostic tables were still wanting. So far we had only the systematic description of minerals as they occurred in rocks, but they were classified according to their chemistry, and not by their microscopic characters. The above work admirably fills that gap, and now, after a few lessons in microscopical tactics, a student with a fair knowledge of mineralogy can start as a rock describer.

The original work appeared in Russian, a language to all intents and purposes beyond the knowledge of European and American scientists, and Dr. J. W. Gregory has made all petrographers his debtors for translating Professor Lœwinson-Lessing's book. Dr. Gregory had intended to add a chapter on the petrological microscope, but one of his multifarious duties called him to African exploration, so that his partially-finished chapter was placed in the able hands of Professor Grenville A. J. Cole, who has acted as the contributor of it to this joint work.

In the introduction we have a general outline of the methods to be employed. In the next chapter the main requisites of a petrographical microscope and their uses are described. In part II. of the book we have the tables. Minerals are divided for investigation into opaque, semi-opaque, and transparent, and under each the optical, chemical, and other tests are given. The transparent are divided into colourless and coloured. These groups are subdivided again, according to whether the mineral shows crystal outlines, crystalline grains, or grains in crystalline rocks. Table III. gives a classification of other morphological characters, such as tabular crystals, needles, broad tablets, flakes, filaments, or aggregates, while the presence of colour is again used to group the minerals. In table IV. the determination of the crystalline system under the microscope is treated of. This is followed in table V. by a classification of the rock-forming minerals according to their crystalline system. The book terminates by a table of minerals according to their optical signs.

Only one or two questionable statements attracted our attention, one being, "It is convenient to leave leucite in the prismatic system, which it resembles in form; it appears, however, to be really a monoclinic mineral which has acquired prismatic form by compound mimetic twinning." Most of us hoped that the interminable literature and ingenious contrivances of transcendental crystallographers to explain the optical anomalies of leucite were once for all finished, and that simple temperature strain in an isometric mineral had been accepted as the cause.

In fine, we can only say of the book to all students of petrography, "get it!"

H. J. Johnston-Lavis.

The Journal of Marine Zoology and Microscopy. Vol. i., no. 1. November 1893 [Quarterly]. 8vo. Jersey and London. Price 2s. per annum, post free, or with 14 Microscopical Studies in Marine Zoology, 21s.

It is unnecessary to call attention to the beautiful microscopical preparations sent out by the Jersey Biological Station, they speak for themselves; but it will be interesting to many to learn of a new publication, the chief feature of which will be the illustration and explanation of a yearly series of Messrs. Sinel and Hornell's slides. With this first number are sent out three preparations: *Lucernaria*; *Tomopteris*, the pelagic Annelid; and *Salpa mucronata-democratica*. Two plates accompany the descriptions, and though somewhat rough in execution, are amply sufficient for the purpose intended. Mr. Hornell also describes an albino lobster, 14 inches long, taken in St. Aubin's Bay. A list of other albino organisms is given, and a note follows on the colouration of sponges. Some observations on the Octopus are also added, notably information concerning the strength of this mollusc, its food, and the cleansing process of skin-casting from the suckers. The journal consists of 24 pages, the first 14 being devoted to the special notes just enumerated.

The Out-Door World; or, the Young Collector's Handbook. By W. Furneaux, F.R.G.S. 8vo. Pp. 411, 18 plates, and 549 figures. London: Longmans & Co., 1893. Price 7s. 6d.

Popular books on natural history are so seldom noteworthy for accuracy, that we are glad to draw attention to one that is distinctly above the average. Mr. Furneaux's second title explains the plan adopted, and also accounts for the disproportionate amount of space devoted to certain groups. The volume is divided into three sections: animal life, the vegetable world, and the mineral world; but three hundred pages are devoted to the first section, which is also much the most satisfactory. Butterflies, moths, and birds' eggs are especially well illustrated, the coloured plates being particularly good for so cheap a volume. Certain of the other groups are not so well done, many of the mollusca, for instance, being scarcely recognisable unless one already knows the species. In a second edition, the botanical portion should be revised. The plan has evidently been altered while the proofs were being corrected, for a chapter on grasses is misplaced, and is illustrated by plates out of the numerical order. We would suggest also that in future editions it would be well to draw attention to the importance of noting the exact date, locality, and conditions under which each specimen was obtained. The recording of these facts is an excellent training for the boy, besides adding greatly to the value of his collection.

We learn that the first part of Mr. C. Davies Sherborn's "Index to the Genera and Species of the Foraminifera" will appear about the end of this year. This index, which is brought down to December, 1889, and which is being published by the Smithsonian Institution, occupied the author over four years, and went to press early in 1890. The second and concluding portion may be expected in 1895.

OBITUARY.

HERMANN AUGUST HAGEN.

BORN MAY 30, 1817. DIED NOVEMBER 11, 1893.

SO long as the study of Insects is continued, so long will the memory of Professor H. A. Hagen be venerated. The author of "Bibliotheca Entomologica; Die Litterature über das ganze Gebiet der Entomologie bis zum Jahre 1862," which appeared in 1862–1863, has done so much to lighten the labours of his fellows, that he will always be remembered with gratitude. Hagen was born in Königsberg in 1817, and practised there, as a Doctor of Medicine, until 1867, when he left Germany for America and became assistant to Louis Agassiz. In 1870 he was appointed to the chair of Entomology at Harvard, a professorship he held till his death. Hagen, though specially interested in the Literature of Entomology, found time to write several hundred papers on Insects themselves, among which may be mentioned a Catalogue of *Termitina* in the British Museum, 1858.

AUGUST KARL EDUARD BALDAMUS.

BORN APRIL 18, 1812. DIED OCTOBER 30, 1893.

THIS well-known ornithologist was born at Giersleben, in Saxony. He studied theology at Berlin, and in 1859 was elected Professor at the Köthen Gymnasium. Here he became acquainted with the brothers Naumann, and profited from their ornithological researches. He was pastor at Dielzig in 1849, and passed, in 1858, to the same office at Osternienburg. After his retirement, in 1870, he lived at Coburg. Baldamus collaborated with Blasius in bringing out Naumann's "Naturgeschichte der Vögel Deutschlands," and wrote "Illustriertes Handbuch der Federviehzucht" (ed. 2, 1881), and "Vogelmärchen," 1882. To his initiative was due the foundation of the German Ornithological Society; while from 1849–1858 he published the journal called *Naumannia*.

GEORGE BENNETT.

BORN, 1803. DIED, OCTOBER 1, 1893.

BORN at Plymouth ninety years ago, George Bennett was educated for the medical profession. Visiting Sydney in 1830, he decided to practise there, but did not finally settle until six years later. A lifelong friend of Richard Owen, Bennett became an enthusiastic naturalist, and provided his friend with rich and valuable material. The first to find Nautilus in the living state, he forwarded his prize to Owen, who wrote upon it his celebrated "Memoir on the Pearly Nautilus," 1832. Bennett wrote numerous papers on Natural History, the most important perhaps being his observation on *Ornithorhynchus* (*Proc. Zool. Soc.*, 1859); he also published "Wanderings in New South Wales," 1834; and "Gatherings of a Naturalist," 1860. On the foundation of the Australian Museum, Bennett was chosen as first Secretary, and the growth and importance of that Institution is largely due to the energy and industry of this eminent naturalist.

NEWS has been received of the death of Dr. Dionys Stur, the eminent geologist and palæobotanist, who passed away at Vienna on October 9. For more than forty years he was connected with the Imperial Geological Survey of Austria, and in 1885 became Director. He retired from the service in April of last year. In 1890 Dr. Stur received the Cothenius Medal from the Leopold Caroline Academy, and was elected a Foreign Member of the Geological Society of London.

WE also have to record the death, on November 13, of Mr. William Dinning, the well-known Honorary Secretary of the Natural History Society of Northumberland and Durham. He had attained the age of 57, and had been in failing health for some time. Mr. Dinning's chief scientific occupation, pursued in whatever leisure he could obtain in the midst of absorbing business-engagements, was the collection and patient preparation of the vertebrate fossils of the local Carboniferous and Permian formations. He accomplished much in association with the late Messrs. T. P. Barkas and Thomas Atthey, and several of his beautiful drawings illustrate the papers of Messrs. Hancock, Atthey, and Embleton in the *Annals of Natural History* and the Northumberland *Transactions*.

IN our October number (p. 308) we gave a short notice of the late Edward Charlesworth. We were unable at the time to ascertain the dates of his birth and death. We learn (from the *Geological Magazine*) that he was born September 5, 1813, and died July 28, 1893.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Dr. Karl von Dalla Torre has been appointed Extraordinary Professor of Botany at the University of Innsbruck.

Dr. A. Grob has been appointed assistant in the Plant Physiological Institute of the Polytechnic at Zurich.

Dr. Achille Terracciano has retired from his position as Conservator of the Royal Botanical Institute at Rome, and Dr. Oswald Kruch has taken his place.

Professor P. Soraner resigned, on October 1st, the superintendence of the plant physiological research station at Proskan, and Dr. Rudolf Aderhold, formerly first assistant in the similar institute at Geisenheim, has been appointed in his stead, while Dr. F. Krüger takes Dr. Aderhold's place.

The Professorship of Geology in the Bohemian University of Prague has been filled by the election of Dr. Woldrich, of Vienna.

Professor Charles Stewart, of the Royal College of Surgeons, has been elected Fullerian Professor of Physiology at the Royal Institution of Great Britain.

The Owen Memorial Committee has entrusted the execution of the statue of the late Sir Richard Owen to Mr. T. Brock, R.A.

Dr. T. Pleske has succeeded the late Dr. Strauch as Director of the Natural History Museum of the Imperial Academy of Sciences, St. Petersburg.

It is proposed to establish a small Biological Station at Millport, for the study of the Marine Zoology of the Firth of Clyde and West of Scotland generally. A temporary station has already existed for some years at the same place, and during 1891–92 the Government Grants' Committee of the Royal Society of London provided £100 for the investigation of the Clyde Sea area. Much valuable work can be done in the district, and contributions to the building-fund of the station are earnestly solicited by the Committee, who have just issued a circular on the subject. The treasurer of the fund is Robert Gourlay, Esq., Bank of Scotland.

On November 8, the Mayor of Carlisle opened a new Institute of Science, Art, and Literature, built by the town corporation at a cost of £20,000. The Museum contains a large collection of local rocks and fossils, antiquities, and birds, besides other natural history specimens.

The Year-Book of the Bergen Museum for 1892 has just come to hand. In it Dr. J. Brunchorst gives, in German, an account of the biological station in Bergen, and of the machinery by which the aquaria are supplied with air and water. In addition to the official reports of the curators, the volume contains papers by A. Appellöf (on "Cephalopoda of the North Sea"), O. Nordgaard (on "Polyzoa, Echinodermata and Hydroids of Beitstad Fjord"), J. A. Grieg (on "Ophiurids from Greenland"), and J. Brunchorst ("Some Illnesses of Norwegian Timber"), as well as two archæological papers.

Better days are dawning for the South African Museum at Cape Town, which is at present "cribbed, cabined and confined" in one wing of the handsome Public Library building. A new Museum is to be erected in the Government Gardens, at a cost of £20,000, and the present rooms handed over to the Library. Let us hope that the working expenses of the Museum will be provided for in a like liberal manner.

In Hong Kong there is a Museum rather of the Curiosity Shop order; but an energetic literary and scientific local society, known as The Odd Volumes, are taking the matter up. They have had a deputation to the Governor, Sir William Robinson, and a lecture from an official of the British Museum. The Governor is willing, but the Press are not. The old objection is in the way—want of money. We can but wish The Odd Volumes the success they deserve.

We learn from the *American Naturalist* that some public-spirited citizens of Chicago have formed a corporation for the purpose of creating and sustaining a Museum, which shall furnish to the public of the city an educational exhibition. It is proposed that the Museum shall be located near to Jackson Park and the University, and for the present the California Building of the World's Fair is to be utilised. Professor F. W. Putnam, the distinguished archæologist, has been appointed managing director. The idea is that Professor Putnam will organise the Museum into departments, placing over each a competent head, who will make the institution a medium for original research, as well as for exhibition, as is the case with all the best museums of the world. It will thus become useful, not only to the general public, but to the University and to the Academy of Sciences. This body of scientific experts, connected with the Museum and University, should stir up the Chicago Academy of Sciences, which has laid dormant so many years.

A Royal Medal was awarded to Professor Harry Marshall Ward at the Annual Meeting of the Royal Society of London on November 30, in recognition of his researches on the life-history of fungi and schizomycetes. The Copley Medal is this year awarded to the distinguished physicist, Sir George Gabriel Stokes, Bart. The only change in the list of officers of the Society consists in the succession of Sir Joseph Lister to the Foreign Secretaryship, vacated by Sir Archibald Geikie. The new members of Council representing Natural Science are Professor A. H. Green, Sir John Kirk, Sir John Lubbock, and Professor Burdon Sanderson.

The Zoological Society of London have printed, in their last issue of *Proceedings*, a valuable and important statement of the exact date of issue of their octavo publications. The dates of their quarto publications have long been listed and printed on the covers of the *Transactions*. The information, which shows the number of the issue, the pages contained in each issue, and the date of delivery from the printers, has been supplied to the Society by Messrs. Taylor and Francis, the Society's printers, and will be of assistance in settling vexed questions of priority. We have often wondered why Societies are so shy of printing the date of issue of their publications side by side with the signatures. The example has been set in certain separate publications for many years, *e.g.*, Bronn's "Nomenclator Palæontologicus," and Godman and Salvin's "Biologia Centrali Americana," where the dates are inserted with painstaking accuracy. Of course, this inserted date must be that of issue, not that of writing the MS., for the latter is an imposition which cannot be too strongly condemned.

THE Cotteswold Naturalists' Field Club have recently published the first part of vol. xi. of their *Proceedings*. An address by the retiring President, Mr. W. C. Lucy, contains some notes on a large boulder found imbedded in quartzose sand on the top of Cleeve Cloud; the force that transported the boulder and sand have also considerably disturbed the Oolitic rocks of the hill. This number also contains an important paper by Mr. R. Etheridge, F.R.S., on the rivers of the Cotteswold Hills within the watershed of the Thames, which discusses their importance as a supply to the main river and to the metropolis. Accompanying the paper is a Hydro-geological map of the Thames Basin above Wallingford and Oxford. The author discusses the practicability of forming reservoirs at various places in the upper basin of the Thames. The number concludes with an account by Professor J. Allen Harker of the experiments that have led to the conclusion that leguminous plants can obtain a supply of nitrogen from the atmosphere by the aid of the symbiotic organisms that form the tubercules on their roots.

A SOCIETY known throughout the country by the results of practical investigations in which it has been so long engaged, the Yorkshire Naturalists' Union, with a total numerical strength of almost 3,000, may be said to occupy a premier position among county scientific institutions. Probably there has never been a gathering more thoroughly representative of the scientific activity of Yorkshire than that which took place in the Town Hall, Skipton, on the 14th ult., the occasion of the Union's thirty-second annual meeting.

The usual sectional meetings were held during the day at the Grammar School, and at these reports of the year's work were received, and officers elected for the ensuing twelve months. Each branch of Natural Science has a president and secretaries, whose duties are to control and direct the work of their own department, and prepare the results for publication in the *Transactions*, which are issued to members and contain a permanent record of all observations.

The business affairs of the Union are in the hands of a permanent General Committee, which met in the afternoon under the presidency of Dr. H. Clifton Sorby, LL.D., F.R.S. Seven committees of research, in connection with the British Association, were again appointed, and amid applause, Mr. W. Denison Roebuck, F.L.S., who has served the Union faithfully for many years, and to whom in main it owes its success, was, for the eighteenth time, elected hon. secretary.

In the evening an exhibition and conversazione, arranged by the Craven Naturalists' Society in honour of the visit of the Union, was held in the Town Hall, and at 7 p.m., when Mr. Henry Seebohm came forward to deliver his presidential address, an audience had assembled which quite overfilled the spacious building. In his opening remarks Mr. Seebohm, who is practically a native of Skipton, said how deeply he felt the honour which his position as President for 1893 gave him. To use his own words—"Little did I think, when a lad catching butterflies and tramping o'er hill and dale for what information I could get of the natural history of the neighbourhood of Skipton, that I should one day occupy the position I do to-night among Yorkshire naturalists." After thanking the Union for the honour they had done him, he proceeded to his address on "The Geographical Distribution of British Birds," of which a printed copy was presented to each member.

On the motion of Dr. Sorby, a vote of thanks was accorded to the President, and the Union is now able to add one more to the long list of distinguished men who have honoured it by their tenure of office.

It was announced that the Presidency for 1894 had been offered to, and accepted by, Mr. R. H. Tiddeman, M.A., F.G.S., of the Geological Survey, and the next annual meeting is to be held at Doncaster.

CORRESPONDENCE.

Emendations of Nomenclature.

In your last number (vol. iii., p. 328) you are severe on those who emend generic or specific names to make them agree with well-known rules of composition and orthography. Certainly anyone would agree with you that "the practice of emending names the etymology of which is not clear to the emendator's mind is a reprehensible one"; but when the etymology is clear, not only to the mind of the emendator, but to all those who have considered the origin of the term, and even to the author himself, is one then to be blamed for spelling a word in such a way that it shall convey to others the original intention of the author? Is an author's ignorance or carelessness to be eternally perpetuated to his own disgrace and to the perplexity of those who vainly try to discover the meaning of his names? Or is scientific nomenclature on a par with nonsense rhymes and the game of Russian scandal, where the essence of the joke depends on its absurdity? Of course, it is not often that a scientist is so unlearned in everything save his own subject as to describe a fossil as made of ivory *(eboreus)*, when he means to convey the really useful information that it occurs near York *(eboracensis)*; but it is unfortunately more often the case that scientists, like other people of humbler pretensions, do drop their "h" s. But since we do not all talk about Mrs. Enry Awkins, is there any reason why we should all talk about *Eleocharis* when we mean *Heleocharis*? Certainly your remarks are hardly consistent with the British Association rules of Zoological Nomenclature, of which number 14 is, "In writing zoological names the rules of Latin orthography must be adhered to." The eminent compilers of that code also say, "In the construction of compound Latin words, there are certain grammatical rules which have been known and acted on for two thousand years, and which a naturalist is bound to acquaint himself with before he tries his skill in coining zoological terms." True that, in the particular instance, you are dealing with Botanical names; but the rules of common-sense should apply equally to both Zoology and Botany. There is nothing that sets the scholar against the natural sciences more than this apparent ignorance of rules known even to me, your old friend,

Macaulay's Schoolboy.

[Our youthful friend forgets that scientific nomenclature is for the use of scientific men, not for those students of Latin and Greek to whom he arrogates the title "scholar." In the opinion of those who deal much with names, universality is at least as important a factor of intelligibility as etymological correctness.—Ed.]

TO CORRESPONDENTS.

All communications for the Editor *to be addressed to the* Editorial Offices, 5 John Street, Bedford Row, London, W.C.

All communications for the Publishers *to be addressed to* Macmillan & Co., 29 *Bedford Street, Strand, London, W.C.*

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CONTENTS.

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CONTENTS.

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CONTENTS.

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CONTENTS.

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